



**Dr. Steven Lukasik**  
**1970-1975**

*Interview: January 17, 2007*

**Interviewer:** Your name and years of your tenure ARPA.

**Lukasik:** I'm Steve Lukasik. I was the Director of ARPA officially from April '71 until December '74, but was Acting Director for a couple of months prior and a *de facto* Director for a year prior and that screws up the arithmetic.

**I:** What was the world like during your tenure?

**Lukasik:** Well, the single feature that set the world as it existed then was the development and use of nuclear weapons by the United States at the end of World War II. The Soviets couldn't tolerate that asymmetry. They had already penetrated our weapons development establishment and in September of 1949 tested their own bomb. We were building bombs and delivery systems. They were building bombs and delivery systems, so if one thinks about the country from a defense standpoint, that was the single fact that set the terms and conditions for everything in the defense establishment and international affairs.

Now, there were some smaller things that happened along the way. There was the French in Vietnam. There was the French-U.K. invasion of Suez, the Korean War—but it was a nuclear world. It was a dangerous nuclear world with weapons that can do enormous destruction. And while we could argue we would never do that, it was always a hard argument because we *had* done that, and we had absolutely no confidence that the Soviets would ever restrain themselves. And so that nuclear confrontation, implying great power and great technological expertise, was a period perhaps unequalled in history where issues were posed in technological terms. Technological issues were important, technological development and rate of technological development. We talked a lot about the strategic balance and the technology balance and so we really were in the position of watching the other side very carefully from a number of dimensions: manpower, political, technological, and strategic.

That's what the world was like in the 1950s. And then, of course, the signal event which changed that balance was the Soviet satellite launch, which really wasn't just a satellite launch issue; it was that they really had heavy-lift, long-range rockets. So, the space issue was not just a "we've broken into the high ground." The space issue was, "See, we really can. You've seen us explode nuclear weapons." They were doing atmospheric testing, of course, "and now you see what we can do in space."

Now, overlying that was the Vietnam War. So, in essence, we had a two-level competition. We have the level I've talked about, which one might call the main event, but then we also have the developing world, the breakdown of colonialism, wars of independence, insurgencies, and we have the domino theory. There are things going on in peripheral areas that were linked by the domino logic to the central balance because one of those balances was who owned how many square miles and who had the most strategic real estate and who could therefore base weapons from this real estate. The Cuban missile crisis in 1962 was such an example—there could be short range missiles, which means much more accurate than long-range. So, this competition was largely a two-sided competition. You knew what to worry about, where to look and who to worry about, much different than the world today.

I: How did you happen to come into the Department of Defense?

Lukasik: Well, it was a very logical and direct extrapolation of what I was saying. After World War II, one could argue about military roles and strategic roles and the role of science and engineering in winning that war, but there certainly was in a lot of people's minds that it was technology that won the war. Some of it was production technology. It was aircraft technology. It was submarines. It was being able to build ships quickly. It was nuclear weapons. It was radar. All of these technologies played in. It became clear after the end of World War II that the conflict had shifted and now our concern was not our former enemies but our former friend. We worked very hard to bring Japan and Germany on our side. We succeeded in the case of Japan. We ended up with half of Germany. We saw that technology was really important. This was not just a question of turning down our level, demobilization, a switch to a peacetime economy. We did, of course, because there were a lot of civilian needs that were unmet from that 1939-1940 through '46 period. But technology was going to be important.

I came out of college in 1951, right after the Soviets had exploded their weapon and when space was at least—(chuckles)—on the horizon, to make an unintended pun. And so the Defense Department put heavy emphasis on the training of people. I was at MIT and the Office of Naval Research, in some sense the country's first science agency, had a large program that was really just to train graduate students. I spent a little bit of time in anti-submarine warfare while I was a graduate student. Now, the effect of that is not only that you have received critical financial support but, in fact, they're *friends*. And you thought about submarines. You thought about anti-submarine warfare. It was quite natural.

Then what did I do? I went to Westinghouse. What did I work on in

Westinghouse? I worked on reactors for submarines which has totally changed the nature of undersea warfare, the fact that you don't have to come up for air every day or so. I did that because it was the most interesting job. So, there was this natural relationship, which was not uncommon, especially for physicists. There was another aspect of this period. We talk about science and engineering and technology, and give the engineers their due, but the stars in the production at that time were the physicists. In fact, an earlier story is one about pointing me in the direction of physics.

When I was fourteen years old, I went down to the corner store one August morning to buy the paper. For some peculiar reason, the family never had the paper delivered so we went down to the corner store to buy the paper. And there was the headline that said, "U.S. DROPS ATOM BOMB ON JAPAN." I went in and the proprietor of the store—very fine man, amazingly smart man for a candy store owner and operator—proceeded to tell me about nuclear reactions and how important this is.

So, here was another case. This was even before I went to college. I'd been thinking about science. I'd been thinking about chemistry. Somehow or other, I schmoozed into physics and now I've joined the team of physicists. If you look at that era of physics, and there certainly have been some very important engineers in this whole process, there was just this natural flow of physics and public policy. I don't know how the community developed, but in—certainly the scientific community—there were those who felt very guilty about what they had done with the bomb, and those who felt not only very proud of what they had done but felt that this was really an important thing, and there was nothing to be ashamed of.

So, you had some physicists who would join the weapons labs to build more and there were other physicists who would join the Federation of American Scientists and people that started the Bulletin of the Atomic Scientists who were looking at the downside of all this, disarmament. When you were in physics, it was more than just being interested in physics or interested in the applications of physics, but you were sort of marching in a parade. And that parade had a very strong public policy aspect.

And then, of course, if you have any interest in history at all, history is really public policy after it's happened. You had the feeling, in the fifties—because I got out of MIT in end of '55—you had this feeling that it was a big problem. It was an important thing and, yes, you had to make a decision on how much you wanted to be in it and how much you wanted to be focusing on disarmament and how much you wanted to go do something totally different. But you were just swept into this and, of course, people on the street felt this too.

Well, this was not World War II but it was the equivalent, remember, we were building fallout shelters. I dug holes in the yard and prepared to live out a nuclear attack. This was sort of a time of air raid drills and taping school windows and that sort of thing. We were really besieged. We were pushed by the Soviet Union and aggressive communism and so we were all part of it. Much more than now, I think, when you can sort of ignore the business. I mean you wring your hands over terrorism and then you go off and worry about whether

you're going to get a hybrid car or not and where you stand on global warming or something. But those things are not quite having the punch that the thought of imminent destruction brought on.

I: How did you move into DARPA?

Lukasik: I entered as an Office Director. That was an interesting thing. I said I was supported by ONR and I was at MIT. I was in the Acoustics Lab at MIT. I worked on ASW, but ASW really equated to underwater sound in those days. And when I was a graduate student I had done some work for what's now called BBN, then called Bolt, Beranek and Newman.

I was in the acoustics business and I went to Stevens Institute of Technology after my Westinghouse period, and Stevens wanted to do some things in underwater sound. So, I went up to talk to one of the local underwater sound laboratories, which was the Hudson Laboratories, which was a part of Columbia University. The director of that laboratory was a very fine man named Bob Frosch. And talked to Bob Frosch and helped Stevens get started on some projects.

And then the next thing that happened was that while I was at Stevens I did some consulting with a company called Vitro. I was a consultant on a project, watching Soviet nuclear developments. Now, this was 1958 or something like that.

The U.S. didn't have U-2's and didn't know what the Russians were doing. We were working with the people who ran a ground system for detecting—not just detecting but estimating rates and yields and doing atmospheric sampling. We were working on a balloon system. The idea was you would float balloons over the Soviet Union. For some reason, we went to a meeting at ARPA, and there's my old friend Bob Frosch, who's running the Nuclear Test Detection Office in ARPA. So, one thing leads to another. Of course, ARPA was always looking for people, and I guess I was good enough to at least be above the line, and I got a call from Bob Frosch, and he said, "How would you like to come to ARPA?"

I said, "Well, that is a very interesting place." I had been involved with ARPA doing some consulting also for IDA. So I knew about the Agency and Frosch is in the NTD office but, in fact, he is in the process of becoming the Deputy Director of ARPA and he's looking for a replacement.

So, there's this whole chain of events: defense, acoustics, Stevens, Hudson Lab, Bob Frosch, ARPA, some work in nuclear test detection on a balloon-borne system. There suddenly I find myself heading up the ARPA program in Nuclear Test Detection. I later renamed the office Nuclear Monitoring Research Office.

So, anyway, that's how I ended up where I did. It was another of those characteristics of the scientific community—*still* is the characteristic of the scientific community—where people jump from stone to stone in the river of life. And it's because you know this stone is here and this one talks about that. And you jump to that stone and this one knows you and says, "How about we go this way?" And so it's this path of what the physicists would call a "cooperative

phenomenon.” We were all part of some larger thing, whether we were on the bomb-building side or the disarmament side, but we were all part of the same thing.

