An Interview with

STEPHEN LUKASIK

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Conducted by Judy O'Neill

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Abstract

Lukasik discusses his tenure at the Advanced Research Projects Agency (ARPA), later the Defense Advanced Research Projects Agency (DARPA). The interview focuses on the Information Processing Techniques Office (IPTO) of ARPA. Topics include: the work of Eb Rechtin, the development of computer networks and the ARPANET, artificial intelligence research, the recruitment of IPTO directors, the effect of the Mansfield amendment-- which specified research should be relevant to the military --on IPTO and ARPA funding, the grant process, and the development of ILLIAC. Lukasik concludes the interview with a discussion of the name change from ARPA to DARPA.

* This is an updated version reflecting the interviewee's edits, entered on 25 May 2016.

LUKASIK: Have you read the Barber study of the history of ARPA?

O'NEILL: Yes, we have a copy of it.

LUKASIK: Good, this saves me a lot of effort because I commissioned that study.

O'NEILL: Oh, did you? I wondered who had commissioned it because it was right at the end of your time there.

LUKASIK: Well, actually I commissioned it, and I wasn't supposed to be in it but [the project] dragged on long enough after I had left, so they figured they'll pick up my period, too.

O'NEILL: Okay. Let's start with a few background questions. Why did you go to ARPA?

LUKASIK: I had, while I was at Stevens, come in contact with Bob Frosch. He was at Columbia, a place called the Hudson Laboratory, and I had gotten to know Bob. Bob then went to -- let's see -- for this discussion, let me call it ARPA, because we're talking about a stage when it was called ARPA. He had shifted to ARPA, to the Nuclear Test Detection Office, and I had some occasion to visit him in ARPA, and he asked me if I would join ARPA. He was in the process of being promoted from that office to Deputy Director of ARPA.

O'NEILL: So, he was leaving to go into the Deputy Director spot and asked you to come and head the Nuclear Detection Office.

LUKASIK: Right, and it looked like an interesting job, and I had been at Stevens for eight or nine years. So, it was an opportunity to do a different kind of a thing, and so I did it.

O'NEILL: Okay. You were familiar with ARPA before you went there.

LUKASIK: Yes, I had done a little committee work for them in connection with another activity in the previous years. But it was just started in 1959, so we're talking about 1965 when I was recruited, so, yes, I had had a little contact with them.

O'NEILL: Okay. And then, after a couple of years at ARPA you became the Deputy Director.

LUKASIK: Frosch left to become Assistant Secretary of the Navy -- and this is an incredibly complicated story. I don't know if you want to take time on it, but -- Frosch became Assistant Secretary of the Navy and was replaced by Peter Franken as Deputy. Then Herzfeld, the Director who hired Franken, left. Franken became Acting Director, and they pulled me up [from] office director to be Acting Deputy Director. Eb Rechtin was eventually appointed as the Director and, by rights, Franken should have dropped back to Deputy and I should have dropped back to the nuclear office. Rechtin and Franken didn't get along at all, so Franken left. So I ended up staying -- being confirmed -- in the position that I had been acting because Rechtin and I got along well.

O'NEILL: Okay, that was leading into my next question, which is did you basically agree with what Rechtin was trying to do when he came into ARPA?

LUKASIK: Yes and no. Rechtin was brought in by Foster, who was the DDR&E then, and one of his major tasks was to rectify an important deficiency in ARPA, which was its inability -- a real or perceived inability -- to transfer the results of its work into the military services. And Foster said, "This has got to get fixed," and Rechtin came in and said, "I looked at it. You're right. It's gotta get fixed." And they were both right. So, to that extent, when we're talking about the matter of moving things out of ARPA into the services, that's actually right. Now, Rechtin came in with

another enthusiasm, which was counter-insurgency. This is now 1967. We're deep in the Viet Nam war, and there's a lot of good technology to transition into the field, driven by the requirements of the war, just as happened during Desert Storm. But, Eb Rechtin felt that counter-insurgency was researchable, that technology had an important role to play, and he was probably overly enthused about counter-insurgency. Consequently, we kind of divided up the operation and -- are you familiar with the notation 6.1?

O'NEILL: Yes.

LUKASIK: Okay. So, Eb tended to be less interested in the basic research activity, 6.1, and so Eb tended to focus on counter-insurgency and some of the other activities, and he handed over the 6.1 activity to me. So, it was a good way; we each sought out our own areas of interest. But I would say substantially Eb was right, with this little over-enthusiasm for counter-insurgency or for technical solutions to it.

O'NEILL: Okay. One of the things that we noticed in reading some of the Congressional hearings from when you became Director was a shift much more into how this research was going to be of use to the military. Is this something that you felt Congress needed to hear? Something that you believed in? Something that the Department of Defense, generally, was trying to push?

LUKASIK: This is exactly the Rechtin thing. I said I agreed with Rechtin on transfer. I really did.

O'NEILL: So, it was continuation of Rechtin's transfer policy?

LUKASIK: It was a continuation of the development of transfer policy. You know, I have sometimes said at times like this, Eb Rechtin taught me everything I know about technology transfer, or at least up until that time. And, I absolutely agreed with him. Over the last 20 years or so, my thinking has been confirmed. It's true in every organization I've worked in. You put money into research, but how do you change the product mix? How do you change the bottom line? How do you make things better in society? I mean whatever it is you're trying to do, it's got

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to get out of the research labs. And so, yes, I was not just saying things because Foster and Rechtin said I should, or Congress wanted to hear. As a matter of fact, I think if you're reading the Congressional Record, or anything I have written, let me say for the record that everything that you read there is true. I don't speak and write for consumption.

O'NEILL: Well, I guess I was thinking, not that it was false, but rather trying to understand the motivations for the change in emphasis, because it looks like it's changing then, although what you're telling me is that it's really Foster and Rechtin starting that process and your believing in it and continuing it.

LUKASIK: Yes, that's right. That's an accurate way to capture it. Remember that when ARPA was founded in 1959, it was told to do certain things. And so by 1966, you can look and see if it's done them, and some of the things it did very well, and some of the things it did less well. But there was this general feeling that there was just tremendous potential and capability and people and ideas, and it wasn't affecting the rest of the Defense Department; [the Department] was going on as it had been going. And so, there was a frustration on all of our parts that the Services weren't picking up these ideas. And, in this interview, I'll say "the Services," but obviously some things ought to get transferred to defense agencies. I'll just use "the Services" as, generically, the operating parts of the Defense Department.

