**Dr. Robert Fossum**
**1976-1981**

*Interview: March 14, 2007*

**Interviewer:** Please state your name and tenure at DARPA.
**Fossum:** My name is Robert Fossum and I was the director of DARPA from December of 1976 until July of 1981.

**I:** Quite a bit of time.
**Fossum:** Well, very interesting times.

**I:** How did you become the director of DARPA?
**Fossum:** I came there because of Bill Perry, who was the Under Secretary for Research and Engineering, USDR&E at the time. I'd known Bill since college. He was a math professor where I went to school. In spite of that we remained friends, I worked for him at the Electronic Defense Laboratory in Mountainview, California and ESL, Incorporated, a company which he was the driving force behind. When he became USDR&E, he asked me the question, "Would you like to become one of my deputies?" and I said, "No, thanks."

And I said, "However, if the position that George Heilmeier is in, the Director of DARPA, becomes empty in the foreseeable future, I would be very happy to interview for that."

George decided in August of 1977, that he wanted to go to industry. But there was a special program that he wanted to be still the Director when a certain technical event happened, and that was in December, so that was the time at which we agreed the transition would take place.

**I:** Why did DARPA appeal to you?
**Fossum:** I'd known DARPA for a long time as a contractor. DARPA had very good people working for it. While there were many other places in the

government, especially in the intelligence community at that time that had very good people working for them, DARPA was outstanding. The projects I did as a contractor at EDL, Electronic Defense Laboratory, had to do with high-frequency radio, and I liked high-frequency radio. It had also to do with ECM against ballistic missiles. And, in fact, that was the first time I ever met Charles Herzfeld. Back in '67, or '68, or somewhere along through there, I had to brief Charlie in the Pentagon on what progress we were making. I was impressed with him at that time and still am.

As a contractor, I worked on a variety of DARPA projects, all of which had to do basically with programs closely associated with intelligence. Although they weren't intelligence programs as such, they were technical collection efforts and things that we needed do to understand threats.

Steve Lukasik did a good thing. He established, along with the Defense Nuclear Agency, a working group in '73, '72—I can't remember exactly when—which was called the DARPA-DNA Long Range Planning Panel. I was on the technical part of that panel, headed by Don Hicks. And that was an opportunity to see a lot of DARPA, a lot more than I'd seen before, and to get very interested in a variety of problems. If I were ever to come to government—I decided DARPA would be the right place for me. I never dreamed I'd ever be the Director, but it would be nice to work there.

**I:** Do you remember your feelings when Perry called you up and said, "Hey—"?
**Fossum:** You know, I don't remember such a telephone call—(chuckles)—because we had discussed it, and his view was that that would be a nice thing if I came to DARPA as the Director. His view was that "George was doing a good job, so why don't we leave well enough alone until George decides to leave?" George decided to leave, as I recall, in August or September of 1977. (Chuckles.)

I talked to George and he decided when he would leave—when the transition would occur. It was sort of an informal discussion that whole time.

But you have to understand I'd known Bill since college, so we were in touch from time to time. It was not as though a telephone call appeared from Harold Brown or from Bill Perry, saying, "Have I got a deal for you." No, that didn't happen. (Chuckles.)

**I:** What was it like? What kind of a DARPA did you walk into?
**Fossum:** Well, I walked in on a DARPA that was the same as I'd known under Steve and the same as I had known under George, a variety of projects. All seemed to be very good projects, though I knew only a few of those. There were fast-moving people. It was probably the people that were most impressive. They always impressed me. I was happy to be associated with them. I was happy to work with them. As a contractor I worked for Kent Kresa. I worked for Jim Poor, who worked for Steve Lukasik. DARPA was a great graduate school—(chuckles)—in the sense that people went on to bigger and better things.

I was probably impressed most by the people themselves—as well as the variety of projects. The projects weren't uniformly exciting, but they were all very good. They were run by people interested in the common good, and that was an important thing to me.

**I:** As you look back at the projects at DARPA before you then, how did you weed through them—incorporate new programs and transition the older ones?
**Fossum:** I think that type of transition happens normally. I felt that it wasn't necessary to step in and make great changes. DARPA had a collection of good projects. The key people were not the Director, in my mind, but were the scientific officers, the project engineers, both military and civilian. They would come for a certain length of time. There were really no career technical civil servants at DARPA in the scientific area. Thank goodness for the career civil servants in the management area and in the bookkeeping and accounting and law areas, but we had no one technical who made a career of DARPA. They were to do a specific job.

They inherited projects and initiated their own. The ones they initiated lasted for probably five years, six years, and were over. It was then time for that project engineer to leave, just as it was time for the Director to leave after a while also. And that's one of the strengths I found in DARPA. So, the transitions and the management of those programs were relatively easy, because, in a sense, they either were transitioned to the Services. They died because of some technical failure and, that's a good thing. If they were spectacularly successful, we continued them. Those were the ones you had to watch out for. We did both of those.

So, again, the Director made some changes, but not a great many. When I came in, the program was running. It was not congressionally mandated, but congressionally briefed. The testimony for the next year was essentially written. The budget had been submitted and approved, so there was not any need, nor was there any real opportunity to make big changes in the program, and I don't think it would've been the right thing to do.

**I:** How long did it take you to get up to speed?
**Fossum:** It always takes—(chuckles)—awhile because of the variety of those programs. You go to any company, whether you're a technician, or whether you're a new manager, or whether you're a new engineer, it's going to take you six months to begin to understand what happens. And if you're lucky, you'll understand it in six months. The variety of the scientific and engineering efforts in DARPA were very broad, but still limited, so that it was possible to get your hands around most of them.

There were projects I did not understand thoroughly, technically, and I think that's reasonable. All of us who go to graduate school these days come out as specialists, not as generalists. I don't think there're any generalists made—(chuckles)—any more, and so we understand certain things better than we do others. The programs had to do with space, programs that had to do with systems and the contributions to various types of systems I understood pretty well.

**I:** What was the national defense and security climate during this period?
**Fossum:** Well, this was a time in which the major threat was the Soviet Union. The Soviet Union had always been a substantial military threat. We perceived of it as a substantial military threat in conventional forces, but the growth of the strategic and theater nuclear forces was of substantial concern. I think the top of the worry list for Dr. Brown and Dr. Perry was the growth and the development and installation of third-generation ICBMs and SLBMs. There was still a growth in the quality of the conventional forces also.
There was even, of all things, a move not only to have quantities of forces, but to have quality in those forces. We often think that the Soviet long-range air and the Soviet naval aviation were not a threat, but I think it was a great threat, so the Barbfire and Badgers were a great threat to our naval forces—our carrier taskforces. The ICBMs, because of the increase in accuracy, the increase in yield, and the addition of multiply independently targeted reentry vehicles had become a real problem.

The problem was our strategy was to ride out a first strike by the Soviets. Because of the quality of their forces, Dr. Brown and Dr. Perry became very concerned about whether we could actually ride out a nuclear attack and have substantial retaliatory power. They were also concerned about the survivability of naval forces, and they were concerned about the quality and quantity of the armored forces in Central Europe. Those were the dominating factors, in my mind, at the time.

**I:** Had they deployed tactical nukes in Eastern Europe at that point?
**Fossum:** They were in the process of deploying medium—what we call theater nuclear weapons—yes, they had deployed tactical nuclear weapons. The major threat that we were concerned with was in the theater nuclear weapons, the SS-20, which later on became part of the treaty negotiation and then did not deploy, but that was a major threat at the time.

**I:** Brown is kind of interesting to me, because he's been on a couple of sides of the ABM argument.
**Fossum:** (Chuckles.) Yeah, that's right.

**I:** So, where was the argument at this point? ]
**Fossum:** We had what you could call ABM problems. I mean we had ABM research programs, but they were typically DARPA—far out. There was no issue of deployment of anything. This was primarily research and development, and all of our programs eventually were transferred by Bob Cooper to another agency.