We all were working together and, of course, I've just described it as bomb builders and arms controllers but that's an oversimplification. It's a group of people who are very much concerned about the application of important technology to national survival. And so some people lean to the right and some people lean to the left, but it was really a cooperative enterprise among all. And, in fact, a previous Director of ARPA, Jack Ruina, has been very much on the disarmament side, very deeply involved in the Pugwash conferences and so on. That was the science community's—particularly driven by the physicists—attempt, at the depth of the Cold War, to talk to Russian scientists, the idea being, “Let's get together.” I mean, “We built this thing. We have to find our way out of it and the chances are, however we get out of this thing that we all created, we're going to be an important part of it.” And that was certainly the case. The push by the scientific community for nuclear arms control was a very important feature of this process, particularly in the early sixties, because we were now exploding nuclear devices in the atmosphere at quite a rate and it was messing up the atmosphere. And so starting in the early sixties, particularly from India, there was great concern. They were, of course, downwind of all of the Soviet blasts and close.

And so that led to two variants of the discussion. One was, “We shouldn't be testing nuclear weapons in the atmosphere,” which was the weaker form of the argument, and the stronger form of the argument was, “We shouldn't be testing nuclear weapons at all. In fact, we shouldn't be *having* nuclear (chuckles)—weapons. We should get *rid* of nuclear weapons.” So as in anything there's a spectrum of viewpoints but it did certainly come down to putting some lids on this thing because either it's just going to get bigger and bigger and engulf us all—and we almost put the genie back in the bottle—but at least we can put it in a potential well and let it rattle around.

I: How far along was VELA when you took it over?

Lukasik: ARPA was established in 1958. If you look at the Roy Johnson period the central focus was “get us moving in space,” and the closely connected issue was ballistic missile defense. There were other issues that were connected to that. There was propellant chemistry, the matter of fuel for missiles and the more oomph out of the fuel, the bigger payload to the longer range. So, that was the obvious connection there. But there was also this matter coming up, 1958, 1959, about limits on nuclear testing.

I honestly don't know when VELA started. If I had to pick out a date I'd probably say late '59 or early '60, while I joined ARPA in 1966. So, I joined in year six, which was only like year seven of the Agency as a whole. And that year seven will be relevant to the things we're going to explore later.

I: You managed the VELA project, and then Herzfeld leaves. What was your next step?

**Lukasik:** I came to ARPA in April of '66 and Charlie Herzfeld was the person who hired me. By the time I got there, Bob Frosch had left ARPA and was Assistant Secretary of the Navy for R&D.

So, I'm there a year. I think Charlie left in maybe June, July or maybe my fourteenth month. I was on a trip to Scandinavia, romancing the Scandinavians into allowing us to put up a seismic array there. I came back from that trip and I was called to the front office. Charlie was gone by that time. Peter Franken was the Acting Director. Peter said, "We don't have a Deputy Director right now. Would you be the Acting Deputy Director?" which was a little bit astonishing because if you look at my history, when I was at Stevens I ran a small research group that ran at about \$500,000 a year. I am asked to go to ARPA and I'm running a \$50 million program which is a factor of a hundred expansion from the kind of money that— (chuckles)—I'd been used to dealing with. And now, all of a sudden, when I've just barely got my feet on the ground, I'm going to be the number-two person for a \$250 million agency. So all of a sudden, these numbers are getting pretty big.

That reminds me of a funny story, by the way, if I can inject on this matter of dollars and the amounts of dollars you handle, a story that Ed David relates when he was the President's Science Advisor.

He shows up on his first day and, of course, you're immediately whisked off to a meeting and you're with one of your people and you set yourself down and try to follow as best you can. You had to have briefings, right? And you're talking, and one guy says, "Well, I think it ought to be 1.3," and someone says, "No look. It has to be 1.75," et cetera. Ed is trying to participate in the discussion as best he can. And so the meeting breaks up and he asks his colleague, "What was that all about?"

And the guy said, "Well, I think the number that we settled on, \$1.4 billion, was about right."

He said, "*Billion?*" (Laughs)

The President is worrying about trillion-dollar budgets so the characteristic of government is the numbers get frighteningly large and they get large to the point where we maybe don't even appreciate what you can do with them.

**I:** What's the state of ARPA at this time?

**Lukasik:** To some extent, I'm sufficiently new to ARPA and Washington that I probably am not the world's most perceptive person. I'm just a guy who's been sent from his D-Ring office to his E-Ring office to be the Deputy Director. It was a period of uncertainty and the period of uncertainty related to the Acting Director.

The Acting Director did not have the confidence of the DDR&E, Johnny Foster. He was a very fine man, a brilliant man, good sense of humor. Nice person. But he just wasn't resonating with the building. These were serious *times*. We're in the middle of the Vietnam War on top of everything else. Everyone sort of figured out that that was not going to be a long-term solution.

**I:** Foster was not happy with the way things were going.

**Lukasik:** Yes.

At that time, I'm in a funny situation because I'm not trying to take the guy's job, but he's not going to be Director. Frankly, I was just seeing myself as a temporary person. I was going to go back to running the Nuclear Test Detection office. When it was announced probably in late August or early September—Eb Rehtin didn't actually arrive on the scene until November—but it was announced that Peter was not going to stay as Deputy Director. There was this sort of, "What happens to him? What happens to me?" And we're both saying that.

In any event, I drew the typical deputy's job, which was to organize a set of briefings for the new Director. That turned out to be a wonderful learning experience, aside from having been Acting Deputy for a month or two, because we really had to get everyone to come in, say their thing in a sensible way, explain things, not hiding things, not overselling. You had to give it to the new boss straight.

I was in charge of that and when Eb came aboard as Director, he was quite well prepared. Through that period, through a whole set of processes I'm totally unaware of, the Deputy Director decides he's not going to stay. Or maybe they both decide that was not a winning combination so, therefore, there I was—(chuckles)—having been in a position to get everything all set. I got along with Eb very well. I think Eb Rehtin was one of the world's finest men, and he taught me a lot.

I became the Deputy Director and I was always trying to figure out, "Why me rather than one of the other Office Directors?" And no one has ever explained that to me and I never quite figured it out either. But that's the way it was, anyway.

I: Did that uncertainty trickle down through the Agency?

**Lukasik:** Yes.

We're going to get to the same thing at the end of Eb's tenure. The Agency, the people in the Agency, especially the technical program managers, who had joined the Agency typically for a particular number of years, had in mind this was what they were going to do; and if the Agency was wavering, wandering, failing that was an important thing.

You see, one of the characteristics about this, true of a lot of government agencies, people identify with it. The Agency is like a *family*. You *care* about your family. You care about the cause. You care about security. You care about technology. You care about avoiding technological surprise, all the aspects that we all thought we were there doing. And so *anything* that upsets that is like having a fever. When you're not 98.6 you *know* things aren't right.

So, yes. People were quite uncertain, didn't have confidence that Peter was going to pull it off, and, therefore, didn't know who was going to be the Deputy Director. Maybe some thought I had the inside track but I was just there because someone had asked me to do the job temporarily. At one point, I became the Acting Principal Deputy in DDR&E. I forget what happened, but for two months I was in that same position. So, it was normal to be Acting and then

you'd go back to what you were in your mainline job. Since I only had fourteen months in, I had a lot more to do.

I: Had there been any of that transitioning going on?

Lukasik: No. That was all Johnny and Eb, Johnny telling Eb what was wrong and Eb getting the charge to fix it.

Whether there were ideas in the halls, I was never much at picking up the bureaucratic and political gossip, and so for all I know, there were. Certainly, at one point while Eb was Director, he told me when he came aboard part of the brief that he had from Johnny Foster was that people were seriously talking about abolishing this Agency. But I didn't pick it up at the time. I can believe it from the various events that occurred during ARPA's lifetime to date. And so it could well be that other people saw the situation in far more serious terms than I did.

I: What exactly did he implement?

Lukasik: It was a quite important direction that Johnny gave him. Go back to the beginning of ARPA, February 1958: the space program, Roy Johnson, pulling the space program together in Defense, getting it organized, but almost before you could turn around, the talk on the street was, "We're going to have a civilian agency," because there was this *huge* battle over military versus civilian control of the space program.

ARPA was really put in a quite untenable position at that point because it was told, "Get this stuff organized." You're in the Defense Department and you have it all and you know about this stuff, "Oh, and by the way, we're not going to give it to you. We're going to give it to a civilian agency." So, it was really a most awkward position that unfolded.

So ARPA got started and after a year and-a-half it was almost wiped out. Then we had to build up a program. So, let's say ARPA prime started more like 1960 and here it is 1967 and awfully good work done but nothing's come out. That is, it's all still in ARPA. The Agency is growing and is doing good work but everything is still in ARPA. And as long as something is in ARPA, it isn't anyplace else. If it isn't anyplace else, it's not doing any good.