O'NEILL: Okay. As far as your interaction with IPTO, I'm assuming that that started as you became Acting Deputy?

LUKASIK: Right.

O'NEILL: Did you have any background in computing or computers, or have any knowledge of that area coming into ARPA?

LUKASIK: Relatively little. Part of the relationship came about actually -- it seems like a dumb reason -- but, when I was in the Nuclear Test Detection Office, the office across the way was IPTO, so therefore those are the people that you ran into and talked to. They happened to be some of the smartest people in the Agency so, therefore, there was another attractiveness of talking to them, just because of their -- not only their individual ability, but -- the quality of their principal investigators. So, when you mixed in that circle, you were dealing with smart people. The other thing is nobody had much of a knowledge of computing in those days. The first series of digital computers grew out of World War II -- that's late '40s. I was a graduate student at MIT in the early '50s -- that's when Whirlwind was being built. And so in some sense the first -- it's hard to say what the first digital computer was -- but one of the early digital computers was when I was a graduate student. Then I went to Westinghouse and I used them to do reactor calculations. I went to Stevens and I learned FORTRAN, and I used them in my research. But, by 1966 there were relatively few computers. Remember, computer science departments were just starting in the 1960s, so it was just being recognized as something other than an incidental activity of electrical engineering. So, yes, I was in very early on with no formal training. I was a physicist, and of course remember also, in those days physicists thought they could do anything, from any kind of engineering, any kind of subject.

O'NEILL: And certainly you had more exposure with your background in physics to computing than other people would.

LUKASIK: Right. And I had a natural affinity and inclination. So, when I was handed the 6.1 portfolio, that had IPT in it, and remember that it was only later that I grew the 6.2 program elements, and that was both a way of transitioning stuff out of "research" as well as, quite frankly, creating new budget elements so the IPTO budget could grow, because, remember in those days the pressure was on to explain why anything is increasing. And so, if you could -- the way to increase something was not to increase it but to start something like it, and then that could grow and so you could multiply the size of an activity by the number of program elements you could successfully establish. So, to that extent, there was a little bit of tactics involved -- but, again, it was very important because 6.1 programs thought and worked differently from 6.2 programs, and so the way to get something to start to transition was to have a 6.2 program element.

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O'NEILL: Did you see that difference primarily in terms of where the research was done, the university element being in the 6.1, or what?

LUKASIK: Well, it is true that the 6.1 offices -- I'm now fuzzy on the numbers, but to allow me to make up some generic statistics, probably the 6.1 offices did about 80 percent of their contracting with universities. Maybe it was 70 percent, but in any event, some large amount, and some smaller amount was done with industry. And they hardly ever used think tanks, because think tanks are policy-oriented and operations-oriented. So, it is true that where the work was done was a large part of it, and it's a little bit unfair to expect a university professor to work his research into a weapons system. That's not his forte -- whereas, if you do it in 6.2, you have a more industrial kind of environment and you get think tanks figuring out how it could work into what, and you get industry taking it and working it into systems of some sort. So, yes, there was a certain amount of ease of transition involved.

O'NEILL: Now, you mentioned that when you were in the Nuclear Detection Office, the IPTO office was across the hall from you, or whatever, and so you were running into people going in and out of IPTO. Did any kind of connection with that whole IPTO community, in terms of both the contractors, the Program Directors and Program Managers, did that continue on?

LUKASIK: Yes. As a matter of fact, when I did have a couple of involvements -- when I became Deputy -- remember I was really only the head of the nuclear office for a matter of a year. And then I was double-hatted, Acting, and then I was Deputy Director, but I was still acting in the other job and I was phasing out of it, so in that period, yes, I was absorbed by the IPTO contractor community to some extent. For example, they used to have contractor meetings, and I attended several of them at various stages in my career, which was an opportunity to talk to the contractors directly. And, as a matter of fact, Ed Feigenbaum has me quoted in his recent book [*The Rise of the Expert Company*]. It's not his latest book, but it's one of his recent ones. Anyway, he quotes a vignette that I appeared at one of their meetings and I gave a healthy plug to artificial intelligence and said it was a long-term investment account, and how that was well received by the community. I guess by that time I was the Director of the Agency. Anyway, here's a higher-

level person in the agency saying, "You guys are doing great stuff, important stuff. I'm behind it," so, yes, I interacted with the community on various occasions.

O'NEILL: So you would say Feigenbaum's characterization of your interaction was accurate?

LUKASIK: Yes, it's in I think the introduction to his book. If you run across it . . . it's accurate.

O'NEILL: Yes, we've come across a couple of other comments by contractors saying that they felt that you were one of them. I was wondering if you felt that way, too?

LUKASIK: Oh, yes. In fact, most people around here see me as a computer type, not as a physicist. So, whenever I'm on a committee or whenever I'm working someplace, I always draw the chores that are related to computing, software, communications -- most of my peers probably view me as a computer type.

O'NEILL: I see. How involved did you get in the research that they were doing, to what level of detail? Was it technical, or was it just a matter of, "Yes, I support artificial intelligence"?

LUKASIK: No, it was a reasonably technical level. I'll give you two examples. One is the matter of the ARPANET. A little story about the ARPANET, by the way, before I tell you the example, because it's illustrative. I walked out of my office one evening, and Bob Taylor, who was the head of the office at that time, came out and said, "Steve, we just achieved a fantastic result. We finally got all of our principal investigators to agree that if we set up a computer network, that they will use it and do their research on it." This was at a time, remember, when each principal investigator was at a university. They each had their computer center. As a matter of fact, in those days, each computer center had its own operating system. We were paying for writing operating systems with BBN and University of Michigan, and MIT, and so it was really weird in terms of today's thinking. And so they finally agreed. And at that time each university, each principal investigator, each computer center was seen as an island, and they finally agreed that they would use it -- I'm sure IPT used a heavy club, saying "We're paying you guys. You'll do what