But Harold never seemed to be concerned that we were doing AMB research, doing highly technical and highly stressing programs in tracking from space and generation of high-energy laser pulses. And our idea, of course, which he may have thought would never happen—(chuckles)—so he might have not been too concerned about it - was that we would attack the second stage of any ICBM, or SLBM, while it was still in powered flight and destroy it before it had placed warheads into their orbits. Dr. Brown never talked to me about the ABM problem. So he didn't see the research as a problem, he ABM was a political problem, a problem which was covered by treaty. It was, however, a problem that we had high confidence we would solve, at least in our lifetime. (Chuckles.)

**I:** It is a problem to solve, right?
**Fossum:** (Chuckles.) Right.

**I:** Sometimes the organization takes on the personality of the director. Sometimes the director is kind of guided by the existing programs and the personality of the organization. What was the case when you were there?
**Fossum:** I didn't see that the organization had the personality of George Heilmeier or any particular Director. I thought it had a DARPA personality. I think we had, first of all, strong, confident project engineers and scientific officers. Influencing those guys was sometimes a real—(chuckles)—problem, but you could influence them, hopefully, by leadership. And you could direct them to do things, and they would do it, sometimes reluctantly, but they would do it. But it wasn't really necessary. These programs tended to be their programs, and they were intensely devoted to the success of those programs.

The leadership style that Bill Perry had—and I always worked for Bill, it seems like—was one of providing guidelines about what's important—make sure that the people who work in the company, or in the agency, or in the Department of Defense understand what goals are important, and then expect them to do the things that they know are right to meet those goals. Bill had those goals and expectations. If a person required constant direction, then I don't think they would long work for Bill. I don't think that he'd have them working for him. He chose people that tended to have independence. I certainly hope I had that independence. And certainly DARPA's strength is in its independence as an independent research agency.

So, that's the way I did things and that's the way I sense a lot of DARPA engineers did things. It would've been presumptuous of me to tell people how to detail out the powdered metallurgy problem, or the radial wafer blade problem, or the stereo.

I could say that the style of leadership, which I believe fit DARPA then and now was to give lots of independence to the project officers within the constraints of what we knew was needed to meet DoD goals. There were always other things that everyone wanted to do, but DARPA couldn't do everything. Therefore, we had to restrict projects to major thrusts. You've heard about the major thrusts that we've had.

There were things that DARPA shouldn't do. For example, we shouldn't have been heavily in the silicon integrated circuit business, because industry would eat our lunch there. I mean they knew how to do it, and they could put 20 times the amount of money on problems—and did. On the other hand gallium arsenide integrated circuits because of the unique military characteristics was different, we were invested. DARPA just had a certain number of people—75 scientific officers and 75 administrative people. We had to restrict the number of programs we did.

**I:** Were there "DARPA-sized" programs or problems that you were out to solve?
**Fossum:** Well, I don't sense that there were "size" problems in defense which the Services had trouble taking on. I categorize the DARPA or any defense research and development problems into four categories. These are related to military mission and technology. I divided a square into four quadrants. In the first quadrant, I placed existing missions and existing technology. The defense R&D that went on there was tweaking and subsystem replacement for missions that were clearly identified. In this category there were clear military requirements written down. And since science and technology and engineering always moved ahead, there were improvements to be made. That was the first category and Services did those well. DARPA should not be in that business, so when our people wanted to get into that business, I tended to reject that.

The second category was very interesting one. Could we find new missions for existing technology? And since we had military officers who I insisted act like military officers, they would come up with new ideas using existing technology. There were no written requirements for these ideas, these new missions, so they would have great trouble getting those missions done within the Services themselves.

The Services have a tyranny. That's the only description I can give it. It's the tyranny of the requirements process. If a person has an innovative idea and it doesn't fit into an approved requirement somewhere, it's very difficult to get it done—not impossible, but very difficult. On the other hand, DARPA loves those ideas.

An example of the second quadrant or the second category was defense of submarines against air attack. We developed a little missile system which was nothing elaborate, it was just "racking and stacking" of existing technology, so to speak. The missile was to be launched from underwater when the submarines were under threat of attack. It was called "SIAM". I don't know what that meant. But all we did was take a Stinger Post missile, put an IR homer on it and devise a way to launch it from underwater.

It turns out that the submarine could hear an airplane at a significant distance (chuckles). Acoustics is wonderful stuff. And you could almost always get a warning. If there was an aircraft looking for you. And this little missile, which was simply existing technology, racked and stacked to do a new mission if the Navy so chose. A Navy officer came up with that idea. It was an example of a new mission with existing technology.

The third category is existing missions, but brand new, rather revolutionary technology. Obviously Stealth fits into that. The things that were initiated, really, I think, under George Heilmeier's watch. Stealth technology was a solution to the problem of penetration of a massive Soviet air defense, all based upon radar. It was exceptionally nifty. We had airplanes that enemy radar couldn't see and that was remarkable. You know, talk about really being impressed, I was really impressed with all that developed in the Stealth area. But the mission was classified; to penetrate an intense air defense network.

The fourth category was new missions and new technology. I was trying to think last night of something unclassified in that area. I may think of it when I'm on the airplane going home tonight. But it's that fourth area—brand new missions we'd never done before and new technology to do those missions. So, we developed a technology. The technology issue was always easier than the mission issue. We were technologists and not strategists or military people as such. In fact, that was the reason DARPA had military officers. That was their primary reason for being in DARPA. From time to time they came up with new missions.

**I:** You've been talking about the interrelationship between technology and mission. I'm curious about the line DARPA and DARPA seems to have walked between basic research and applied research.
**Fossum:** We had a limited basic research effort. The 6.1 effort was limited and wasn't a large percentage of the budget at any given time. It was an important percentage. I divide research into broad categories, not by the technology, or not by the technical aspects of it, but what it's there for. Much of 6.1 is there to find new ideas and develop them, and that's a sort of "research push." Colleges do that especially well. The Office of Naval Research and comparable agencies in the other Services do that well, as does NSF.

There's another category, though. I don't want to call it "requirements pull" because I'm very concerned about the use of the—(chuckles)—word "requirement." But, let's say it's "technology pull." There are cases where we have technological problems in defense that need a focused research effort to remove uncertainty. Geophysical uncertainty for example. A substantial portion of our research budget was in nuclear monitoring research. Another one was in underwater sound propagation. These areas are full of geophysically generated noise and therefore require a great deal of research on the noise, to understand its effect on sensors and to understand its effect on the signal source. In the case of underwater sound it's obviously a submarine source. In the case of nuclear monitoring research we were very concerned about foreign nuclear testing. The problem was not "did they test or not"—although that was a problem—the problem was yield. What yields were the tests?

So, the DARPA research tended to be in areas needed to make progress in applications. Therefore, it was motivated or pulled along by the applications.

Another area where we did a great deal of research was in computer science which was oriented by command-and-control and communications. The goals were very 6.1-oriented. The technology that arose was glued together into systems. But the basic efforts in writing protocols and so forth were really 6.1.

But you can see that those projects were generated by a clear understanding of their military utility. They weren't simply projects generated by college professors, or research institute researchers, or graduate students, who had great ideas. They were pulled along by a real military need—not a requirement, but a need.

**I:** Where were the ideas for nuclear and underwater monitoring generated? From presidential policy? From Defense Department policy?

**Fossum:** Well, any sort of monitoring of weapon development in a closed society was very important. Any method that would help us understand the yield of that weapon, whether it was an air, surface or a surfaceburst—information of that type was valuable. Clearly, the adversary never told us. We had to divine that, so to speak. Therefore, we tried as many techniques as held promise. In the case of the nuclear monitoring we had used acoustics since the blast wave itself would propagate over enormous distances, primarily in the lower, the upper troposphere. We could also estimate blast characteristics by radio means.