In the formation of ARPA, as you go back through its history—and I suspect other people have already touched upon on this in the interviews—there was some talk that ARPA would do *all* of the Defense Department's R&D. The Services would do none. The ARPA would develop *all* the weapon systems. Now, that was a pretty silly notion, but nevertheless, that was a view in many people's minds. But at least it was supposed to produce *something*, even if you fall back from the position that all ships, planes, tanks and so on will come out of ARPA. You just can't sit there with a charge of protecting against technological surprise and every five or six years, someone says, "How are we doing?" and have ARPA say, "Not to worry. We're not going to be technologically surprised."

So, something was supposed to come out. ARPA had developed a number of processes, getting good people, getting good problems or, responding well to good problems when they're put to them, organizing people, doing

research, contracts, building things, testing things, trying things. But it was fair to say, "Nothing's coming out," and ARPA had no processes for putting things out.

It could kill things and it could influence, by means of public or private statements within the department, other programs that were going on. It was sort of a brain trust, perhaps, but it wasn't producing anything.

As nearly as I can tell, Johnny gave Eb two directions. "I want something out of this place and I want something for my money, something on the street, rubber on the road." And the other thing was, "ARPA has become the Defense Department's National Science Foundation. We *have* a National Science Foundation. The Defense Department doesn't need one of its own National Science Foundation. I want you to cut that stuff out." That's the issue of 6.1 basic research, its relevance, a large set of issues that, again, we may explore later.

So, Eb came in saying, "We've have to look at that 6.1 stuff and make sure it turns into 6.2, and we have a lot of good stuff, particularly in 6.2, that we have to get out." So, those were the two central charges. They were certainly the things that Eb passed on to me and I didn't see him doing anything secret, I assume that whatever peaks appeared to me on the landscape must have been the peaks that Johnny Foster made him aware of.

**I:** The Vietnam War put pressure on ARPA, didn't it?

**Lukasik:** Enormous pressure. From '67, which is the first time I really became aware of ARPA's overall budgets, through '71, '72, it was, "Spend some of your resources on helping with the war, plus we need money for the war, so you have to take a five percent cut. And that's on top of the five percent cut you got four months ago" — (chuckles)— "or the ten percent cut that we're going for next year." So, yes. In fact, one can either argue that it was very challenging—that is, how do you get all this good work done and do it even more elegantly and cheaper and more effectively—or you can get pretty discouraged and say, "They want me to do this and then they cut my budget whenever I have a good idea."

In Washington, there is tremendous infant mortality for new ideas. All the people on both the Hill as well as the OMB—it was then called the Bureau of the Budget, BOB—see that the best way to control future expenditures is to kill off children. So, they are always suspicious of these new things, because these new things with a low buy-in price are going to grow up and become expensive adolescents before too long.

So, yes, there was, and it had an effect. And, in fact, you see, there's another part of this transfer because what we're leading into is the DEFENDER transfer. At that point, we were at the point of deploying a ballistic missile defense system. That was the main thing that was left in ARPA after the space program went to NASA and the Air Force in 1960.

So, we're deploying a system and it was quite natural that people deploying the system should have not only a research arm, which the Army did have, but an advanced research arm in order to address evolving threats. So, therefore, why not take all of this stuff that ARPA's been doing in advanced research and move that over to the Army and become part of the Army system?

Eb immediately set out to put boundaries on what should go and what should stay. We transferred probably \$50, \$75 million and about half of that staff. And that had been the largest office. We lost a big chunk of brainpower and a big chunk of budget. The reason why that's important is your last year's budget is always a starting point for next year's budget. So, when you lose \$50 or \$75 million of budget, you look a bit smaller the next time you go on the Hill. And while you're going to get some of it back, you're not going to get all of it back.

I'm probably coming across as a bureaucrat that wants a lot of money, but, of course, if you don't have money, you don't do the job you've been hired to do. And if you don't have as much of it, you have to shed a lot of things that might be very important. Might look like dogs. Might turn out to be very important.

So, yes. The loss of people, the decrease in size, which then goes back to the staff, getting back to these other issues that we mentioned about the staff is always very, very shaky. Especially in view of its history of near-death encounters that have occurred throughout the life of the Agency until then.

So the staff says, "Well if you give away \$50 million here maybe I'll be next, and that's another 20. If this process goes on, this new Director's going to leave when there aren't any people left in the Agency, and there isn't any money in the Agency." And so it was this giving away large chunks that bothered the staff. One of the things I did was to try to get away from that large chunk transfer process. Giving away, yes, but well, I guess that'll come later.

I: How did you become Director?

Lukasik: How it happened was one of these morale-shattering experiences. In fact, it was probably the worst since the Roy Johnson space transfer.

Because Foster loses his Principal Deputy, Eb was drafted to be the Acting Principal Deputy the same way that I had been drafted for my job at ARPA. Now, that would've been sort of all right, because ARPA was right down the hall from DDR&E, and it's an easy sprint down the hall if something is really important. But ARPA was also forced to give up its space in the Pentagon. Space in the Pentagon was very tight and we were moved to Rosslyn at the exact same time that Eb takes up his new duties in DDR&E. Matter of fact, I don't think Eb ever set foot in the ARPA offices in Rosslyn. That's not said to be critical; that just says that he was there, and we were here.

So, what people have, and they're already smarting over, is this DEFENDER jump, this carving out of a cubic foot of skin. And now they've been moved to as we called it, "Siberia" and they have a Director who never comes around. And although I'm sitting there as a Deputy, and I'm not the Director, but the official Director isn't around, and nobody sees to or talks to the Director. It was a weird year. 1970 was a truly weird year. We had at least three major morale-shattering blows to the Agency.

People felt very badly about that, because the people in DARPA were very bright. They could get a job anywhere they wanted. They simply weren't going to stick around a sinking ship. So, it wasn't just a matter of "the employees are

unhappy." This was a matter of your employees may start walking anytime, and it may be hard for equivalent people to walk in under those circumstances. So, you worry about morale.

The reason ARPA went without a Director was for the same kind of logic that went on with the Peter Franken and me period. That is, Eb thought everyone had assumed that the option was there would be a new Principal Deputy and Eb would come back to ARPA. So, he wasn't giving up his beachhead there. I understood completely, and I didn't have any trouble with the arrangement, but I can imagine that other people would, particularly the program managers.

That's how it happened. After a year, it was decided; it wasn't that Eb's confirmation was held up in any way, but it was decided that, by golly, he was going to be the Principal Deputy DDR&E. It was not unreasonable that they said, "Look, give ARPA to Steve. He's been doing okay, knows the place. Don't want to go for another search." Then it was that I had the question of backfilling. What was I going to do for a Deputy? And, in fact, I did the same thing. I pulled up one of the Office Directors. So that was the story of the transition from Eb.

When you look at ARPA and you look at these various things—turnover project, turnover people, service agents, no facilities of its own, ARPA orders, structure—as you look at all that, what Eb added was a process called "transfer." Eb added the critical output stage to the Agency.

We all recognized that giving yourself away in large chunks did not encourage the long-term purpose of the Agency, because, after all, the Agency can only do its job if people believe in the Agency; believe it's going to exist; have confidence. You'd like to be sure that your roof isn't going to blow away and you're going to be comfortable in your house. And people weren't at all clear that maybe the roof wasn't going to blow away on them. There were some good things done and then there were some things that had to get tweaked and I was the chief tweaker.

I: How did you bring the Agency back?

**Lukasik:** Well, there are three parts to the answer to that question. There's what you do on the inside. Then there's what I would call, to use an overworked popular word, "outreach," and then there's just the passage of time. Those are three different things that happened.

The inside processes in some sense were the most critical, because the people on the inside who were worried are there. They're worrying today. They were worrying yesterday. They've been worrying for six or eight months. So, morale was a major problem.

The way the staff interpreted this series of events is they called Eb the "absentee landlord." They said, "We are so unimportant, that nobody even cares to appoint a Director for us." A Deputy Director only makes sense when you have a Director. If you don't have a Director and if you have a Director who is distracted, absent, doing other things, not coming back, you really don't...

It's very hard. If I had been an *Acting* Director, I probably would have had ten percent more traction, but I wouldn't have had much more traction, because

Acting Deputy is not a strong title, either. It's a placeholder for something better that's going to come along.

At least part of the healing process was, "Well; now we've got a Director." And, in fact, probably the good news was they knew me. The devil you know is always better than the devil you don't. It was a short period and there wasn't any uncertainty because when I was named Acting Director it was also understood that my name was going forward to become Director. So, that at least healed some fraction of the wounds right there. That at least healed "no one bothers to appoint a Director." At least they did that. They probably could have said, "Well, he was the easy choice," and so on, but those are second-order arguments.

I: How did you attack that problem when you became Director?

Lukasik: We had to do something that said, "Look, this place is back on its feet." It was like adding water to something dehydrated. It's supposed to spring back immediately. This was not a three-year comeback sort of thing. You have to work the personnel problems. You have to let them know there is a boss.

I did one thing on the very first day. There was a particularly egregious personnel problem and I came in the first day and called this person to my office and told him to divide his projects up among his two senior people that I identified and to be out of the building by noon. By 10 a.m. if there was any question, people knew they now had a Director.