we want you to do." But, on the other hand, to an academic community, you don't give them orders. So, therefore, it was all very well to have a bureaucrat in ARPA saying thou shalt do something or other, and the university people just saying to heck with you, I'll go look for money elsewhere. So, it really was a managerial achievement by IPTO to get the principal investigators to agree. Okay, well then, so that's December of '66. And, so then finally the thing gets put together in '69 and '70. Anyway, by 1973, they put me on the ARPANET. It was now running -- it had about 20 sites or something, and so I personally went on the ARPANET. It was my introduction to electronic mail and networking, and therefore, as a user, you could understand the problems of the net. And, as a result, when you go to ARPA everyone has a PC or a workstation -- I mean, nobody has terminals anymore -- and they're all on Local Area Nets and there's probably a couple of Local Area Nets, and they're all connected networks. I was the first one outside of IPT to go on the ARPANET in ARPA, and therefore most of the office directors who needed a way to communicate with me realized it was through electronic mail, and so almost all the offices then got on the net, and then the Strategic Office understood its utility and the Tactical Office understood its utility and my old Nuclear Monitoring Office understood its utility, so the present -- of course, one can argue that even without me, everyone would be on networks because that's the way you work today but, in fact, I really worked on it. For example, on the first system, I was getting these whole collection of messages. And I said to Larry Roberts, who by that time was head of the office, "Larry, what do I do with all these messages? They just accumulate." And so he literally went home one night and wrote a little system called RD, which was read your mail, throw it away, put it in this category, put it in that -- the first beginnings of a mail system, as opposed to just sending telegrams -- up until that time, electronic mail was sending telegrams to people, and they threw them away. They read them, they were like buck slips.

O'NEILL: So, it was more like a broadcast message. You just got that message when you logged on.

LUKASIK: Right. And the guy did something with it or not. And so, it was my complaining to Larry Roberts that caused him to go off and write a little program using TECO that handled the mail; that read the headings and all that kind of stuff, and if you wanted to send a return message it read the heading of what you got and -- did all those little service things that now -- Okay, so that was one example.

O'NEILL: So that would have been '73, and you were able to start using that system right away then. He just

went off and wrote a program for you, basically, in order for you to deal with the mail problem.

LUKASIK: Right, and then I started using text editors, stuff like that. He subsequently complained that he probably made a mistake, that he put me on the net six months or so too early because I saw all of the flakiness. When it would collapse, I'd have a phone connection to what was then called a TIP. This thing that came after IMPs if the MITRE TIP was down, I'd be on the phone complaining to people.

O'NEILL: Sounds very visible.

LUKASIK: That's right. So they exposed me. On the other hand, it also meant that that gave it a good handle on fixing things, because the guy who's in charge of the money is unhappy. So, anyway, so I learned a lot and I possibly contributed a little bit. Now, the other example of involvement -- Well, in general, the AI stuff, I was always very interested in. The speech understanding program was at my instigation. Larry Roberts, you remember, got his PhD. at MIT, working at Lincoln, and he managed the ARPA program there. Lincoln was a contractor, he had probably a million or two worth of contracts there. So he came to me and said, "Steve, we're having a program review up at Lincoln. I'd like you to come with me, because they really don't listen to me up there. They still treat me as a graduate student." You know, here's little Larry Roberts in his white pants, and he's gonna go up and tell us what to do. So, he asked me to come up. Well, I went up with him, for this program review, and I was there to give him political heft. I wasn't necessarily supposed to do anything else. But, anyway, we listened to the review, and it was absolutely the most pedestrian stuff I've ever heard. I still remember -- I still have the agenda of that meeting; I ran across it in some old papers a year or two ago. About 3:30, I finally got frustrated. I said, "Why don't you guys do something interesting? Like, fixing it so computers can understand speech?" Well, "humpf, humpf," "what do you know?" and so on, and so they did some thinking and decided to have a summer study, and out came this summer study -- this must be '72-ish, or so -- and came a little book you've probably seen on speech understanding that laid out the program that became the DARPA Speech Understanding Program. And, so there was a case where, again, it was not just signing the checks or listening to briefings and having them on ahead, it was an involvement in the direction the program had to go.

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O'NEILL: Were there more people than just the Rand people there at this review?

LUKASIK: . . . those were Lincoln Lab.

O'NEILL: I'm sorry, I meant Lincoln.

LUKASIK: No, it was just Lincoln people, Larry and myself. It was their review. Oh, give you another example -- on the AI stuff, the AI stuff at that time, research, was -- the AI researchers used games as the domain because the rules are known, and they're not mysterious, and they're reasonably finite, and so the idea of teaching a machine to play a game was a perfectly good way to study machine intelligence. The other generic task that AI researchers worked in was robots. And, there it's a question of path planning and structure and tasks and logical ways -- block stacking was a favorite one, because, you know, you have to know that a block won't fall over if you put it too far out; so there's a fair amount of intelligence about block stacking. So the blocks world, hand and eye manipulators, robot path planning and game playing was the sum total of artificial intelligence up until about 1970. About that time, I said I was enthused in it, but I also had this interest in transfer and, by the way, the speech example is another of those transfer issues. Eb Rechtin had been very, very unhappy about the eye-hand class of work. He felt that it was not computing and artificial intelligence, it was mechanical engineering. He had a point, because, obviously, if you take a million dollars and you pour 800,000 dollars into better hinges and flexing and so, you're not exactly doing computing. How you flex was the smaller part of the program, it was building these crazy machines. You know, there was a thing -- I forget what the SRI machine was called, but it trundled around and had little wires --

O'NEILL: Was that SHAKEY?

LUKASIK: SHAKEY, yes. And, so Eb wasn't interested in hand and eye stuff, and he had a point, because the emphasis was on the engineering issues, not on the computing issues. So he said no more hand and eye research. So, we backed a little bit out of that. Anyway, along around 1970 or so, the stuff was looking very good and, again, I

said, "If this stuff is good, why don't we try using it?" And so the speech work was one of the AI applications that we pushed. It came for two reasons: it came from AI applications; it also came from a dissatisfaction with the Lincoln Lab -- the quality of the Lincoln Lab work, or the direction of it. The other one was the processing of data from underwater acoustic arrays. I won't lead you through the whole ASW business, but an array can tell you there's a target there and you follow the target and you lose the target, well, with artificial intelligence techniques you can figure out where the target was roughly going, and therefore tell the next array that it ought to be seeing it. The next array can now start to point its beams where the target was, so kind of do a hand-over, so you can do continuous tracking. But, it requires, "AI," so we got some work going in the ASW world. So those were the two vehicles I used; one into ASW, which was kind of very military, and one into speech, which was sort of a dual-use technology but had obvious applications to command and control. So, that's just a very long answer that says, no it wasn't just gee whiz and patting them on the back, it was understanding what they're doing, grasping its implications and relating that to the world of problems that the Defense Department either had or would be facing.