In the early '60's the USSR and the United States went to underground testing. Then the problems and the methods that had been worked on for atmospheric tests were very difficult to apply.

Thus seismic waves transmitted through the crust and core of the earth became very important. A major thrust of our nuclear monitoring research was the understanding of the propagation of those seismic waves and the understanding to the point of being able to say what the yield of the device really was, what was the yield of the device.

The problem of understanding the internals of the nuclear device was much easier when they were atmospheric testing because we could get samples from the atmosphere of debris. We also developed small satellites that detected atmospheric blasts. DARPA built little satellites and the Air Force launched and operated the satellites. Their purpose was to detect unannounced explosions.

The underwater acoustic problem, of course, was a natural evolution from World War I hydrophones which located submarines and from sonar later. Even when I was in the Navy, active sonar was a primary system, passive listening became a big thing during the Cold War. Our particular efforts were in long-range detection, passive detection, of foreign submarines.

(Chuckles.) Every time I'd turn over a rock at DARPA, there was a new array program—(chuckles) — usually disguised as a signal processing program. We must have had at one time three or four array programs. Some of which were very big arrays and could detect sources over long, long distances, over many convergence zones. They were successful, but these were research systems in the sense that an objective was to understand the sound propagation in the ocean.

DARPA had institutions like Scripps Institute of Oceanography as contractors. The people there were brilliant people working on long range propagation issues with the goal of tomography of an ocean. That program included signal processing and ocean engineering. A few problems arose when we were careless in monitoring contracts. Unbelievable some of the mistakes we made. Certainly we weren't perfect.

**I:** During your Directorship, how were projects prioritized—by either Perry or in the Secretary's office? Where did that prioritization take place? What was the chain of command?

**Fossum:** My chain of command, in a sense, was directly to Bill Perry and his Principal Deputy, whose name was Walter LaBerge in the later part of my tenure. Jerry Dineen was the Principal Deputy in the earlier part. From there, it went directly to Harold Brown. Harold left most of the technical leadership to Bill, who established the priorities.

Bill, as I mentioned earlier, did not say, "These are my priorities." He did two things. The first, he took me with him for his Congressional testimony from time to time, just as a hanger-on or strap holder. I was there to listen to what he had to say. Hence, I was aware of the problems that were keeping him awake at night. The priorities for DARPA, while never written out other than in his testimony were always there.

The second way of communicating priorities was periodic visits to DARPA. Each Tuesday morning, when it was at all possible, he would come over at 7:30 to Wilson Boulevard, sit in our conference room, turn off all the telephones, and we would talk technology. Each Office Director would brief him on technology. There were simple rules associated with those briefings. One, you could request money. You could mention personnel. You could mention technical problems but you couldn't mention administrative problems or funding at that time. Those were in no sense to be used as marketing briefings. They were there to help Bill Perry understand what we were doing and for us to listen to his comments so we understood his priorities. And let me tell you, those were some of the most wonderful meetings that I've ever had.

The DARPA people just loved them. The reason they did was Bill and Walter LaBerge were very quick, very quick technically. He would pick up on things and ask some of the very best and very penetrating questions of our scientific officers. He is such a gentleman and he made us feel like we were important to DoD. That was the leadership style that he showed. That was the way we understood his priorities. We could see him sit up when you mentioned something important to him. We could see the body language so we knew almost immediately what was important. He really didn't have to write out, "Do this," or "Do that."

What that doesn't mean, though, is that he didn't occasionally have an iron hand. I remember one time he called me up and said, "I want you to initiate a competing contract for an expensive missile." It was a lot of money, though. DARPA needed two booster contracts for competition. Booster rockets cost a lot of money so to initiate the new contract was painful.

So, I complained, "Bill, I don't have that type of money." And he listened for a minute, like he always did, and then said, "Bob, you're not listening." And that was the end of the conversation. He made clearly known that this was not an issue for discussion. It was a direction.

Harold Brown did the same thing to me once in the nuclear monitoring research program. He phoned me and wanted me to put money back into the budget which I had cut. Dr. Brown read the DARPA proposed budgets in detail. He was one of the most brilliant guys I've ever dealt with.

In this case, I tried to explain why I'd taken the money out. He listened quietly but then said the very same thing, "Bob, you're not listening." And that was the end of that conversation. (Chuckles.) So, those are the only two examples I know of direct instruction on what DARPA should do.

Bill Perry did help even more. Just the fact that he supported DARPA, which he did consistently, all the time, kept me out of having to argue my programs constantly. People didn't argue with me. Since I had Bill Perry's approval of the DARPA programs they knew that when push came to shove, he would most often support our program. So I was seldom into hassles over programs content. Many of these programs involved the Services of course and involved spending Services' money. While money's not important, technically, in the Services it's way ahead of whatever's in second place most of the time.

**I:** What was the relationship at that time between DARPA and the Services?
**Fossum:** Well, in my mind, there's no such thing as "the Services." (Chuckles.) There's the Army, and there's the Navy, and there's the Air Force. Those were our key customers, of course. Our relations with the Army were very good. I always had a great regard for the military officers in the Army. Especially the general officers. They were decisive individually. But, I felt sometimes the Army had a little trouble understanding what it wanted to do. That wasn't because the green-suiters didn't understand what was necessary in their own individual areas, but the Army generally has tough problems.

Our relationships with the Air Force I would call civil—(chuckles). We seldom screamed at each other—(chuckles)—but there was a lot of tension. We had trouble with the Systems Command people—especially the Commander because we were doing the things that he felt the Air Force should be doing. We had at one time four or five aircraft under development with a budget that was roughly one half of his aircraft budget. At the same time he had on or two airplanes under development. We were doing things that he felt he should do. I think the Air Force should have done the programs also. But the facts were they weren't doing them. So, we went ahead with them.

But we had civil relations. I would get together on Saturday mornings with the individual that I considered our greatest adversary—(chuckles)—and he'd berate me for a while, and then we'd get down to solving problems. So, it worked out. It was civil. He was a good flag officer

Our relations with the Navy were rocky. As a young man I was a Naval officer—maybe that was the reason. I had high expectations. I was a lieutenant and my contacts were usually flag officers—(chuckles). We all know about Sea Shadow. That program required an enormous amount of work. Further, it required Bill's intervention in many cases to make that program go. So, they were rocky.

Now, when I say that, in the Navy, the people in the Pentagon were where we argued the most. We argued with the ASNR&D, who was a former Office Director in DARPA. We argued with the OPNAV people, especially OP-02, OP-03, and OP-05. Those were submarines, surface ships and aircraft offices. That took constant attention.

Now, you drop down to where people were actually doing things and the relationships were almost uniformly good. The DARPA scientific officers dealing with engineers or officers in the Navy didn't seem to have problems. Everybody seemed to be pulling together. It got a little bit bent out of shape at the higher levels where roles and missions and so forth were important.

DARPA had freedom that the Services really didn't have. We would do things that stepped on their toes occasionally. They were very good, very strong advocates for their own programs. The minute I started building an airplane, that airplane program threatened another program somewhere in the Navy or the Air Force. It just did, and therefore somebody said, "I'm going to make sure that DARPA program doesn't kill my airplane program," or even "my ship program". The Service people were very dedicated to their programs and often saw ours as a threat. I know that's the case, because I remember speaking one flag officer later after he'd retired who said, "Well, it really wasn't that bad a program, but you were into my knickers—(chuckles)—and I was really worried by what you were doing".

Nothing I could say, or nothing I could explain to them, would allay those fears. And I think that's where all tensions came from.

On the other hand, if there were technology programs that they personally wanted done, we would do our best to do it.

"Stealth? Do we need a stealthy ship? We have the stealthiest ships in the world. They're called submarines."