More than that, this was not a capricious act at all. This was well-based on a lot of track record. It also projected an important ethical principle. To me, it's not just enough that you're operating and operating smoothly and you're making progress or something like that. There's an ethical side to this. Whether it's written down or not, there are certain things that are acceptable and certain things that are simply not acceptable. And it was very important to project the notion of, "You know what's been going on. You probably know more about it than I know and I am sending out, in the most pointed way possible, that that behavior is not going to be tolerated."

It was not just a getting out of an uncomfortable situation; it was making a positive statement about leadership principles and the implied message was, "And I certainly hope that you do the same thing every time you're in that position." That was one of the first things I did.

I had an image for ARPA. I don't know to what extent it has been sustained. In the beginning, in the so-called "Presidential Era" period, a problem would be thrown over the transom. You immediately organize an entity to work that problem. By definition, each problem was basically orthogonal to what we were already doing. Each problem wasn't a little extension of something else; it was something totally new, a totally new dimension. So, it was very natural that you would have a one-to-one relationship between the office structure, the organizational structure, and the particular assignments that you had gotten. Of course, in many business situations, that's exactly what you want. You don't want to have three divisions all in the same business area, so you like things to be separate. Congress likes things to be separated. When we want to buy ballistic missile defense, we know who to talk to. When we want to buy nuclear

test detection, we know who to talk to. When we want to buy command-and-control, we know who to talk to.

By this time, 1971, the Agency was 12 years old. The Agency has grown. There are a lot of good people in it. Even with the turnover, it had a lot of good people, and they're very broad. You have all sorts of people in sciences, in branches of engineering and mathematics and computing and public policy. It seemed to me that this was a time when we could change the way the organization worked. The business literature would call it "synergy" or something like that but it basically said knowledge, skills experience in this area can be applied to that area.

So, my image of the Agency was—and, again, this is a somewhat tired word out of the business literature—"team building." What I really meant was if you're working on this and you know something that can help that fellow you're not supposed to say, "That's his problem and I got my problems." You're supposed to work together.

So, for example, to give you a few cases: the Nuclear Test Detection Office knew a great deal about signal processing because you had to process seismic data in order to pull signals out of the general rumble of the earth. We started to work with the Navy, and this was one of the things that we did, was the Navy had an underwater detection system called SOSUS to listen for submarines that did signal processing. Well, it turns out that the way the seismic people do it is to do what's called "beam forming," it's the same principle as phased-array radars, to look in a particular direction and, therefore, you filter out all the noise around it and you get better signal-to-noise ratio. You may have to do some scanning, but you have of all the possible directions.

So, we wanted to do some experiments because we thought that would very much help the SOSUS signal processing and, in fact, we did. Since this was a massive computing job we actually used the ILIAC processor. So, here we have what was—now I'm a little bit ahead of the story chronologically—but here we have the Tactical Technology Office that is working with the Navy underwater problems, and we have the Information Processing Techniques Office who has developed a very powerful parallel processor, and we have ideas about beam forming and in the processing from the seismic community. That's the sort of thing that I was trying to accomplish. To do that, therefore, you must have, in the management literature, "to do some team building."

We had weekly meetings. And you say, "Well, what's so special about that? Every organization has weekly meetings." Well, it has always surprised me how many organizations *don't* have weekly meetings of the management group. And so the idea was bring everyone together, substantive discussions not management garbage, just substantive discussions. "You're doing this. What are the results?" "You're doing this. What are the results?" People began to see how other people were doing things. They began to see the rate other people were moving. And, quite honestly, while I wanted to do team building, I also wanted to develop a little bit of competition. If these fellows seem to be moving ahead and we haven't had a big result or a breakthrough, maybe we ought to push a little bit to get this thing moving.

There is this dynamic between competition and working together. In any event, the enabling device is to make sure people meet and talk to each other. And more than that, practically everything I wanted to do, I brought up to the office directors and, obviously, the financial people. Russ Beard headed the Program Management Office. I'd say, "What do you guys think of this?" We discussed whether, for example, we ought to take 6.3 money or not. And we went around the table, probably people wrote some memos after the staff meeting. And the group came down and said, "You know, Steve, that's not a very good idea." So, we didn't do it.

I may have been smart enough to come to that same conclusion on my own, but I had seven or eight other very smart people to help.

There was a time when I was again dissatisfied with the organization and asked, "Are we organized right for our future work?" We had an off-site. I had everyone write papers ahead of time asking if you're the Director and you have a free hand in organizing, what would you do?" And so we got a number of good papers. They passed them around. Everyone had an hour to make their pitch, to be queried by the other Office Directors. Out of that came a set of ideas that then guided the reorganizations. I'm not a big fan of the "if you have a problem, the first thing you do is reorganize," but it is certainly true that if you have an organization which has developed in the past and now you're facing a different future, different modes of operating, then one can expect that that should be reflected in some organizational issues.

For example, when we were working with the Navy, the Navy said, "Look, we are very uncomfortable if six different people from ARPA come to us with ideas. I mean we just can't deal with a crowd. We're very busy people. We have our own problems and we can't sort out where each of these people are coming from. We want one point of contact."

I appointed a single point of contact. He was the Deputy of the Tactical Technology Office at that point, Kent Kresa. Did a superb job. "You want to talk to the Navy? You go through Kent. You have some ideas? You go through Kent." He then became the Navy specialist. He knew the entire Navy world by that time. He added value, but from the standpoint of the domain that we wanted to work in, it made the Navy much more comfortable that they weren't getting a bunch of random thoughts.

Those were the sorts of things that we did that I wanted the Agency to do. More of a team approach, and I know that's overworked, but I really had in mind some very specific things. What I was exploiting was the principle of the nonlinear relationship. When you add one brain and one brain, you get more than two brains of output. And when you add eight, nine brains, and when they're all very good, you have an even better Agency than you would have from just nine, separate brains doing their separate things.

I: It sounds like you're working out a new retail model.

Lukasik: That's right. We hear a lot about the customer. In fact one of the management enthusiasms—and I forget what the management literature name for it—was basically that if you have a large organization, which by definition has

a number of separate pieces to it, aside from the customer that whatever comes out of this organization serves, the organization has lots of internal products. One part gets things from other parts and it passes on a product to others.

As you use this particular notion, it says that each sub-group is supposed to explain what it needs from other people. The implication is if you don't need it and they're supplying it, maybe it's a waste of the organization's effort. If you think you're doing something super for these people, and they say, "I never heard of it," then you shouldn't do it.

The idea was to get the Agency working, helping each other, as well as producing its own output.

We saw that we had certain clients. This was a very big jump from the original idea of ARPA. We're going to prevent technological surprise. Whatever's important, we're going to have done it first, or know why it's not a good idea. And we're going to work on these major problems, structure them and feed results into the rest of the national security enterprise. But that's really an oversimplified idea. You just don't have one, super brainy organization that thinks of everything and tells everyone what to do. These other parts of the system have expertise. They have problems we didn't know about.

The second major direction is outreach. The organization has got to have a set of understandings with the larger world. It's not just, "I know physics and I know mathematics and if I put that together I can solve all your problems and I'll mail you the answer."

No. It has to be that you talk to these people not because you're broadcasting solutions to them. They're feeding problems to *you*. Now again, one of the things that you've heard in these various interviews: "Is this a presidential problem?" and, "Do what the White House says," and, "The President is worrying about this and the Secretary of Defense has this as a major problem. Please solve it for us." That may have been the case in the mid fifties, early sixties: ballistic missile defense, nuclear test bans, strategic balance, space. That was the way it worked.

As the presidential problems dried up, we worked on self-generated problems. Self-generated things are very dangerous because they can also be called sandboxes and pet and other uncharitable names. You have to be in equilibrium with these larger systems outside you. It was very important to get outside, especially when the problem wasn't easy, like you simply did what the President or the Secretary of Defense told you to do.

Well, if you look at presidential problems, a lot of that was really Eisenhower administration talking. Eisenhower, having been a General, knew what was needed whether the Defense Department recognized it or not. When they departed the scene there was some continuation because the Kennedy administration was quite interested in the counterinsurgency problem. The nuclear test ban issues clearly did not go away. Ballistic missile defense issues didn't go away.

So, the presidential problems had staying power but that was no longer a source of new problems. Therefore, ARPA through the sixties transitioned into this thinking of its own problems but then that makes it very important that you

talk to the right people, and the right people are people outside—not just inside. So, outreach was very important.

I: Now you aren't getting any input?

Lukasik: Right. You're not getting input, and you're giving away pieces of yourself. Going back to the morale issues, I should have added when you asked me to explain the dimensions of that morale problem, the DEFENDER transfer smarted. First of all, people could say, "Yesterday it was DEFENDER. Tomorrow, maybe it's me. I don't want to be transferred to Service X, or Service Y." That was a necessary move, but it had a real downside.

When the guy who was identified with it is no longer in the picture, it looks like maybe he cut and run. This is not the way it was, I can assure you, but these were the perceptions and people tend to do worst-case thinking, especially when it's about their own career.