O'NEILL: How much did you rely on -- it sounds like you're interacting with this whole community of contractors. How did you interact with the Director? It sounds like you and Larry Roberts were very close, informal.

LUKASIK: Yes, yes. And Bob Taylor, too. First of all, the office was small. If you go to IPTO now, I mean, it's an army of people; there must be 20-25 people working there. Have you been to DARPA lately?

O'NEILL: No, I haven't. I know it's more than what it used to be.

LUKASIK: Well, anyway, yes, there's a bewildering variety of people and they come and go. But there was only like three people: there was Larry, Bob Taylor and Barry Wessler. A three-person office. So, you know, interacting with the office means having lunch with three other people, or two of the three. So, quite honestly, it was -- just to describe it in sort of bureaucratic terms -- usually you have trouble getting the boss's ear. When you can have the boss's ear because the boss is interested in what you're doing you obviously spend a lot of time talking to him. So, it was one of those things where there's an attraction between two people: the chemistry is there, you're interested, you

like each other, you talk to each other, and so you tend to have more interaction in areas where there is a natural affinity of interests, a particular degree of respect. And, so some of the offices avoided me because they felt I wasn't sympathetic to what they were doing, and there were offices that were attracted to me because I was sympathetic with what they were doing. That meant all of the 6.1, because I really had a basic research flavor, and my old nuclear office. Obviously, they knew I knew. And then there were shades of interest in the other offices.

O'NEILL: Well, that would seem to bring up the question of how you replace these people when it's time for them to move on, because it's such a close group, a small group, very informal and very personal. One thing we don't very much about is *how* new Directors were really chosen and some of that background. Now, Roberts left in '72, I think, and you were in charge at the time, so you must have been involved in looking for a replacement.

LUKASIK: Yes, it was very hard.

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LUKASIK: The problem of replacing an office director was a general problem and, curiously enough, it was hardest in this area of IPT. Most of the time, in the case of the other offices, an ARPA Office Director position was seen as very powerful in influencing directions in the field, a certain platform to project one's own ideas of what you think is important and so on, and so it was usually *not* difficult to fill a job. In fact, if there was any difficulty in filling an office directorship, it was in getting the right kind of person, because there were certainly a lot of self-serving people, people looking for power, and so what you had to do was find someone who was not only technically competent in the subject, was managerially competent to run tens of millions of dollars of programs, could do so within the context of working in the United States government, as opposed to working in industry or being a professor in a university. So, by and large, what you would have if you had an opening -- we'll get around to IPT, where it's very different -- you had an opening, you could probably think of half a dozen people -- or three or four people you'd really like to have -- you may easily generate a list of ten or fifteen or something if you took the over-the-transom applications, the recommendations, and the like, you're going to have a pretty long list. Some were you interested in, and so on. And

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so you usually had options in filling an office directorship. Not so in the case of IPT. Probably the brightest group -- that may be doing a disservice to other groups of researchers, but -- very possibly the brightest people in the country. And, it was leading edge stuff, rapidly moving in the '60s and '70s -- still rapidly moving -- and, frankly, none of them wanted to stop what they were doing to take a job to manage the thing. The IPT previous -- it was also true that replacing, filling a directorship, was usually viewed as a job of the outgoing Director. You know the field, you know how good you are, you offer me up someone equally good. The only time you didn't do that is if you didn't like the direction you were going, in which case you didn't want more of the same and so you try to do something different. In the case of IPT, the community had a very -- in fact, it was probably the one area in which I was unhappy with them -- they were bright people, they were doing good work, but they didn't seem to have the collective responsibility that says, "Hey, guys, one of us has gotta take their turn in Washington." And, I went after any number of principal investigators. Some of them were very academic and wouldn't have the managerial credentials, but there were a fair number of them that had all the credentials, and they just wouldn't come. I had a terrible time.

O'NEILL: Give me some idea of how many you're talking about. Are we talking about five different people that you approached? Two different people?

LUKASIK: Oh, five to ten people.

O'NEILL: Okay, that you individually went to and spoke to yourself?

LUKASIK: Or someone went to and spoke -- Larry Roberts may have done some recruiting. Bob Taylor might have done some recruiting. But, in general, in fact, they probably talked to more. It would be more interesting to ask Larry Roberts and Bob Taylor, who were the two Office Directors through this period. Anyway, we just had trouble filling it. Luckily the office started to grow on its own, so Bob Kahn eventually became an Office Director. Craig Fields, in a slightly different field, nevertheless, became an Office Director. I had to bring Licklider back. Lick had headed up the office around '64, and this is not a reflection on Lick, but I don't think it's quite fair to ask people to do something twice when someone else ought to be taking responsibility, and so Lick came back as an IPT Director. After I left, a

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fellow named Dave Russell became Director. He was an army colonel and, while I think he did his best, he was not in the mold of a Licklider, or a Roberts, or a Bob Kahn.

O'NEILL: Now, he was -- the search for Russell would have taken place after you were gone.

LUKASIK: Yes, that was after I went. But I always felt that the IPT research community were not -- didn't quite discharge their responsibilities, considering that ARPA literally funded their field. It wasn't much of an industry in this business, and NSF had very limited resources. NSF was behind the Defense Department in recognizing this, so that, in those days, the ARPA program was probably three-quarters of the national research program in computing, outside of the engineering of machines, and I always felt that they really should have gotten together and gotten the ten principals in a room, and said, "Okay, Sam," or "Okay, Jill," or whatever it is, "it's your turn," and they didn't do that. They not only didn't do it, they didn't respond to entreaties. But, the whole business has gone anyway; one can't say this was a national disaster. Anyway, that's the way the process worked. You try to get the outgoing Director; unless you want a change, you generally had a good selection, except in IPT, where it was very hard. And, of course, the researchers were so good the Director had to be at least as good as the best researcher. So there was a very high standard that you had to meet, and yet those peaks of excellence. Now, I almost had Ed Feigenbaum at one point. Ed Feigenbaum was willing to come on the grounds that the office could operate out of the West Coast, and I was interested in doing that because, by that time, we had the ARPANET, and it would have been quite interesting to have run an office. I was willing to do this management experiment that says you're managing a program for ARPA, and you're out there and you're on the net. I don't care whether you're on the eighth floor or you're three thousand miles away. So, I was willing to do the experiment. It collapsed, and I forget why it collapsed. I think, in fact, it dragged on so long that Ed got other commitments, or we couldn't agree on something -- but there's the kind of person I would have gone after, you see. Ed Feigenbaum would have been a superb Director of the office and a natural extension of the quality people that have worked in that area.