Well, I couldn't argue with that. (Chuckles.)

A lot of Navy work went reasonably smoothly because the DARPA scientific officer was a Captain, USN, a Ph.D. from MIT. Very bright guy. He was a signal processor. That's why there were signal processing programs all over the place and each one of them had attached to it an acoustics array. That's what was interesting to me. He was able to get through some of this pushing and pulling just by the fact that he had four stripes on his lower arm. Not only that, since he was very competent people in the Navy looked up to him, looked on him as an expert—one of "their experts."

**I:** What were the mechanisms to transition things from DARPA into the Services?
**Fossum:** Well, that's a big problem. Successfully transitioning was probably our major problem.

We felt our primary mechanism was through the use of agents in each Service to technically monitor the contract and please the contract. DARPA headquarters had no contracting people whatsoever. At the beginning of each year, I signed an agreement with the Assistant Secretaries, a legal agreement that specified exactly how they were required to spend our money. Their R&D organizations knew what they were tasked to do. These agreements further specified that I would be the source selection authority and that they would allow us to technically direct the program and make key financial decisions. In other words, DARPA was not in the business of simply handing them money and saying, "Go do it."

We also asked them to provide scientific and engineering people to monitor the program, DARPA was spending about and 300 million, which is small compared with today's budget, but it was big money in those days. When I left DARPA, the budget was about $750 million. But, we couldn't monitor all the details with 75 technical people without the Services' help. It was our hope that—and in most cases it worked well—that because their labs and their development organizations were working on these DARPA projects, that the transition would naturally occur.

Transition occurred often at the technological level. They may not have accepted the entire technology program to put it into the inventory in some sense, but they would accept parts of it. So, you'll see DARPA technology many places.

There were places where the agent system didn't work at all. These were instances where the agent just took the money and did with it what they wanted, and that didn't go over too well with our people, and those were cancelled. I mean those were cancelled almost within a day, and that program was transferred to another agent, sometimes outside of that service. We could change agents very rapidly. We had the authority to stop an agent's work, stop all the contractors work and withdraw money and thus essentially fire the agent. And I can think of two cases where that happened.

Now, there're other cases where development programs took a lot of effort to get started.

At the highest levels of the Air Force, except for the Chief of Staff of the Air Force, the Stealth programs in the beginning were fought tooth and nail. But the Chief of Staff of the Air Force controls the lot—and particularly the careers of a lot of people—(chuckles)—as I remember the story, it took the Chief himself to get the program underway. But once that happened, the Air Force moved quickly. It took a lot to get them to do the program but they did, and did it well. The F-117 was out of HAVE BLUE and the B-2 developed out of TACIT BLUE. The latter was an example of transition that caused almost no problem.

The corresponding transition of Stealth to the Navy simply didn't work. That was the Sea Shadow program. Now, it didn't work for technical reasons. But in another senses, they didn't want a radar stealthy ship. But there were technical issues—again, geophysical issues—wake reflection, for example, was a real problem. But most of the transitions worked simply by the adoption of technology or subsystems.

Bob Kahn and Vint Cerf, for example, were just brilliant, are brilliant guys. They developed the packet radio switching technology which came out of the ARPANET. That was the neatest network of radios I ever saw, the neatest concept. Packet radio was one of my favorite programs, and that transitioned immediately. To this day, I can see evolution of that program in Army communications.

We bought ten of the radios and took them to 18th Airborne Corps for trial. Now, they didn't know what they had. The elegant technological level was totally transparent to them. That was one of the wonderful things about packet radio. From the field in North Carolina the packet radio net connected to the ARPANET and then to their computers. All of a sudden, their problems of efficiently scheduling the loading out of airplanes were orders of magnitude more easily done. So, they could see the utility of the application but had little appreciation, in my mind, for the elegance of the technology. The elegance of the application to loading out, they understood. The elegance of the technology and packet-switched architectures, they just didn't care about that. They were warriors, soldiers and not technologists.

But it transitioned. I can see in the Army inventory where the technology still exists. Packet radio transitioned naturally. Actually, the Army tended to accept technology reasonably easily, not always, but mostly they did.

**I:** Inquiring minds need to know what the projects were that you said, "All right. Stop." I'd love an example of something like that.
**Fossum:** The first example I can tell you was administrative, but I can't remember exactly what the project was. We moved out of a Navy lab in San Diego, and moved it to an Army organization in Hunstville. I'm not quite sure what that project was. It had to do with tactical technology of some sort. That happened because the office director became so frustrated with the Navy agent that he got the program moved on a Monday morning. That happened in one day.

**I:** You still had the flexibility to write the check and start it? I mean was that how fast or agile—
**Fossum:** We were very agile. And, again, we were agile for the reasons that you say. We had control of the money. The Treasury issued DARPA warrants to spend money, so we could sign checks and spend money. But we were agile mainly because we liked our agents, took care of our agents, spoke almost daily to our agents, respected their problems, and only used real fast-track when it was absolutely necessary.

So, we didn't exercise it fast track for drill, but we had agreements, and it happened maybe once or twice. DARPA and certain agents could get money on the street in 15 days. That meant we could start the ARPA order, get it to the agent, and they would have the contract out—spending authority in the contractor—in 15 days.

And there were one or two examples of that. And that only occurred where there were special problems—not management problems, but starting important new ideas. That's why we negotiated that. That all of a sudden needed attention.

And for those days, that was pretty remarkable. The intelligence community would usually do it much better than the Department of Defense, and get money out very fast. I remember because I used to work for them as a contractor. DARPA had good agents. We respected what they did for us, and that was making our interactions mostly technical and not bureaucratic.

**I:** The Army, the Navy and the Air Force. What was the relationship with Congress at this time?
**Fossum:** We had a wonderful relationship with Congress. Congress was a place we liked to go. The people talked to most were the staffers. And our relationship with the staffers on the Senate side was very good. The Services also had good relations with the Senate staffers.

As I recall, not all the Services had good relations on the staff side—the

Armed Services Committee. There were some Service people who annoyed the staffers and some staffers that annoyed the Services. But some reason we got along well with the staffers. As a consequence, we got on well with the members.

The reasons aren't hard to see why. First, we were explicitly honest. We never spun anything. We never tried to say "Well, that's somebody else's fault." So, we took responsibility for failure and gave credit for successes. And the Congress could depend on us to give an honest answer. That was the main thing.

The second reason was we genuinely liked most of those guys, even the people the staffers and the Services didn't like. And they liked us. And we tried to get them over to DARPA often. If we could get them into DARPA like we did Bill Perry and Walter LaBerge, we would have them know everything that was going on in our group.

There were few surprises in Congressional testimony since they always received written testimony in advance. And DARPA tended to get the programs done efficiently, the way we said we were going to do, and guess what? They trusted us.

What that didn't mean is we always got what we asked for. We didn't always win. We lost in two ways. The first way was that they did like DARPA and the members tried to put too many programs into DARPA. We had only 75 technical officers. We couldn't take on too many programs. It wasn't a money issue. It was an issue of what we could do and couldn't do, specifically how many programs could we handle.

But the members of Congress— especially in the Armed Services Committee—all had their own little technology favorites. Some people call then "hobbyhorses"—(chuckles). And they would love to get those programs into DARPA; I had to push "hobbyhorses" away. That was the pushback problem.

But because the staffers understood all of that, we didn't have too many problems. We did get a program or two, but not all that the members wanted to push on us. Those programs took the time of our scientific staff. They were of lower priority; therefore, they took my time to make sure at least we were moving them along. In other words, they were lower priority of our scientific officers and the Offices to which they were assigned. Further, they left us with "out-year mortgages" because none of them were less than five or six years long. And after about two or three years they forgot where that program came from. And guess what? You just pay out of the DARPA budget. Maybe we were liked spoiled children, but we didn't want to use our pennies on other projects. That was a problem.