I: You said a "build and rebuild around sensible, safe themes."

Lukasik: Yes. There are a whole bunch of things you can do with "safe," but they are dull too, and are hardly worth doing.

So we were in the counterinsurgency business. We did some good things. There was a lot of good hardware stuff: the foliage-penetrating radar at Lincoln Lab, we did some small arms work, put the finishing touches on the M-16, which is still the standard weapon, and there was some very useful work done by RAND to interview Viet Cong prisoners to find out about counterinsurgency from the insurgents' side which is a lot better than sociological and political theories and historical analogs that may or may not be applicable. But, basically it was a hard problem. Every once in a while, people toss you these DARPA hard problems and they turn out to be too hard for even DARPA.

Insurgency is a political thing, so it's not something that for which technical people with technical solutions, or technical models, or technical modes of thought are necessarily helpful. They very often turn into nasty military operations, and when you have a military operation, this is not the time to invent new theory. You work on the accumulated theory, the military wisdom of hundreds of years, or maybe the recent decades, and what you're equipped to do, trained to do, you have doctrine to do. That was one that we decided to move out of. And by that time, Congress was getting impatient and they certainly helped us on the budget front to get out of it.

I: I'm curious what kind of impact that had on the perception of the ability of ARPA to handle problems.

Lukasik: Well, while you're in the process of failing you're certainly not looking very good. This is not to say that other people succeeded brilliantly, but somehow or other we managed to become part of the problem instead of part of the solution.

It's one thing to try things and they don't work. It's another thing to take up people's time. For example, one set of initiatives had to do with nation building, something that we're hearing today also. But when people in the United States

talk about "nation building," it's someone else's nation, and they have some ideas about this, and so in many cases, we were doing things like talking about the relationship between hamlets and the central government.

Well, maybe the central government, they had their *own* ideas on how to manage hamlets, and so that a lot of things that are from a top-down view seem to make a lot of sense don't make a lot of sense in a foreign country. They may be superb ideas, except that the incumbent government doesn't like them. That's the relevant observation, not how good the ideas are.

AGILE had a systems approach. Systems thinking is very much a Western, technological kind of thing. It's our culture of analyzing problems, understanding relationships, interfaces—all that kind of stuff. Works great when you're designing an aircraft carrier. It doesn't necessarily work great when you're designing a country and so it was a brave experiment. It's just not the way the world works. It did some good things. None of these things are all good and bad, but in balance, it was better to move on.

I: "Social science and political science aren't nuclear science or rocket science."  
Lukasik: (Laughs) That's right. That's right.

In fact, there is even a big divide between social science and political science because social science tends to be theoretical, may be data-driven instead of model-driven, but the point is that it is data and, of course, we all know that politicians and political structures have their own ideas, their own incentives, their own ways of doing things, and if the data gets in the way, it's soon sent packing.

I: Now you had to rebuild a reputation.

Lukasik: Right. We did two things. Let's start on the tactical side, because AGILE is a part of that.

We had troubled offices. We had the AGILE, whose name has changed to Overseas Defense Research, partly to hide the name "AGILE" and bury it someplace. And we had an Advanced Engineering Office that was the home of several new ideas, but sometimes one could not quite figure out what the military problem was.

Then there was the Advanced Sensor Office that really became a hardware office for Vietnam. Some very interesting things were done by the Advanced Sensor Office. We heard recently about a remotely piloted vehicle that was able to kill an important terrorist. Well, we were doing that with drone helicopters. The Navy's Dash helicopter that we used for ASW and we were doing the same thing. Unfortunately, they were old and most of the flights terminated in a crash. But we were doing the same thing that 30 years later was seen to work. So, there was some good stuff there. In that case, the technology wasn't up to it, or, maybe the systems engineering wasn't up to it. We put these three offices together and we called it the Tactical Technology Office.

We had maritime stuff, which grew out of some of the advanced engineering things, plus other initiatives that we started. You put things together. To use a signal processing analog, the good ideas rose out of the noise and

some of the less successful stuff just became noise and faded away. The Tactical Technology Office started with a budget base of three offices, so it had money. It could slowly work the losers out.

Now, we had to do the same thing in the strategic area. We had a DEFENDER program, and it was doing both offense as well as defense. It was doing warhead decoy discrimination; it was building radars, doing signal processing to do that. It was also in the penetration aids business, U.S. penetration aids, as well as understanding Soviet penetration aids. Well, when you pulled all that ABM stuff you were left with, in the words of the Office Director Dave Mann, "a bunch of cats and dogs." And it was things that together made sense. When you pull out the head of the family, as it were, you have minor pieces left. So we had to rebuild that. And Dave Mann did a superb job. We still had the PENNAID stuff, as a matter of fact. We also had the high energy laser programs though by that time we were losing confidence in it as an ABM device. The idea was resurrected during the Reagan administration's focus on the Strategic Defense Initiative but it didn't work any better there than our thinking back in the early seventies.

So the renamed Strategic Technology Office went into space object imaging. When you go into the hospital and get a CAT scan, the x-ray unit takes pictures of you from different angles and when you put them together you get a three-dimensional view of whatever it is that they're looking for inside you. Well, it turns out when you have a satellite in orbit it shows different aspects and if you keep observing it with very high-resolution radar you can get the same effect as you do in a hospital CAT scan.

We could build up three-dimensional images, virtually produce a photograph. Okay, a two-dimensional image of a three-dimensional object. That's a projection onto two dimensions of a three-dimensional object. With this, you could actually build a three dimensional image.

Now, we were very interested in what's in orbit. We worried about anti-satellite devices. We were obviously interested in what the other guy has so we could figure out what he was finding out about us. There was, for example, one time when the solar panels failed to open up fully on one of our satellites. It wasn't going to work because it wasn't going to get enough power the way it was. We were able to image that satellite and determine just how far and which one was stuck and where it was and so on. It's very useful on your side. It's very useful on the intelligence side. It was part of newly developed strategic issues as opposed to ABM. We had to build up in the strategic area, and we had to rebuild in the tactical area, and along the way, turn some lemons into lemonade.

In the Nuclear Test Detection Office, the original formulation back in the early sixties was, "There's going to be a nuclear detonation and we must detect it. But must also be able to discriminate between earthquakes and explosions. Thus if we sign a treaty we will know if they're cheating."

When you think about it, you really want to know much more about it. How big is it? What's the yield? Is it a multi-stage device? What can you learn about the technology from this teleseismic signal? So, you get into diagnostics because that's why are they doing what they are doing.

If you see a test, it's very different if it's a brand new, revolutionary device, or whether it's a test to see why something in their inventory doesn't work. The missile "detection" test is much more than the simple matter of distinguishing between explosions and earthquakes. We moved in that direction also. There were really some important new things to be done and it was, of course, supported by the intelligence community and by the nuclear weapons community because everyone wanted to know where we stood in the strategic balance. So, we had a good deal of rebuilding to do.

Another aspect of the rebuilding was the "6.1 offices." We had the Materials Office, the Information Processing and Techniques Office, and the Behavioral Sciences Office. If people were worrying about output from the 6.2 offices what on earth do you say about these 6.1 offices? And they were an extreme form of Foster's concern over ARPA as the Defense Department's National Science Foundation.

Well, they weren't. They were really doing very important things, but as long as you called them "basic research," you just weren't going to see any "rubber on the road" output because basic research is knowledge, not development. That seems to be contrary to the notion of grab a problem, hit it hard, and move the solution out the door or, at least tell people that solution won't work, and everyone is the wiser for that also.

What I did was I said, "Look, you guys know all these good things. How about we move some of that closer to things that people can understand?" And to do this, we formed some new 6.2 budget line items. 6.2 is called "exploratory development." The "development" word is important.

What did we do in the case of computer science? Well, they knew a lot about path planning for robots. Robots do very important things. In fact, what is now called the Future Combat System is basically major robotic additions to Army forces in the field. They knew about that artificial intelligence. I posed the question, "Why don't you guys figure out how computers can understand speech?" We established a speech understanding program and now, although certainly a lot of us struggle with absolutely stupid voice response systems, the fact is that very often they work. Now when you say, "I want to go to Chicago," to a computer, it really knows that you want to start looking for airline flights from Washington to Chicago.

Consider a behavioral science example. As you probably heard in these early interviews, all this great stuff we did in computing and networking goes back to the vision of a very brilliant man, actually not a computer scientist but a psychologist, J.C.R. Licklider. When brought in by Jack Ruina to work on command-and-control, Lick said, "Well, there's two parts to command-and-control. There's the processing of the information that you need to do the commanding, and then there's the how the commander absorbs this information in order to issue commands." He had both parts of the problem in his mind, in the same office.