O'NEILL: Licklider was back for his second term of office. Were there very many changes from how things had been running under Roberts that you saw?

LUKASIK: No. No. The changes were largely externally-driven changes. More requirement to document and justify programs, more efforts related to relevance, more probing Congressional questions, so in his second tour, it was not like his first tour. His first tour, it was in the formative stages, and ARPA gave directions, and did things and by his second term, it was a chore working as the Office Director. You just spent a lot of time doing things that, in earlier days, simply were not done -- weren't seen as important -- but by that time, there was so much critical focus on the defense budget through Viet Nam that almost anything you did had to be -- and the whole matter of that was a time when ARPA used to -- we're now slipping into the current usage -- ARPA had a lot of sole source contracting freedom. Basically, in the good old days, ARPA decided who to let a contract to, and said, "Give them $300,000," and that was all there was to it. There was a program management office that in a sort of perfunctory way generated a piece of paper so that if anyone said, "Why did you give $300,000 to X?" the answer was well, there's this piece of paper -- a very short paragraph -- excellence of work and unique capabilities and special facilities, and blah, blah, and we'd write some sort of pro forma thing and that was all that was needed. Nowadays, competitive procurements and RFPs and source selection boards and meeting criteria, and contract award protests -- the whole business has just become very -- the desire for fixed-price things -- this whole matter of how the government buys things has become very complicated. That was beginning to happen in the early '70s.

O'NEILL: It would seem like, in the time that you were at ARPA, you would have seen both the old way working and the new way coming in, if not fully engaged by 1975.

LUKASIK: Yes, that's right. I saw that. Let's see, there was another thing that was happening in the matter of recruiting that's worth mentioning, and that has to do with the matter of interest rates. Interest rates, if you remember, in the early '70s were a period of inflation, severe inflation -- eight, nine percent inflation. Interest rates were, therefore, going up. It was very hard for people to take a job in Washington because they'd have to sell their house where they had a five-percent mortgage and buy a place where they'd have to have a ten-percent mortgage, and so there was some growing immobility due to housing costs. It was generally more expensive in Washington than in some town where the University of X was, and interest rates were high, so you were trading off. Congress

kept the cap on upper-level wages because there was a structure that you couldn't get paid more than a congressman, so we were facing a situation where we could offer people only, like, through that period, the top level of salary I could offer went from about $25,000 to $36,000. So here you're offering someone something in the low-30s, they're trading in a low mortgage, they're going to a high-expense area, and so we were finding -- and we were just beginning to see the beginning of the two-career family, which also cuts down mobility. So there was some externalities quite apart from the responsibility or lack of responsibility of the community that was making it hard to move the right person X into the directorship of an office. And not just directorships; the same was true for Program Managers, except program managers were lower so they lived in less good places and they'd be satisfied with a lower-cost house in Gaithersburg and they'd commute. That's all.

O'NEILL: During this time when there was additional external pressure, Congress and, I guess in some sense, some public scrutiny.

LUKASIK: Yes, a lot of public scrutiny.

O'NEILL: Did you have a set of criteria that you used to judge any given program? Or IPTO programs? Would it get so many points if it was easily transferable and so many points if it was --

LUKASIK: No, I've never believed in those management by the numbers -- I probably wouldn't have said this at the time, but I'm looking back. Yes, there were two criteria: quality and relevance. We weren't going to put money into things that weren't of use to the Defense Department. Remember that whole relevance flap in the late-'60s --

O'NEILL: The Mansfield amendment?

LUKASIK: Mansfield amendment, yes. So, we had to show everything was relevant. Well, the belief was that the Defense Department was spending a lot of money on things that weren't relevant to defense, and by golly, they were going to find them and cut them out. It was really a way of cutting the defense budget by alleging that something

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wasn't relevant. So, everyone who was in the research business stood on their head and for a year wrote relevance statements. But, in fact, hardly anything was eliminated because, in fact, the department had been pursuing relevant research. The Defense Department is a mission agency, it knows what it's about and all about it. But I had to worry the relevance issue. IPT gave money -- they have this Centers of Excellence thing -- we stopped calling it Centers of Excellence. That didn't sell; that was one time. We just picked a better name for it. But, anyway, IPT had a thing called Centers of Excellence. So they had, like, eight or nine universities. They're what you'd expect: Carnegie Mellon, MIT, Berkeley, Stanford, Utah. And so they used to give them large chunks of money. And, there was a certain amount of local autonomy to do the right thing. Well, about AI, we got them away from the game-playing and the block world into things of more direct utility, because I thought it was ready for application. I was in an office in IPT one day. It was just like this office -- there's piles of stuff all over the place, so I looked around as I was talking to someone, but my eyes are going around -- and, by golly, there's an orange report. Well, orange is my favorite color, so it caught my eye. So, I pick up this report and look at this -- I won't name the university -- it's on computer-assisted choreography. Now we're in the middle of the Mansfield relevance stuff, and here's this report. I have no objection to computer-assisted choreography except that it acknowledged support from the Advance Research Projects Agency. This is the kind of thing -- I don't need computer-assisted choreography. So, I told them, you have to knock that off. Well, it turns out it's not an unreasonable thing to do, because you're talking about formalisms and ways of describing motion and ways of describing coupled things, I mean, heck, almost anything is relevant to something. But, nevertheless, in 1969 or 1970, or whenever it was, I didn't need computer-assisted choreography. So, there was a case where, if one had to draw a relevance issue that was on the other side of the line.

O'NEILL: Okay.

LUKASIK: I could make the argument, but it would only sell to a university type. If someone said I want to do computer-assisted choreography and here's why it's relevant, I'd say, "By golly, you're right." But with Senator

Mansfield looking over my shoulder, the answer is "Thanks, but no thanks." So, relevance was really important. I consider it to be the primary criterion, but one which is unimportant because A) almost everything was relevant and B) we were pushing relevant things anyway. So, while it was a first thing, it was a test that you almost automatically passed all the time, so it was not there for a significant discriminator, even though it was mandated by law. We couldn't spend money if it wasn't relevant to military operations. The second one was quality. If you assume now this parade of people and ideas that come through your office are relevant, and that allows 99.9 percent of them through, then how do you pick it? You pick it on quality. So, that if someone said, "I've got a fantastic way to build a new computer," and they were very good people, one would be interested in that, even though you could probably argue that the computer manufacturers ought to do it. Computer architecture was one of the issues that we had. On the other hand, if someone said, "I've got this fantastic data fusion idea," and it turns out they really weren't very good researchers, why should I stuff money into a more interesting idea if they're not very good people. So, therefore, I tended to see quality as really the important discriminator. Now that really discriminates, because you line up a hundred things, and you just know that the top twenty of them are fantastic people and the bottom twenty are kind of . . .