Once in a great while there was a problem with a contractor. As an example, one of my best friends from California, whose corporate lobbyists had inserted key technical specifications of a laser communications system into the Appropriations Bill, and the only place we would procure the laser was from his company. The lobbyist had somehow gotten the specification in there. I was pretty bent out of shape at that. It was poor form, in my mind.

But, we got that straightened out relatively fast, because that corporation had multiple programs with us, and I could make big changes; let me put it that way — (chuckles)—on those multiple programs. My friend said, "It didn't happen on my watch, Bob, so help me."

(Laughs.) So, anyway, that was solved.

Again, one where we lost. I asked once for a plus-up in one program and failed to get it. It was in a Navy program and I'm sure there was a lot of back-channel pressure put on the Congress and the staffers not to increase the Navy budget for that program in DARPA.

**I:** Well, I was going to ask about instances when Congress would want to stick it to or get even with one of the branches, or something like that.
**Fossum:** In the time that I was in DARPA, I was not conscious of programs given to us as a punishment for lack of action in the Services. That would be where they would take a program from the Services and transfer it to DARPA and say "You go do it, because the Services are not doing it." I don't think that ever happened while I was the director.

However, new technology programs, especially those that were favorites of certain members, occasionally found their way to DARPA because it would take too much time for those programs to get any attention—(chuckles)—in the Service. That was their point of view. I'm not saying that's true or false, but the Congressional view was it would take forever.

**I:** You never heard the "Why DARPA?" question asked?
**Fossum:** Did I ever hear that asked? It seems to me I've heard that asked, but I can't remember the exact context. I certainly don't recall it asked in the same context of the question you asked me—as a punishment for the Services' lack of action on something.

**I:** Or the Services saying, "Well, we can do that. Why DARPA?"
**Fossum:** Oh, they always said that. I mean they said that a lot. The issue was that they could do it, but Congress, in particular, did not believe it. If they had a program they really wanted to make sure was done they would worry that it would not get done outside the Service. The Services often are very bureaucratic. If you don't have a requirement or if you don't have adequate internal support for it, it will die.

I mean it will die an unnatural death. I can give an example of a program that was signed out of DARPA in 1980 or '81—and it's taken forever to make progress. And part of that is not just lack of attention. Part of it is just adversarial. They just didn't want to do that.

**I:** What's the program?

**Fossum:** Well, it happens to be the electric gun—(chuckles). I think that it's taken way too long to develop and it's because of the way it was funded and managed in the Army, not the green-suit Army, the civilian Army. But I shouldn't talk too much about that. 03:21:06

**I:** We mentioned Global Hawk and the time that it took to be transitioned.
**Fossum:** I remember going up to the tenth floor in the Architects' Building on Wilson Boulevard where the Tactical Technology and Air Technology people were. And I don't remember the exact dates. I used to like to walk around and talk with DARPA people.

Well, there were some "wild-eyed" contractors sitting there feeling badly because they had been unable to score with the Office Director to get a program started. They'd just finished briefing him.

When they saw me, their eyes lit up, and they said, "Dr. Fossum, how would you like an airplane that flew for 24 hours at 50,000 feet?"

I said, "Make it a week and 70,000 feet, and I'll buy one."

And, sure enough the proposal came back for one week—(chuckles)—and 70,000 feet, so I funded that. And I told the office to fund it, really. I said, "You get crackin' on this. This is worthwhile," because these contractors were garage shop people. They weren't a big airplane company so we had no way to judge success. And we didn't have any idea of how to do the engine. But that was the beginning of a program called HALE—High Altitude, Long Endurance—out of which, 25 years later, Global Hawk emerged.

Now, there was effort all along in that, because I knew people that would say, "Come In, Bob, and I want to tell you about this program." And they would give me a little hint on what was going on—it was being worked on. But that was a severe technological challenge, so I can't say that that was just lack of management attention. And I think there was a feeling that that was of high utility. But it certainly did take a long time to make that happen, even for tough technology.

**I:** Are there any other projects that come to mind?
**Fossum:** Like, if I asked for a week, I got a week in the air. If I asked for 70,000 feet, I got 70,000 feet. Of course, these were all paper studies, so they were not easy to make fly. Can't think of any other projects that were like that at the time.

**I:** Anything that comes to mind of a project you really liked, but maybe died or had to be killed, or, "Wished I'd just pursued that a little bit more"?
**Fossum:** I think I have a list of projects I'll—(chuckles)—answer question later, if that's okay.

**I:** Okay. Where do the ideas come from and how important are people to DARPA? Can you give insight of the importance of people in that organization?
**Fossum:** I honestly believe it's because of our method of management, because it was a decentralized organization and people were most important. They had to have their own ideas, which many of them did when they came into DARPA and, therefore, were able to push these ideas, if they could get the Director's attention.

But also, they had to absorb ideas. They had to recognize ideas as being technically feasible in the long run, as having a high military utility—as being a fourth-quadrant program. And even though these ideas didn't always fit into the Office thrusts at the time, they had to recognize the idea's feasibility and utility. And the people were the key elements there.

The actual project engineers were the key to DARPA's success. The office directors had a lot of management problems, but the project engineers did the technical problems. They were uniformly good technically. There were occasionally some guys that needed to be brought up short. Let's put it that way.

Therefore, in our decentralized structure, we got a substantial portion of our people from industry, people who were middle managers. They'd already managed projects. They had some feel for the art of the possible; and, yet, they were technically very competent. We would hope they were technical leaders in their field. That doesn't mean they were Nobel laureates, but they were recognized as technically competent engineers and scientists in their field.

They had other characteristics—they felt a sense of urgency and they wanted to see these things done. Those were the characteristics we looked for. They didn't all have to have Ph.D.s. It turns out that many of the best of them didn't, but those characteristics: sense of urgency, technical competence; were important.

Sometimes, these project engineers were so dedicated that they would never accept failure. That's where the Office Managers and the Director had to become hard-nosed. There's a time at which some technological ideas are not going to succeed. As an example, we had a nifty program to locate submarines using a blue-green laser in an airplane. It had been going on for quite a while and we had everything waiting to test. I remember trying to find an airplane. I was even going to buy an airplane, if necessary, if I couldn't get the Navy to provide one. This was an important mission.

But the problem was the contractor couldn't get the laser to work and so I listened to that for a year and said, "All right. Just keep working on it." The second year I listened to it and said, "Look, I'm beginning to worry. You have one year to make that thing work."

The third year they came back and they said "this was the most wonderful year yet," but still it was not up to adequate intensity. But unfortunately, I said, "No. It's all over."

That laser wasn't going to work and when a laser won't work, it doesn't make much difference — (chuckles) — how clever the architecture of the system was. It wasn't going to work, so I stopped the whole thing.

And that was a terrible problem because the person who was the technical leader in DARPA was one of my best people. But he was so dedicated; he knew the contractor was going to make it work! They were the best. They were some of the very best laser contractors, and they were going to make it work. But it never did.

There's a time you have to cut the losses, and that's what—that's what the Director—(chuckles)—had to do.

And there was blood all over the place and gnashing of teeth and arm wrestling and so forth. But in the end, it was cancelled.

Later, after the change of administrations, I was still the Director of

DARPA and I remember going to a meeting in the Secretary's office—Secretary Weinberger's office. And the Secretary of the Air Force for some reason began a long tirade on "Why DARPA?" "We can do everything that DARPA does. We don't need DARPA!" And he said, "Let me give you an example," and he used Stealth. "We developed Stealth. We developed that."

Incidentally, it turns out there were more than the two airplanes that came out of that program. There were other things going on.

And—"We did that," "We did that." "The Air Force did that."

It's true, but the Air Force did it as our agents, and we had DARPA agents, and we had to force the use of some Air Force money to make it happen.