After Lick left the behavioral scientists said, "Well, we'd like to have our own show." The Information Processing Techniques Office pushed the hardware and software side of Lick's vision but the human understanding, interfacing,

decision making was broken away from the project and was now in the Behavioral Science Office. The Behavioral Science Office didn't really do that; they had their own agenda, their own things that they wanted to do, and that happened not to be one of them. One of the things we did in the behavioral science area was, first of all, back them away from foreign-area research, where they were both getting in the way of anthropologists and generating the suspicion that these anthropologists weren't really anthropologists; they were really CIA people who were recruiting in the halls.

We set up a 6.2 program element on decision-making. How do people really make decisions? And it had some good technical aspects to it. There is a branch of study called "decision theory," Bayesian statistics and how you work your way through multiple connected probabilities. Some of that decision theory work had its application a few years later. The U.S. was negotiating with Panama over ownership of the Panama Canal. Panamanians had their ideas. U.S. had their ideas, and so on.

Using some of the decision theory techniques, working hand in glove with the negotiators, showed that you could achieve what was called a Pareto optimal result. It's the maximum positive for both sides, as opposed to, "How can we get the most and to heck with them?" and the other way around. So, it was a way of defining and structuring and asking questions and getting weights and so on, so that you could then say, "We'll all get further, and we'll all be happiest if we go after this and we not get bogged down in this other issue."

Another example is the work we did related to the all-volunteer force, something DoD adopted around 1972. You can staff your military if you draft people. Now, how does the Defense Department work its military staffing without a draft? It's much more like industrial personnel management. Well, that's not the way military forces work, and that's not the way the draft worked.

We said, "If you think your way through this problem of recruitment and training and selection for tasks, promotion, structuring incentives and so on, there would be cost-effectiveness tradeoffs."

We're backing away from these "Lord knows what you people are doing" to "You people are *helping* us." So that now you find people in the Congress and in the military Services and in OSD, DDR&E enthusing about the behavioral science program that in the mid to late sixties, no one could see any virtue.

Basically, it was inching your way back when you were way out on a limb to nearer the main trunk, working together on things that made sense to the customer, that weren't just trivial. They were still doing things that were not natural to the bureaucracy and military folks. It was where the civilian, bright outsiders came in, but it wasn't brilliant, bright outsiders coming in doing their own thing. It was brilliant, bright outsiders coming in with a very clear understanding of what the department needs, meaning what the military Services need, in the final analysis.

**I:** Almost a door-to-door salesman....

**Lukasik:** Uncharitable, not off the mark.

I didn't have to do it myself. That's what office directors do, but the point

was, just to use that analogy, I was the one who said, "Hey, guys, you all have to become door-to-door salesmen."

Now, I was still there to say, "That's a dumb idea. That is not something that's going to work. It's going to cost too much money. You're going to get into a heap of trouble." I'm saying, "You're just not sitting in an ivory tower, thinking of these wonderful things."

The end of the story is successful. Take the McNamara wall, one of these other examples of technologists solving a problem. This happened to be an invention of the JASON group, a very good group, and made all sorts of sense. They said, "If you have a war in South Vietnam, an insurgency, whatever you want to call it, and it's being supplied from the North, what we'll do is instrument those supply lines so as soon as we see the stuff moving into the South, we'll know it, and we can stop it right there." Sort of a current immigration-border control notion, "We'll seal the borders, and then we'll just work on what's inside, we'll attrite that in some way, but at least they won't have an outside source of supply."

This gave rise to the notion of sensors, sensors connected to computers, and then you would manage your military response. The way walls work is you never build a wall that nobody can get through. You build a wall that slows people down, with enough sensors so that people can then rush in. That's the idea behind the spacing of towers on the Great Wall of China. That's where the soldiers were. When they saw someone coming over, they stopped him before he got over the wall. It didn't work that way in Vietnam but it's a description of the current instrumented battlefield. So, it wasn't fundamentally a bad idea, but it was an idea that was not going to work then, in that place, with that technology, with our then level of understanding of insurgency.

I'll just give you a humorous example. We have these sensors, seismic and acoustic, and optical sensors. A sensor would detect something. The military would say, "There's a bad guy there," and they'd go kill the sensor. Instead you use the sensor data to build a track and predict an intercept point based on when response forces can get there.

It was called as a cover the Defense Communication Planning Group, but it was an experiment, and it was a good idea. It basically has worked, but it takes a while to develop doctrine, understanding, sensors, and algorithms. That was another case where you just had to back away from these things, that they weren't quite right. But the lessons were learned and have propagated into the world we have now.

**I:** Did you operate within the Services at an appropriate level to break up the ground to make your job easier?

**Lukasik:** No. No. It was rather the opposite. I said, "This is what you guys have to do because I can't do all that. I'm only one person, and that's not the only thing I have to do."

So, no. I was the one who said, "That's the way we have to operate," and then I have all of this wonderful, multiplicative effect of not only of my bright office directors; they each have bright people working for them. So, no. In fact, it's a

push-down of "this is what we have to do." Of course, we always had in ARPA military officers. And they were brought in because not only did they know the problems; they knew who to call. If you have an office of ten people and it has three military people, you'll never cover the entire world of military operations. But the point was they knew who to call.

If they said, "Look, we have to go talk to TRADOC," or, "We have to go talk to U.S. forces in Korea," or "We have to talk to EUCOM," or "We'd better go out to CINCPAC" I found in this outreach process the Unified and Specified Commands was a particularly useful level to work at. And there were several observations made by some of the gray beards who were assisting the government in setting up. They said ARPA would really be very good as the R&D arm of the CINCS.

Now, each CINC is an integrated organization, it has Army, Navy and Air Force components, and it has a regional focus— Europe; Indian Ocean; Mediterranean; Atlantic; Pacific. I found them to be a particularly good organizational point of contact.

Having all three Services, they had semi-solved the problem of getting the three Services to work together to conceive of appropriate responses to whatever might happen. Therefore, they understood the problems of response, and unmet needs, and they had the political insights of their region. The Commander-in-Chief of the Pacific, having responsibility for the entire Pacific was really a major diplomatic post. Quite apart from whatever forces had to be deployed and emergencies attended to, port calls and, joint exercises and so on.

When you went to EUCOM, you were in the NATO central front, a major land battle with the Soviet Union. In the Mediterranean, you were dealing with the entire set of actors and actions from the Middle East, Northern Africa, and entry to the North Atlantic. So the CINCs were a very important part of thinking through future technologies and contingencies.

There was another part of this outreach. Now, you were asking if I did that myself. There were a few things I *did* do myself. My general concern was that we didn't have a sufficiently broad set of contacts and problems and work across all Service interests. You don't have to do all of them, but while we were very deep with the Army in ballistic missile defense, basically the artillery side of the Army, there were domains such as tank-antitank, aviation and the air-land battle we were not examining. We were with the Air Force in strategic offense, but we weren't in with tactical operations. With the Navy, we had *practically* nothing. It was that Navy direction that I wanted go, because at least we had a foothold in each of those other areas that I wanted to broaden, but in the Navy we didn't even have that.

We ended up talking about SOSUS and signal processing and undersea warfare. The Navy's long-standing concern about anti-submarine warfare was almost totally focused on acoustic techniques, because those are the signals that travel the longest distances, but there was a growing interest in a class of phenomena called non-acoustic ASW. It's still fairly sensitive but there is a lot more to what a submarine does than just radiate noise.

It was one of those new things and almost any effort that could be applied

was appreciated. The JASONS turned out to be quite important on this one. They did some very good work. We developed good contacts with CINCPAC. We set up a study group for CINCPAC. This was the Noel Gyler period. He was a very bright guy, asked *all* sorts of questions, good questions.

I personally would go out for two-and-a-half-day working session/seminars. These were not the ceremonial visits, where the Admiral welcomes you and each of the staff elements gives you the standard briefing, then you have lunch, and they send you on your way. These were really "here are our problems, what's to be done," and then we would go back and see what we could do to better structure the problem. These were only 3-4 week efforts. When you do that repeatedly, you really develop a different kind of a picture than you get from talking to people in Washington or from reading.

The submarine people turned out to be an extremely interesting group. One of the things we noted was the submarine people did their own field experiments. They had ships, aircraft, hardware, and they tried things. So, there was an R&D world out there that you didn't see by working through the Navy laboratory structure.

The other thing is they often had some very special time-urgent needs. We were often able to put together devices that they found quite helpful in the near term.

Now, the net effect of all of these contacts, and I've only mentioned a few of them. If I could round up all of the office directors and they were all sitting around here for the rest of the day we would talk about similar war stories, of how they did this and that with people in the field.

Two things come from this. First of all, military Services, like any group of people, talk to each other and so when someone makes an anti-ARPA crack they can say, "Hey, those guys are good. They were out here, they did this, and they took care of it in three months."

It's not a PR campaign because that's just fluff. That's just putting the words together. It's getting things to happen that really helped me. You do that for a couple of years and you do it broadly. While you don't cover everything, you end up changing a perception of "these guys are elitist, naïve. Their ideas don't work." "They have their own theories." "They don't really know how the real world is." That's what you have to overcome.