O'NEILL: But, who's lining them up? How are you judging?

LUKASIK: The Office Director did that, but here's the way my quality criteria would work. Let's say that we're doing artificial intelligence. That's a bad example. Let's say we're working in --

O'NEILL: Graphics?

LUKASIK: Yes, graphics. And, so let's say that that's relevant and there's some high-quality work going on and so the Office Director comes in and he's got his AI program, his graphics program, his computer architecture program, his whatever other sets of programs -- his speech program -- each Office Director would have, in essence, a menu of things. And now, let's say you know that office is probably going to get about $20 million in annual budget. It may

get 21 or 22; it may get 15 or 16, but you always have a sense from last year, keep things going, some things finish up, what's new, so there is turnover. And, if you're looking down the graphics things and you say, "Well, you know, this is pretty good stuff and we put $3 million in," and so on, and he starts to explain it, at some point down the list -- let's say he's explained two and a half million dollars worth of the program, and then he tells you he's going to give some money too, to East Overshoe and you just have the feeling, "Why is he telling me this?" He knows that this is the bottom of his list, and he essentially says, "Look, if we only get two and a half million we go with those and if there's an extra 500K, I'll put it with these other three places." And so, in some sense, the quality judgements were not whether to fund or not to fund, but where to draw lines, and the lines were drawn by quality. And when you got down -- now, he could come in and say, you know, "Boy, there's this new professor at East Overshoe. He's got a fantastic idea. Graduated from MIT and is working with DEC, and he's just got some fantastic idea," well, sure, just because it's not known doesn't mean you don't support it. But, you sort of looked for where the risks were larger, you know, where the ideas were less formed, where there was less of a track record, and if an Office Director couldn't say, "This is a sleeper, but it's very good," and if Larry would say that, or Bob Taylor, I'd generally go along with it. So, that's how the quality issue -- you go down the list and you start to draw a line and you say, look, your work in AI is so high-quality we've got to put three and a half million into it to cover the really good stuff. Now stuff in graphics isn't quite so good. We ought to draw the line at 2.5, because there's work there that's kind of more of the same and the guy wants to add 256 colors to the display instead of 16, and well, that's sort of an extension. So, that's kind of the way the discussions went. Quality, relevance and working down the list, and comparing it with other things.

O'NEILL: Now, when you say comparing with other things, you mean across offices or within the office?

LUKASIK: Well, both. When Office A comes in, Office A has, in essence, five or six programs and so you do that and then you set Office A aside and the next day -- this is a budget review that we have about three times a year -- and the next day it's Office B. So, what you have is from Office A, you have the "must haves," I'll say 18 million, and some other ideas if you had an extra million or two, and then that same thing with Office B. Office B comes in and they have a $30 million budget. There's about 25 of the "absolutely solids," and then they have a whole list of things. So, then when you're all through with the budget review, you take all of those absolutely solid stuff and you come to some

number, and you think you will have room in the budget, some other larger number, and then you look at the solidity, if some other office has a $25 million budget and a $10 million list of really good stuff, and the IPT office has an $18 million list and a $2 million list of good stuff, you know the other office is going to get more of the disposable income. So, there's always a leveling, so that what you end up doing is -- one would like to think in this ideal world -- sorta like the notion of a Fermi level in semi-conductors, you'd like to feel that the first program that wasn't bought in, let's say the Tactical Technology Office, would be worth about the same amount to the country as the first program that didn't get bought in the IPT office. So then it's a front office judgement as to how the IPT program might have been just fantastic intellectual content, and, but, you know, a little bit flakier and a little bit less direct, and these other people have a smart weapon program -- By golly, the department needs smart weapons -- so, in fact, you would begin to level and the job of the front office was to make sure that each office got the right amount, but that they got the right amount for the right things, and that the things that were below the line were about equally valuable across all the offices. And we did that about three times a year.

O'NEILL: So three times a year, as Director, you would meet with each Office Director separately, and go through these lists that they would have.

LUKASIK: Right. We had a book about that thick, and it was line items. There were probably six or seven hundred line items. Not necessarily six or seven hundred contracts, because they could be Phase I, Phase II, studies and needed smaller or larger amounts of money.

O'NEILL: But, you would go on that detailed a level across each program?

LUKASIK: Yes, we had about -- I had about six or seven hundred, as I recall, line items.

O'NEILL: You mentioned briefly about when to have manufacturers do things, computer architecture problem, this leads to the ILLIAC . . . What was your feeling about involving manufacturers in doing some of this research which was obviously not defense-specific?

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LUKASIK: Well, it didn't have to be defense-specific, it had to be defense-relevant. That's very different. Defense-specific means only for defense. Building a parallel processor is -- now the jargon is dual-use; we didn't use the word then -- but, the important thing is that it be useful for defense. It can be twice as useful for other things, that's okay. The whole magic of ARPA, then as well as now, is the ability to create teams, groups, to organize technical effort for the country. Now, for example, if ARPA's pushing networking, well there's researchers and there's people who want to do demos, and people who want to build things, and there's a whole bunch of different people involved. And so when ARPA does a program, it says, "Here's the tasks, and here's the order, and here's the people who can best do them." Some of those things are naturally researchy things, some of those things are people things that you don't want researchers in, and some of them are things that you have to build things. For example, you can have a Len Kleinrock at UCLA, who's just very good at networking, queuing theory, protocol -- some very abstruse technical questions. On the other hand, when you want to design an ARPANET, well, you don't want a university professor who's there and changing his mind every day as he gets new ideas, so you hire a BBN. And BBN will systematically put the thing together. On the other hand, if you want to -- if you critically need a switch, BBN has had a spotty record in designing and building computers, so you go to Honeywell. So, if you're designing and building hardware, and managing systems, and if you're doing research and getting ideas, and then maybe you want a MITRE in there, because you want to relate this to how command and control is working. So, there's a role for everybody. The critical thing about an agency, like ARPA -- and almost unmatched in the United States government -- is an ability to take on a difficult task, structure the task, get the best people, get the best people in each area to work together. Sometimes it's supporting infrastructure, sometimes it's basic research, sometimes it's building things, sometimes doing demonstrations, and so there's room for everybody in that. So, it's not really a question of I like universities and I don't like industry. I do like industry, I do like universities -- in fact, depending on the job and how it structures, there's an important role to be played for everyone. And, Military Services, which tended not to be funded directly, but there's another piece of the budget which says making sure that Systems Command, Electronic Systems Division, gives you a program office that has a colonel and three good majors in it, it's all that kind of work that you have to -- which is not the money, but is the work of recruiting, as it were, or management, or whatever you call it.