But after the Secretary of the Air Force stopped talking, I said, "As I recall, this is the way it happened," and I told the same story about how difficult it had been for Dr. Heilmeier to get the Air Force involved. It turns out that Mr. Weinberger, who we thought was asleep, was not asleep. He was listening. And his eyes were shut but he listened to this exchange, and he listened to my explanation. He looked over to the Chairman of the Joint Chiefs, who was the same person who had been the Chief of Staff of the Air Force when George got the Air Force involved.

And I finished by saying, "And that's the way I think it really happened."

And it was that point Weinberger opened his eyes and said, "That's the way I think it happened." (Laughs.)

That was the end of the "Why DARPA?" discussion.

Again, the people can recognize the common good. Some can, and others are so devoted to their Services, that they won't step out of that role. And this person, whose name was David Jones, the chief of staff, and chairman of the Joint Chiefs, did understand the common good and did understand the importance of that. Didn't say, "Let's get crackin' on this," and defended DARPA in that sense.

**I:** Some of these projects that were really exciting.
**Fossum:** Well, I mentioned the packet radio. I've always liked the—the packet-switched architecture. It has great military utility, and of course it has great commercial utility today. But Kahn took this architecture into a radio setting in which radios could come and go into this network and were automatically incorporated in the network; where the stations were no longer fixed like they were in the ARPANET, but were mobile; and the stations were small and were useful to a company and a battalion-level communication system. I really liked that.

It was that system that we bought 10 or 12 of and took down to the 18th Airborne Corps, so that was one of my favorite ones.

**I:** It sounds like a BlackBerry. Is that what it is?
**Fossum:** Well, I hadn't thought of it that way—(chuckles). It was substantially more automatic than BlackBerry. At the time, of course, there was no "wireless system." There were none of the architectural niceties of e-mail as such at that time, nor was there was a worldwide web. You couldn't search the web—there

wasn't one. But, in a sense, it might have been a predecessor. But it was much larger than a BlackBerry—substantially larger.

The second program that I particularly liked, of course, was Stealth, the TACIT BLUE program. I learned a lot from that program. TACIT BLUE was the demonstrator of the technology eventually was incorporated into the B2 bomber. The DARPA engineers at the contractor had a tough time with that program in the beginning.

We were very interested. Here is another "direction" from Harold Brown and Bill Perry. There was another one I'd forgotten about, "You'd better get a second contractor going in the Stealth area."

I didn't have a choice—(chuckles).

But the issue was to get the radar cross-section of the vehicle very, very low. We knew how low it could theoretically be, so we were confident the contractor could get there. But there was a basic problem: we had to defend what we considered proprietary rights that belonged to Lockheed. We were very careful to protect those proprietary rights. We could not tell our TACIT BLUE contractor how to do it, but we knew how to do it. So, we had to force him into developing his own low cross section technology.

Well, the contractor didn't force easily. Not because he was technologically unimaginative, but because he focused on airplanes. He knew how to make good airplanes. But what I wanted was an airplane that was, first of all, a low cross-section airplane. So, there was heavy pushing and pulling on that program.

Their design engineering team was made up of safety engineers, of engine people, structure people, aerodynamics and all sorts of people that go into making an airplane. But none of those people were the right ones to make a low radar cross-section airplane.

Two things happened. Progress got so bad, that I said, "I'm not giving you any more money until you show me that you can design a very low cross-section vehicle." DARPA had given them specific goals. And I said, "Secondly, here's my advice on how to do it. I want you to take some small electrical engineers and physicists"—I mentioned them by name because I knew a couple there that were really smart—"I want you to lock those guys in a room for 30 days and I want no one else to tell them what must be done and I will give you a shape to start with."

So, I drew a fuselage and wings and I said, "You make this shape into a low radar cross-section. Ten you come back to me." I said, "I don't want to hear anything about cockpits. I don't want to hear anything about engines. I don't want to hear anything about anything except the radar cross-section. You'd better be confident, because we're going to put that onto a tower and test it. You make it and we'll test it. Once we get that shape, then we will make it fly."

Well, I'll be darned. They did it.

And so of course we compromised in order to make the demonstrator fly. A lot. We didn't have the most efficient, aerodynamically sophisticated airplane. We didn't have the best engine in the world. We didn't have all of the bells and whistles that usually go with a new airplane. All we had was a shape, but, boy, its cross-section was very low. It was a brand new technology, a different technology from the other Stealth contractor.

So, what I learned from that and what we needed to learn in DARPA was to focus. This was one of the reasons we were successful. Do not try too many things in a single program.

I mentioned earlier that the Air Force would develop one airplane while we were developing four. Further, we did our four with half of their budget level. The point being that we focused on a few advancements, they were forced to include all their bits and pieces of military requirements in their airplane designs. Soon it was expensive and its performance had been compromised. We focused on Stealth.

Engineers on design teams contribute the things they're best at. Everybody on the team gets a vote. If you take away all the votes except the Stealth votes you get a stealthy airplane, and that's what we did. I will tell you that the TACIT BLUE didn't have great aerodynamic, but that wasn't what we were after. We could trade down from the extreme radar cross-section goals. We could compromise those goals. But what could not happen—and it was patently obvious—was they couldn't take their individual goals upward to meet ours. In other words, we couldn't get a general airplane design and then make it stealthy. We had to get a stealthy design and then make it an airplane.

Focus is one of the things that make advanced technology successful. The minute you add too many other requirements, it will compromise the primary goal. So, focus, focus, focus is the big thing, in my view, at DARPA. That's why TACIT BLUE was so important, to me. Great people to work with and an excellent contractor.

The Air Force put great people on the TACIT BLUE program. But it was a challenge! They solved the challenge. We really pushed the agent and the contractor until they did it. And that was a good scheme, in my view.

There's something we haven't talked about—a third project. This one came in sort of out of the blue. Again, Bob Kahn came in with Carver Meade and Len Sullivan. They said, "We want to do something for colleges."

And I said, "Well, that's not really our business, but let's hear what you have to say." (Chuckles.)

And at that time, very large-scale integration was just coming into use. It was important mainly in the computational world. It turns out that Lynn Conway and some of her cohorts had put together a set of design rules and a way of designing integrated circuits which was mostly done by the computer.

It solved many design problems including the complicated real estate problem of smartly placing the gates on the circuit. DARPA could not fabricate the circuits, obviously. Texas Instruments, Intel and organizations like that made integrated circuits.

If you were in a college and you were trying to teach integrated circuits, you couldn't get an integrated circuit, because the way the companies made money was by volume and selling 4 or 500 in the beginning and 5,000 integrated circuits after it had been debugged.

So, Bob Kahn sponsored these people to set up what he called the

Foundry—the VLSI Foundry. It was based upon two enablers. First was the ARPANET. If you were at MIT, at Stanford, or another university on the net, you could design a circuit on your computer using the software that was available on the ARPANET. And you could draw down cells from the ARPANET for your integrated circuit project if you were a graduate student—the users were almost all graduate students in those days.

Later, when I was Dean of Engineering in Dallas, even the undergraduates used the system. We did the same thing for all graduate students down. But in the early days, it was all graduate students. The process was almost like software program development. You designed the circuit and submitted it to the Foundry via the ARPANET. And it was built. Each wafer had 10 or 20 experiments on it. People in the Bay Area, including Palo Alto Research Center, Hewlett-Packard, Advanced Micro Devices and others would take the designs, make the masks, lay out wafer, fabricate the wafer, dice it, package it and send it back to the student so he or she could put it on a printed circuit board in 30 days.

That was a remarkable thing, because all of a sudden, here were these students at five, six, seven universities beginning to design their own integrated circuits.

Now, very few of the designs worked the first time—so the student had to iterate. And if you can imagine having a cost of a million dollars in iteration, for a university it was impossible. For us, we could do it.