So far, I've mentioned Admirals and Captains. But, of course, those aren't the only people you deal with when you go out to any military organization. You deal with a lot of junior officers. Maybe they're the briefers. Maybe they're the people who are doing something that has to be understood, that you're watching and asking questions of. Maybe they're your escort officers. They don't want to look stupid, so those are good people they put up front. You meet them. Eight years go by. Guess what? They've had two or three promotions. Those Majors are now Captains and those Lieutenant Colonels are now Colonels.

As you're building for the future you're not just trying to change perceptions quickly, you find who the good guys are. The good guys get promoted preferentially, and guess what? You know them and they have a good impression of ARPA.

I had named Kent Kresa to be our single point of contact, and we were in a number of very tight Navy boxes. The phrase in the Navy is "behind the green door." We could talk very freely with them, because we had their clearances. Not only were we inside, they had let us in. Operational matters are the essence of it. It's nice to do R&D but they are a bunch of people in laboratories pushing the same kind of paper we're pushing in Washington.

You end up in the Officers' Club in the bar. You absorb a lot of good information by just sitting around with a bunch of guys including some alternate views that may not have been fully presented during the day.

And it's not just a Ph.D. developing mathematical models and explaining about nuclear structure. This is the world of in which people are trying to do things and after a while, they begin to notice that you're on their side.

That was all part of this general changing the perception. It was the ARPA that grew up T equals zero. Several directors fleshed the basic concept out, Cy Betts and Jack Ruina. Then came the mid-range of development, Bob Sproull and Charlie Herzfeld. Eb Rechtin was brought in by Johnny Foster who said, "It's not quite right, fix it." Although Johnny didn't tell me to fix it, I certainly knew what he meant. Going back to one of your earlier questions, I am a student of history, including organizational history. I had absorbed a good deal of what had happened before I got there.

I was a partner of Eb in the fixing process. It was a question of continuing the transfer of programs to the Services including the 6.2 pieces of 6.1 offices. Senator Mansfield, with the Mansfield Amendment, wrote into the DoD budget that everything in DoD had to have clear military relevance. I was in charge, under Eb, of putting together our relevance statements for the Congress that year.

Well, along the way however cleverly and sincerely and intelligently you defend what's going on, you do notice that there's a few things that are maybe a little bit further out than some of the other things that you're defending. There were some things that showed sign of being permanent parts of the landscape. We weren't supposed to have permanent things. We were supposed to get things started, solve the problem and move on to the next thing. One of these was the Materials Science Interdisciplinary Laboratories. Another one was the Arecibo radio telescope—superb ideas, good work done, but for us, after six or seven years it was time to find them a permanent home.

Well, one of the fallouts of the Mansfield Amendment was a one-time pot of money added to the National Science Foundation budget, so transferred projects from DoD were not something they had to eat; they were given incremental money, the best kind of money that you can have in Washington.

I managed the negotiations with NSF, and we transferred the Arecibo radio telescope which is still in operation. But this is now 40 years after it was built. It was a fine scientific instrument. Still is.

The material science laboratories were 12 of the best universities in the country doing interdisciplinary materials work—metallurgy, chemistry, physics, interdisciplinary works drawing on the entire set of academic disciplines.

It went back to letting some things die, restructuring some things, talking

to the customers, terminating things that were just not going to be successful, transferring things that were very good there but looked less good here. Some of it was opportunistic. Some of it was a matter of luck.

I: A critical turning point to ARPA. If not rebuilding, building a raised level of competence in the Services, ARPA looking at some short-term solutions for the Services in order to build long-term relationships.

Lukasik: Yes. Although even some of those short-term solutions would continue to work for as long as they were useful. It was very much a mixture.

ARPA wouldn't be very interesting if it was just another short-term solution house or just a bunch of itinerant geniuses. Yes, it had to work on important, long-term problems and map out new and long-term solutions. But sometimes long-term things come from relatively short-term things. Let me give you two examples. The ARPANET had several rationales. It was networking computers. It was sharing resources. It was sharing databases. It was sharing software. There were very good reasons why you wanted to break away from the idea that your computer is in the computer center and replace it with the notion of your computer is anywhere in the net, whichever one is available and best for the job. Along the way they *extended* e-mail so you could talk through the ARPANET to the local e-mail systems of all other universities.

What IPT found was a long-term effect, which is computer science moved faster. We not only supported computer science, and maybe Lick understood this in the beginning and I was a little bit slow, but the fact of the matter is that we ended up building a long-term thing that made the whole enterprise go faster. The other offices noticed that and they started building the same internal communication infrastructure: the seismic people picked up the idea of putting seismic data on the ARPANET and moving it around that way. The other offices saw that I talked more to the offices that were on the ARPANET, so they wanted to be on the ARPANET. Once they got on the ARPANET they started to push their people and their contractors on it so it was a cumulative thing.

Another example where you get some long-term effects from some short-term things is in the VELA program. Even before I arrived in ARPA, the Worldwide Standard Seismograph Network had been established. The problem is that it's hard to compare one seismogram to another seismogram if the two instruments have technical differences in their frequency response. So, we had designed and installed about a hundred and fifty identical instruments around the world.

But then we build the large-aperture seismic array, which was to test beam-forming ideas I was talking about that later got applied in the SOSUS area also. Then, we built a second improved array. The second one was in Norway. Why Norway? Well, because from Norway you could not only see Soviet explosions at teleseismic distances, it could see U.S. explosions. The large-aperture array in Montana was too close to the U.S. test site to be used teleseismically against explosions. The one in Norway was better suited for that. We built other smaller arrays called SROs, seismic research observatouer.

Then we were now using the ARPANET to bring all this information

together into a central point, the Seismic Data Center in Alexandria. We built in Alaska, jointly with one of our cooperating organizations. So when you got all through, while we didn't really sit down and build a network, we ended up just like the IPT office. Didn't really intend to wire the world together into the Internet but that's what happened. And we wired the seismic world together. It used to be you would mail in your tracings every day or two and so it would take three weeks before all the data came in.

This isn't to say that all limited, shorter-term things always have long-term consequences, but in many cases they really do plant seeds. Good ideas are replicated. The users net themselves together and larger systems evolve. There's not only a cumulative effect of smaller things but in many cases there can be nonlinear effects, so that the cumulative is more than a simple sum.

It was a different ARPA under my period than under Eb's, but that's because Eb was addressing the transfer process and focusing on the difficult counterinsurgency problem. I learned a lot about the transfer issues, and trying a lot of things and being willing to cut your losses.

l: You made the machine leaner, is that a fair analysis?

Lukasik: It is. This is essentially my story. Now, there's one question you might ask, which is, "Was that a plan?"

Well, yes and no. Let me give you the "no" part of the answer. It's not as if I walked in on day one, prepared to be the Director. It's not that I put all this together and sort of went through the book and said, "Okay, week one and two, we do this, and in six months, we're going to do that." No. There was nothing like that.

I had a sense that everything that had gone before, the ARPA before I got there, plus my time under Charlie and then with Eb. It was a pragmatic process, "These are good, and these really work, and these are great. This doesn't work, and that's just a loss that is better cut sooner than later." These were all principles that were in my head, but I probably could not have given a coherent lecture, even, much less a plan on what was to be done. Some of it was opportunistic, like the NSF- Mansfield process, purely opportunistic.

I have to say that in my terms, and I think even in Eb's terms, it was probably good that Congress leaned on AGILE because we might have let it go on a bit longer, hoping that there would be "light at the end of the tunnel," a favorite phrase in those days.

So no. There wasn't a plan, but there was an integrated set of pragmatic principles. It was like the personnel story. There were certain ethical principles that define the way you operated. You were backing away from the end of the branch, where you are too far out, into something more solid, things that fit together. The ARPANET networking idea, growing to the current interest in "netcentric warfare" and terrorists as a "distributed social network," is such a pragmatic principle. It's more than just a technical capability, computer protocols and bandwidth and so on.

The idea probably was unconscious but can be brilliantly explained now. I can't say it was a plan that was superbly executed. It was just this matter of,

"This is what ought to be done." "This is not what ought to be done." "This is better." "This is better than that." "These are best not explored for technical reasons," or political reasons, or bureaucratic reasons, or whatever.

There was another component to the technical work which was certainly clear to me, and it was clear to all of my predecessors. That was the political side. Now, I don't mean Republican-Democratic politics, or doves and hawks, or war-anti-war politics. I mean that we're scientists and technologists. We're operating in a world and most of that world is not people like us and, therefore, we'd better understand it. You couldn't do test ban technology without knowing why people wanted to build nuclear weapons, where they were in their thinking—what would happen when they had them, what would happen when the Soviet Union had them, as opposed to some other country, or countries, that would have them.

You had to understand the economic setting. Now, of course, in the terrorism business, if we ever had any doubts about whether we have energy problems, we certainly know that whatever they are, they've gotten a lot worse. The political, social, cultural state of affairs in the areas that hold the greatest part of the world's energy reserves, oil reserves must be understood as a basis for informed policies.