O'NEILL: Would it be the Office Director's role to figure out how to achieve those things by balancing these areas, or was that more of --

LUKASIK: That's the Program Manager's job. Because, remember, all the world is hierarchical. Remember, everything that I'm describing is also true for any one office. An office in ARPA has Program Managers. The Program Managers each have their one or several fields of interest, that their responsible for. And so they're putting together things and they're offering to the Office Director, and he's doing the same thing as I did. This guy wants a million, but he can use an extra 200K, and this guy wants one and a half million, and you know, and he could use an extra couple of hundred K.

TAPE 2/SIDE 1

O'NEILL: Your view of ARPA funding was characterized in the Barber study as ARPA just needed just a little more money, not a lot more money.

LUKASIK: Well, it has to do with this quality thing. You know, suppose you tell me you have $200 million, and I line up a program and I say, "Congress, that's the best program I can put together for the $200 million that I think you're gonna give me." And then some things aren't included in the program. So now the question is, if there were Santa Claus, I could do some of those things. How badly do you want Santa Claus? What that says is, frankly, if Santa Claus came and said you could have whatever you wanted, I would have asked for another $20 million. Now, some people say, "Oh, Steve, you just didn't appreciate the opportunities. You're just a small thinker. And, my golly, there was another $200 million worth." I didn't see $200 million worth of good ideas. So, I was always a little bit conservative. On the other hand, if someone had said, as now Congress goes to DARPA, and said, "DARPA, here's $100 million, and we want you to spend it." And then two weeks later they say, "DARPA, here's another $150 million, and you're the only person that can --"Now people come and hand it large amounts of money. So, it's just a different world. So, no, I always felt that there were -- DARPA could have used more money, but I guess I would have been

rather conservative. Not conservative as a businessman, but conservative as an intellectual manager. The ideas weren't *that* good.

O'NEILL: So the bottom of the list probably shouldn't have been funded.

LUKASIK: That's right. I never -- I don't think there was ever a time -- well, I had a baloney- slicing -- I don't know if this is relevant -- I had a baloney-slicing approach. If you tell me we're going to do night vision, well, gee, I can spend a million dollars on night vision, I can spend $10 million on night vision. If you give me a million dollars, I'll find 10 good people, and I'll give them 100K each. If you say, "Well, I want to spend $10 million on night vision." Okay, we'll set up a test range, and we'll set up some experiments and we'll set up some target signature programs, I mean, we can do more but in some sense we're not advancing it until we get a sensor -- and I'm perfectly happy to find the 10 best sensor people, and give them some money to pursue their ideas. So, sometimes you can over -- we have this phrase: "throwing money at a problem;" we do have a tendency to throw money at a problem -- and my feeling is if you don't any ideas, then there's no point throwing money at it. Now, the response to that is, "Well, if you had a lot of money you could throw money at the problem, you'd be surprised at the people that would crawl out of the woodwork at East Overshoe, or the XYZ Furnace Control Company, or something." Well, maybe that's true, but I have tended to see the nation as being short on resources, and therefore, a desire to play them cautiously on something. Albeit at the risk of losing some ideas that somehow don't get funded. So, I tend to be more of a conservative in that regard.

O'NEILL: Were there programs that were underway that you didn't think -- other than this one that you mentioned at Lincoln that was re-directed -- were there other programs like that that came your way and you said, "Let's stop funding these guys"?

LUKASIK: Well, the whole collection of programs -- shall we just talk about IPT?

O'NEILL: Yes.

LUKASIK: Okay, let's just talk about IPT, because the money we were stuffing into counter-insurgency to no effect was impressive.

O'NEILL: One question first: Was IPT typical of the other offices in this regard?

LUKASIK: No. IPT -- I never had hardly any trouble -- now this may be a statement of a bias rather than an objective statement -- no, I had very little trouble with what went on in that office. But let me tell you a couple of the areas. Not to unduly take Lincoln Lab to task, but rather the whole area of artificial intelligence needed a push, because the researchers would have played with blocks and hand-eye coordination forever, because they were just -- We had -- clearly, ARPA had to push them into networking. Networking was not a natural desire of the computer science community. In fact, it was done for reasons of managerial and administrative efficiency, quite frankly. I mean, it was probably a better idea than even ARPA recognized at the time. The work in this whole business of writing, of paying a million dollars a year for people to write operating systems, was clearly not a good idea, it was time for the country to begin to standardize on operating systems, not proliferate them. The work in computer architecture was most -- I shouldn't say most -- was controversial because the belief was that industry should do this, because after all, they were going to make the money selling the machines. Let me give you an example of a specific on that. In about 1973, Larry Roberts came in -- I'm not sure of dates, so it could have been Bob Taylor, but in any event, the IPT Office came in and they wanted to fund what we then called a LISP machine. And, I put it in the budget because I thought it was a good idea. And the reason I thought it was a good idea was because I was interested in AI, and I felt that if you give the researchers better facilities they'll, of course, work faster. I mean, if you have a -- if you're slugging out some artificial intelligence on a general purpose time-shared computer, don't expect to make as much progress as if I give you a $100,000 or $200,000 special LISP machine. And that was rejected by DOD management on this argument, that said that industry should do it. Consequently, what happened was I think things were held up and it wasn't, therefore, until somewhere around later on in the '70s that people like Symbolics went ahead and, wherever they got their money -- maybe through side channel from ARPA anyway -- that's the way the whole community operated, in and out of

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ARPA, but I think that we would have had LISP machines sooner if ARPA had been allowed to push it earlier on. I think that the stuff that happened in the '80s in artificial intelligence would have happened five or six years sooner, because as soon as you build a machine, then you build ten of them, and you give them to your researchers, and you build another ten, and the whole business would have caught on sooner.