Thus the Foundry concept put into the educational process a method for students to learn to design integrated circuits. The result of that was a blossoming forth of the applications of integrated circuits in many places. The Foundry effort was sponsored by Bob Kahn in DARPA. It was the idea of Carver Meade, who's a very good scientist, and Lynn Conway, a very good engineer.

I just think that program was one of our finest. I don't think it was in the program budget. We just did it because it needed to be done. We didn't do it alone, though. We had Xerox Parc, Advanced Devices, Hewlett-Packard and others. We had all those people helping. The people out there were as interested, if not more interested in making it happen as we were.

**I:** It's interesting because there's not a military application there.
**Fossum:** Ah! Well, it was a beginning. What then happened was engineers who were in the design of military systems began to say "There's now some hope that I can put on this printed circuit board, a special-purpose integrated circuit, for my specific application". Many military applications did not necessarily buy 5,000. We could debug that circuit. Those VLSI circuits were fast and reliable. We wanted them in military systems.

And that program was, to me, a high point in what DARPA did. It was something that we had the flexibility to do, and we had the sense to do it.

Again, who was responsible? Smart guys. Smart guys came in and said, "Kahn, let's do this." And he said, "Sounds like a good idea to me. Let me go get the money."

Money was not as much an issue as was support from some government agency. We were the natural government agency. Actually, it wasn't an extremely expensive program.

Another one I loved, that you'll never believe—(chuckles) — was a radial wafer manufacturing concept for blades in turbine engines. I won't go into that—Pratt & Whitney was the contractor. The DARPA science guys under Arden Bement did that. The program had a set of nifty technologies.

Another favorite of mine came from an Air Force officer whose name was Jack Thorpe. This was a very small project related to Army tanks. Jack had come to us—we had many Air Force Ph.D. officers. The Air Force had ten times as many Ph.D.s as either the Army or the Navy, so DARPA had no trouble getting smart Air Force officers.

He had a unique experience when he was working on simulators at Luke Air Force Base in Arizona. At Luke there were large where simulators to train fighter pilots."

The high-cost subsystems of those simulators were the mechanical motion generators. It wasn't in the visual or acoustic; it was the moving of the cockpit in this simulation.

Well, as Jack explained it once, all four of the mechanical subsystems went kaput. The support contractor couldn't get them back up at once and one class of pilots went through the simulator training only seeing the visual and only hearing the audio results because the mechanical part that threw them around to simulate maneuvers wasn't there to enhance their training. It turns out it made no almost difference in the overall scoring of the class.

That made a significant impression on Jack, so when he came to DARPA, he wanted to capitalize on that knowledge. One of his projects was called the M-60 Tank Trainer. The M-60 Tank Trainer was one of the first videogames used in training. It had a large video disk with photographs, frame by frame, of the Folda Gap in Germany where the U.S. Army expected tank battles to unfold in the event of war in Central Europe.

With the disk we made a game and the M-60 gunner Tank Trainer so soldiers could look into this trainer.

This was a desktop simulator. It was all visual and audio only. Soldiers simulate aiming and firing with the background of the Fulda Gap. So, they had the realistic visual environment.

The gunnery simulator had an enemy tank which would suddenly appear. So, the idea was to quickly point and shoot accurately. The basic objectives were very simple: shoot first and shoot fast. Shoot fast, shoot accurately, 'cause if you don't shoot first, you may not get a second shot.

We had a big video disk, that's where all the money went—creating and reproducing the disk. You can't imagine. It cost us a quarter of a million dollars just to make the disk.

We then tested it with soldiers. They absolutely loved it. Later it was used all over the Army. That was a relatively inexpensive little device which soldiers loved. It improved gunnery accuracy substantially.

Transitioning was no problem—no problem at all. We helped the Army buy man early models.

And I was very proud of that. It was small, very effective, very imaginative, and it used advanced technology in a sensible way.

We once took that to the Senate and I remember Senator Warner looking at it, and the senator from Georgia, who I loved.

**I:** Was it Nunn?

**Fossum:** Yes, Sam Nunn.

Both Warner and Nunn played with the gunnery trainer—(chuckles)—. DARPA could transport it around, because it was desktop. It was an example of the clever use of technology which did things that were related to weapons. It helped soldiers be more efficient at training. That was important to DARPA and the Army.

**I:** When you left DARPA, what were your feelings?

**Fossum:** DARPA was the high point of my technical life, that's for sure. I thought so highly of the people I worked with. It was a teary thing. The only time I ever felt as bad was when I left the Navy. Well, it was the same regretful feeling, because they were just absolutely great people, I tell ya, it was the people.

Technologically, DARPA just did imaginative things. And it has kept doing good things. It is an organization we should all be very, very proud of. It's such a good organization; we have to be very careful of it. I have often thought, "God willing, DoD will never break DARPA, because if you break it, you can't put it back together again."

Those were emotional days. I didn't want to leave.

And I... It wasn't that I shouldn't leave. I'd been there long enough and DARPA Directors need to change. We don't need career DARPA Directors, and we have not had any. That's one of the strengths. We don't need career scientific officers either.

Many technical people wanted to stay in DARPA, I just refused that. After they'd been there five or six years, that was enough. Even so, there were a couple that should stay and did stay and that was right. But they were exceptional.

Bob Kahn would be an example of that.

**I:** What do you think, then, is the key to DARPA's success and to keep it successful in the future?

**Fossum:** I think the key is people. The turnover rate has to be high. The turnover rate has to include inputs of highly competent technical people. And the turnover has to include inputs of talented feet-on-the-deck, feet-on-the-ground soldiers, sailors and airmen. And those soldiers, sailors and airmen have to think like soldiers, sailors and airmen, less like Ph.D.s.

So many of the military officers came, DARPA ha a very heady atmosphere. And if they're a Ph.D., they came to DARPA, and all of a sudden, the environment was heavy research and development. "So, let me start acting like a Ph.D.," was the natural reaction.

But that wasn't their primary mission, frankly. And maybe one of my failures was not to push the military people hard enough to bring to DARPA the military experience that they had to bring the ideas of new missions with existing technology and new missions with new technology. But it is important that the mission orientation come from the military.

The idea that the Services would ever let civilians write their technical requirements is just anathema. Officers, master sergeants and chief petty officers should write those requirements—not civilians. And that's exactly what we had hoped would happen in DARPA.

Now, the Services found out how valuable smart people are, it was hard to get those people into DARPA.

Again, when I left DARPA, it was a highly emotional issue. I, you know, I love DARPA.

**I:** The Soviet Union's gone—all these—diffuse enemies. Does DARPA play a role in that? How—?

**Fossum:** Well, in a sense, the enemies diffuse. You used the correct word. It's a new type of warfare. It's not that terrorism has not been around—it has been around. You can read about terrorism throughout the British Empire and elsewhere. For example, Somalia has always been a place rife with insurrection and terrorism, you might put it. But it was at a level that never exceeded our—our threshold.

This new type of warfare. It is an "off the shelf" technological threat. How do we fight Radio Shack technology? We knew—we were warned about Radio Shack technology way back, because Radio Shack technology—on the basis of the integrated circuit—was becoming very sophisticated. One could see there was a looming threat.

We also knew that sub-national groups were on the rise even when I was DARP Director. I remember going to an evening get-together at the National War College where we had some of the high muckety-muck civilian technical—especially the Beltway technical guys—and military experts, some of whom had never been in the Services, but were military experts, anyway—expound. But the one most thoughtful papers I heard was from a Navy commander. His name was Harlan Ullman and he's now pretty well-known. He said the threat of the future was sub-national, cross-national groups. And at the time, that paper really caused me to take note of emerging terrorism.

And DARPA did have a small effort at sub national group tracking and data organization, not analysis, but data storage and data organization and data handling associated with terrorist files. It's actually reported in my Congressional testimony. But it wasn't a major effort.