That was also part of the thinking. It was not, in my mind, and in the minds of all of the people who preceded me—it was not a question of what's the science, what's the best scientific solution. It was never that cold kind of a thing; it was that component in a much richer world, and a world which is non-technical, non-quantitative. But the point was to understand that when we were doing our thing, it wasn't doing our thing in our world; it was doing our thing in *their* world, whether it's the U.S. world, in specific countries.

When the Agency was established it happened that the enormous problems in the late fifties, sixties, the beginning of the Soviet buildup, the Cold War, and the strategic confrontations with the Soviet Union, the domino theory, those problems had large technical components.

As time goes by, many problems don't have such large technical components. They have major social, religious and political, cultural aspects. Things are different now. You have to be in equilibrium, as the physicists would say, with those forces. At least you have to understand that those are major factors. So, for example, in the AGILE program, the non-technical components were much greater than the technical components.

It's important for the technical person to know when technology won't "solve" the problem, because otherwise you can waste a lot of your time and a lot of other people's money and get very, very frustrated working out these wonderful technical solutions that simply aren't going anywhere.

**I:** Your tenure saw the turning point of ARPA big-issue project.

**Lukasik:** I used to fund studies, where U.S. and European political scientists would meet and discuss theater nuclear issues. I would attend to listen. I wasn't there to talk. It was their backyard that we were worrying about. I wanted to listen to them.

There is a difference between reading and going to a meeting and talking to them and listening. In the technical jargon it is called "kicking the tires."

Well, the equivalent of "kicking the tires" on strategic matters is going to such meetings. I called it "understanding the requirements" for ARPA research. The requirements aren't always what the military Services tell you they are. They've abstracted from that world, and now they're ready to tell you what the technical requirements are. You always want to go around and look behind the curtain, see who the wizard really *is*. But you're *learning* something. You're learning it through seeing. That was another part of the outreach story.

I: You retooled JASONS, too.

Lukasik: Yes, we did. There is a new book out on the subject by Ann Finkbeiner. I provided her my files on the subject.

The JASONS were a valuable group founded in the early days of ARPA. While what they had done was very good, the evolving ARPA had new needs. JASONS are still in existence fifty years later. In the sixties, it was the physicists who were interested in these larger, national security issues.

What I noticed was ten or twelve years later it was the same JASONS, no better, no worse. And it was very good to start with, so that meant it was still very good, but we found that they weren't in the materials business. They weren't in the computing business. They weren't in areas that were important to us.

I said, "Look, this is not a lifetime membership organization. Ought to be fixed terms and you ought to renew them if people really work. If they don't work, if they don't come to summer studies, you should replace them. We ought to talk about what skills ought to be there. We won't tell you who should be a member, but we'll tell you, 'You need more of this and more of that discipline.'" And so a long set of events is all fully laid out in this recent book on the JASONS. And so we ended up essentially taking this group, not reforming them, but renewing them for the next ten years.

My model of myself would probably be a guy with a leather tool belt with hammers and wrenches and pliers saying, "Oh, we have to tighten this," and, "We'll tear this out and put this newer thing in and, oh, by the way, why don't you build something over there?" I would say it was more a question of constant tinkering to improve matters but in the end had some very important results.

I: By the time you left, did you feel that you left it a healthier organization?

Lukasik: I think so. If you read the history of ARPA that Lee Huff and a colleague did, you are impressed by the number of near-death experiences in the Agency even before it was started and when Eb came in 1967. That's a lot of close calls for an agency that's supposed to have been the solution to our problems. But every year or so, someone's trying to shoot it in the head.

My only measure for answering "yes" to your question is it hasn't had as many near-death experiences since 1975. Not that it hasn't had any. In fact, one of the questions I asked you was, having talked to people, what problems others had, and you mentioned one. But, one in 30 years is a lot better than one

a year for the first ten. And so from that standpoint, it's really the only metric I have that it's, perhaps, in better shape, it doesn't seem to be facing these issues of, "Do we want you?" "Do we need you?" "What do you do for us?"

But there was one other thing, going back to when I said the things to be done. There were two things to be done simultaneously, the morale issue, the internal issue; and the external, the outreach issue. But there was a third factor in it, the passage of time. And I've laid some foundations by mentioning it.

One of the reasons why we were better accepted in 1975 than we were in, I'll use Eb Rechtin's arrival date, 1967 was that seven years had gone by. Eb had accomplished things and I had continued that process. I've explained a number of things. The ARPANET, for example, was everybody's darling, not just the academic researchers, not even the seismic people or the acoustic people; military people. The first 20 ARPANET sites, there were a couple of think-tanks, and maybe one military organization. The growth through '72-'74, enormous, saw a number of military organizations joining it.

Now, when military organizations joined it, their people had to do the work. It was their money that had to buy Interface Message Processors. So, it was really shelling out money and devoting manpower resources to it. They did that and they liked it. That's not a matter of PowerPoint charts. That says a lot of people have voted with their feet, and they've moved to this idea.

Both the pro and the anti test ban people said, "By golly, this organization's really helped. They really did the science. They really laid it out. They really didn't color anything. They really didn't cover anything up, and now we can all understand the risks of what we can and can't do in further restricting nuclear weapon development."

We had worked closely with the intelligence community, with the Navy, with all the Services. People could say, "Yes, they really do transfer things." "Yes, they really do close down losers." "Yes, they really do move things out, just like the rhetoric says." There were solid accomplishments you could point to. I think that the effort that Johnny Foster started when he recruited Eb and pointed him in certain directions, and when I was lucky and able to participate in that process, paid off. There was enough that went on that we didn't have to spin clever arguments and make charts. There was rubber on the road.

**I:** Do you have any regrets as you look back, like, "Jeeze, I wish I'd done that," or, "I wish I'd put more energy in that?"

**Lukasik:** I may think of a better answer tonight. I think the answer is no. I wouldn't do anything differently. That is, I haven't come away saying, "If I had only gotten this started," or "hadn't done that." This sounds sort of self-satisfied, perhaps, but, no. I can't think of anything. I pushed people. "I want this," "I don't want that," "I want it this way," "I don't want it that way." But it's the people in ARPA who do the work.

So, in a way, perhaps, one might rephrase the question and say, "Where didn't things turn out as well as you might have?" In other words, "Where did the people, the office directors and the program managers do less well than I would have wanted, perhaps?" And even there I have to say no. They struggled. They

had problems that I never even knew about. I have to come away saying I was pleased, proud of what the team accomplished. I felt like a football coach who's just won the Super Bowl. I was proud of my guys.

Some of the hardest experiences before you leave are the various dinners. We had a dinner in a private dining room with the office directors. I went around the table, and I spoke to each one of them as to what they had done. It was really a very emotional time for me, because it was me saying, "You guys executed well, and if you didn't do something quite right, well, I understand. I didn't do some things quite right, either. But as a team, we did very well."

No. I came away pleased with my time in ARPA. And, after all, it wasn't that I failed to build the great ARPA in the sky. I could only build the great ARPA that was needed at that period and that had some survival potential. I think I was pleased, quite apart from any technical decisions that were made; that ARPA has done as well as it has because if I had fumbled the ball there wouldn't be an ARPA today.

If I hadn't gotten some of these things going, it would've been less strong and it might have succumbed at the next epidemic. In the 30 years after I left, someone would have gotten it. But that also means that all the directors that followed me didn't fumble either. I've known all of the ARPA Directors except Roy Johnson. And I'm sorry that I can only read about that one.

There is one last story I'd like add, though, as we think about these long-term directions. There is one thing I don't think I talked about this earlier in the interview or the previous ARPA interview.

The matter of smart weapons, the Long-range R&D Planning program. We established the program but we didn't want a lot of kibitzers. We were obviously sensitive to the Services saying that we were getting a bit too far into their britches.

We decided we wanted to have a little bit of breathing room so we called it the Long-range R&D Research Program. You can't pronounce the acronym, and if you can't pronounce it, you hardly remember it. We laid a foundation for what was one of the brilliant things that happened under George Heilmeier.

George was able to take a lot of good things and put them together in a program called the ASSAULT BREAKER, which was the first time that the Services saw in hardware of what you can do with computers, precisely controlling munitions that would take them to the target.

The speech understanding work is another example. I went to Lincoln Laboratory and listened, with the IPT director, to their program. At the end of the day I said to them, "Why don't you figure out a way for computers to understand speech?" The first response was, "That's pretty hard." They had a large meeting in April and decided, "By golly, this idea's good enough to have a summer study."

So, then we had a summer study and that started the area of speech understanding, voice response systems, and, unfortunately, some of the more aggravating parts of the technology. One of the things I had in mind was the COMINT process, because if you look at the way that process goes, it's enormously labor-intensive.

While many ideas come from outside, there is an important internal

process to sort and evaluate. The point was it just wasn't top-down management. If you set up conditions for creativity and synergism wonderful things happen. It's getting these bright people. That's really what the Agency is and I'm sure that's what the Agency is today. And I'll be interested to see how my successors built on this and how they continued to adjust the structure to meet new national security challenges.