You brought up ILLIAC. ILLIAC was another example where the heavy hand of IBM didn't believe in parallel processing -- I don't think it really still believes in parallel processing -- and so, ARPA was going to do it anyway. And, of course, Burroughs came along -- they were sort of a peripheral operator in the computer industry, but nevertheless it's the peripheral people who have more resources, more to gain by success and so on, to explore these ideas. So, computer architecture was another one of those that wasn't getting enough money. Here's an example of where if I'd had more money I would have put it in. So, the graphics work, ARPA didn't do much at the display end, and probably should have done more in hindsight, but I don't think there were any good ideas around at the time. So, there were a number of areas where the issue was not you shouldn't be doing it, you should be doing this instead of that. So, no the IPT office had as many debatable issues, pro and con, and in fact pushing them into -- I shouldn't say pushing them, because people are always willing to take money if you offer it to them -- but, leading the artificial intelligence people into applications was good for the office, good for the field, it led to a certain amount of over-selling and hype of artificial intelligence in the early '80s, but I can't be responsible for that. And, so there were a number of areas where it needed its own corrections. I would say it was no more or less perfect than any other office.

O'NEILL: Were you involved in the situation at Illinois, when the ILLIAC project was having problems?

LUKASIK: Yes, that was -- I was the person in whose office all that stuff landed. Remember, there were two problems with ILLIAC. One was a technical problem. That is, the contractor -- and not just Burroughs, but I think it was TI that was to supply what we now call medium-scale integration -- were not able to deliver on their technical promise. It was probably one of the first times when I learned that you can't have too many new things in a new thing, because one of them is bound not to happen, and then the whole project collapses. So, we were pushing machine architecture, software for parallel machines, what was then medium-scale integrated circuits, and then the application of that to a variety of important defense problems: 3-D hydrodynamics, obviously cryptography

potentially, and some other things. So we were really trying to do a lot, and the contractors didn't deliver, so the whole machine started to scale down, I mean, collapse upon itself. It got to be from a four-quadrant machine to a one-quadrant machine, and those quadrants were cut back in performance. There was an important technical error made -- it never had enough memory per node. Now memory was kind of expensive then, so I shouldn't say it was a wrong decision, technically, but as the -- It suffered from what happens to so many weapons systems programs, that is, it's a stretch goal, they're not met, you fall behind schedule, the budget starts going up, you start chopping around, everyone has a sort of "the sky is falling in" mentality, and what comes out of it is often either canceled, weird, or finally works but is much delayed. ILLIAC was of the sort of machine that ended up weird, what we would now call weird, for parallel processors.

So, then on top of it, quite apart from those legitimate issues of how much is it going to cost and how much of your budget are you eating up in this enterprise -- which was Rechtin's major concern. Then there was the transfer, that is, after we build it where are we going to install it, and who's going to run it? In the normal course of events, we would have installed it at the University of Illinois, because they're the people who invented it, because remember, Dan Slotnik had been pushing parallel processing and so we would, of course, have put it with "the inventor" or the principal investigator. That's at a time when Dan -- and we were all kind of upset -- said, "Yes, the Defense Department has treated me well. Yes, they have supported me on ideas that I think are important," and then he came out against the Defense Department. So, it was very clear that if the person at the University of Illinois thinks you're a bad person, a bad agency, you can't give them responsibility for this machine. He literally said, "Thanks, but no thanks," and we feel he walked away from it. So it was left to us to find a place to put the machine. We did a fairly organized site review -- it seems to me Livermore must have been on the list; NASA Ames was on the list, and I can no longer remember -- but it seems to me we had like five possibilities, and they were typically mission-oriented agencies, not a university. And, so we finally put it with NASA, with some misgivings because someone said Defense put all this money into it and you give it to NASA which is not a Defense agency. But that was the best place and it caused the start of the Center for Advanced Computation at NASA. The machine got used for shuttle calculations and lots of other good things. There were several problems in the shuttle, and some of them had to do with heat transfer.

O'NEILL: Okay. Yes, there's a lot of different questions, but in the interests of time --

LUKASIK: Let me give you something. I was asked to write an article about ARPA for *The Encyclopedia of Telecommunications.* It discusses a number of programs.

O'NEILL: But it's *The Encyclopedia of Telecommunications*?

LUKASIK: Yes, so you can have that.

O'NEILL: Okay, thanks.

LUKASIK: And then if it stimulates questions --

O'NEILL: Was there a problem with the name change? I mean, people still today use ARPA, and that changed in 1972.

LUKASIK: I know. It happened in '72, but I thought it was -- I wasn't in favor of it. The Van Atta guys and a number of people seem to think that it signalled the difference in the agency --

O'NEILL: Secretary of Defense?

LUKASIK: The Secretary of Defense believed in giving the services more autonomy, because that was the view in the Nixon administration. They wanted to make the Office of the Secretary of Defense smaller, so one of the ways that they did it was to make their number smaller, was to establish ARPA as a "field agency." Now, in government bureaucracy that's a terribly important change. But, of course, you can't just say you're now a field agency; you have to change the name. Well, all the Defense agencies, like Defense Intelligence Agency, and Defense Communications Agency -- the Contract Audit Agency -- they all had "Defense" as the first word, so therefore ARPA got its "D," so it became the

Defense Advance Project Agency. The popular literature says ARPA hadn't been defense-y enough and, by golly, it now was going to be defense-y, and it was finally going to do things for defense, and it was going to change its ways, and it was going to get away from the old ways, and it was going to do this -- and it's all hogwash. It was a way to remove 150 heads from the OSD roles and make them a field agency. I thought DARPA sounded like a dog food, and I said, in the agency, "I know that's our name, and the four times a year when we have to sign our name officially, we will do that, but we are called ARPA and we will continue to be called ARPA." And, until the day that I left, two years later, we were still called ARPA, even in our acronym was "ARPA," because, after all, there's no legal or bureaucratic rules that say how to make up an acronym. It's just usage that says you take the first letter or the first pronounced letter . . . So, in fact --

O'NEILL: It really didn't signify anything in your opinion?

LUKASIK: It didn't signify anything. If ARPA was changing, ARPA was changing. But that had nothing to do with the "D". It was neither a driver nor a result. That was a purely a little nit that had to do with personnel bookkeeping.

END OF INTERVIEW