So, there are two points. It's a new type of warfare. Who can attack this new type of diffused technological warfare better than an organization like DARPA? I don't think there is one that thinks militarily, technologically like DARPA or that can respond faster. It's fast-response warfare.

There are undoubtedly instances of more responsive projects in other government organizations. But as a way of life, as an organizational ethic, there are none.

So, DARPA is important in the war against terrorism. But we have to understand how to fight such a war. We have to have people who ask, "Where are we losing in this war?" And one of the areas we're losing in this war is in information warfare. We're not winning the—(chuckles)—information war in areas of the Middle East and even more generally throughout the world.

And can DARPA do anything technical about that? We have in the U.S. some of the quickest—not always most accurate—but quickest journalists in the world, but there's no way of getting their positive ideas to the people in the Islamic Middle East. Is there a way DARPA could provide the technology? DARPA could. The technology of broadcasting our own message—I don't care whether from ships, whether from satellites or where it is. I understand you have to broadcast in frequencies that are technically difficult and difficult to regulate. It's not like dish TV where you can broadcast at a microwave frequency. You have to broadcast, for example, at a VHF frequency where the audience has a radio receiver.

But those aren't technological problems. Can we proliferate radios and provide people with radios to listen to our side of the story? Well, DARPA could look at those problems.

The program content is not a DARPA problem. Program content is a State Department or United States of America problem. But DARPA can contribute by looking at far-out ideas. I think DARPA does readily accept and examine far-out ideas. I mean really—(chuckling)—far-out ideas. I think there is a place for DARPA. It's an essential organization to this new type of warfare.

The second thing to remember is the other type of conventional warfare hasn't gone away. Some people like to think it's gone away, but it hasn't. We still have enemies who will use the same type of technology. The technological battle will continue. DARPA must continue to look for asymmetric technologies.

I don't have any particular wisdom on this issue — (chuckles).

**I:** Were there any that came to you that you—said, like, "We're ... the science honesty broker"

**Fossum:** Well, if you are asking about "science kooks," we always had kooks—(chuckles). We had our share of lead spaceships. In those days, people could freely walk into DARPA. We always had to be alert to people walking in with ideas, sometimes drawn or diagrammed on butcher paper. You never knew how these people were going to behave.

There is one interesting story, it may be apocryphal, and it didn't happen on my watch. DARPA used to get calls from a guy at 11:30 each Monday morning. He would phone long distance and say, "Stop controlling my mind. I've read about DARPA. I know you're controlling my mind."

And we—(chuckles)—got so we would hand this off to somebody, and they'd try to convince this person that mind control wasn't happening.

And after a while, the person to these telephone calls were handed off one morning said "Wait a minute. I'm going to go turn that machine off. You wait right where you are, and it's going to take me a few minutes. You just wait there. Keep on the phone and I'll be back."

She put the phone down and went back to work. She picked it up a few minutes later and she said, "I turned it off. How do you feel now?"

And he said, "I feel wonderful. Thank you very much."

He slammed down the receiver and we never heard from him again—(chuckles).

Now, that may be apocryphal, but it makes a good story.

As far as specific, far-out ideas, there was a natural filter in the offices. The filter tends to become important in another way. It tends not to accept far-out ideas. When the ideas fall between major thrust areas. Because money is important to DARPA, no Office Director wants to fund an idea they can get somebody else to fund. No Office Director wants to give up his fund to fund a new idea that really fits elsewhere.

As a result, I kept a small reserve, which my financial management people controlled. The Office Directors couldn't get their hands on it. At least I don't think they could.

And that was used for those types of new ideas which didn't have a natural home in an office. So, when such an idea came in over the transom, there shouldn't be a reason that we couldn't do it if it was really good. If it was in an Office Director's major thrust, he should do it. And, generally, they did. If the idea fell between office thrusts, I usually funded it out of reserves. The reserves were sometimes looked upon as money to pay overruns. But that isn't what happened.

I don't remember any case of hearing about things that were so wild that nobody would fund, with the exception of the high-altitude, long endurance aircraft program. I happened to be walking through the office at the time. But that happened — (chuckles). That was such a clear-cut case of being in the Office Director's bailiwick. Hence, he should fund it and I made him fund it. I think I gave him the first year's funding, but I said, "You get it in your budget from now on."

I can't think of other things. There—

There's one other problem. When too many ideas come in that are good, you have to watch that the offices do not begin to lower the funding level on existing projects to fund a new project. And one thing that was an anathema to me is "under funding projects." You can assure failure by under funding. In this case failure means stretching the program out so in the long run it's much more expensive or it simply makes inadequate progress.

Further, stretched programs become entitlements. Soon, they become lifetime projects. We didn't want any of that. Never under fund. But often people become enamored of new projects but don't really know where to get the money. They sometimes say, "I'm going to take money from this existing project or that. I'll drop out one deliverable so I can fund this new thing". Something like that.

We had to watch for that, because it happened at least once a year. You'd find an ARPA order that, sure enough, had taken money from one project to pay for another one. That was a red flag, because under funding assures you failure. You can't assure technological success. You can't make people invent, but you can sure prevent them from inventing by under funding projects.

I: Let me ask you about serendipity and its role.
**Fossum:** Well, obviously, you can't plan for serendipity. But it happens. I can't think of too many in which serendipity is a big factor.

One of the more interesting cases of serendipity was a program we had with the Navy. In the beginning the project was a laboratory attempt to evaluate new information technologies for tactical-level flag officers. To this we constructed a simulated "flag plot" carrier battle group. It was exactly the way it should look. But instead of steel, it was wood. One of the unique characteristics was a simulator for war games. It was called WES, Warfare Environmental Simulation.

This simulation had a red team controlling the adversary forces. It was basically to simulate the attack of Russian Badger and Backfire bombers against a carrier taskforce. And the air battle that resulted from that type of attack—which, thankfully, never happened—was really quite remarkable.

First, it was very complicated. Sounds like it would be simple, but it was complicated. It had a variety of outcomes. The battle unfolded very quickly. You would think, "Gee, ships steam slowly." Aircraft don't go that fast, but let me tell you, people had to make all the decisions to defend the taskforce quickly, and you have to make them right. If you don't, you lose.

We put actual flag officers into this simulation, and in that way, we could learn what information they really wanted, really needed.

We found out some interesting things. First, each commander needs different types of information. Each had a different concept of how to understand the battle space. Each has his own command style. We discovered that we can't decide that for them. That is, there is not universal template. We can give them the basis of information but the details they must ask for. Otherwise, we overload them with unneeded details. That's worse than no information, we discovered.

But, those were the things we were looking for. What we weren't looking for was evaluation of other types of warfare systems technology. Suppose we weren't talking about information technology. As it turns out, the air battle in those days was made up of attacking bombers that launched air-to-surface missiles, and they launched those quite far out, and in most cases. Well, they either climbed to very high altitudes– altitudes above which our fighters could not shoot them down– and later dove into attack, or they dove immediately where they were very hard to defend against.

So, the best defense in the air battle was to try to destroy those missile carrying bombers before they launched their missiles. That was far out over the horizon.

We found out that, for example, Sea Shadow, which we were able to insert in the WES simulation, was very useful. If we put in an ordinary picket ship the attackers would destroy the ship and then come rolling on in.

But Sea Shadow was a ship that was hard to find in simulation and attack.

With a low probably of intercept radar—in the simulation, again—it made all the difference in the outcome of the battle, because the commander had early information. The flag officer had early information. He knew where to place his assets. He knew when to place them. He had an excellent awareness of the unfolding battle.

That was serendipity. We had not planned this installation for technology evaluation but it worked out very well that way.

**I:** The Navy wasn't really into Sea Shadow up to this point.
**Fossum:** Oh, they weren't into Sea Shadow ever — (chuckles)—so; it wasn't a case of "up to this point." As I mentioned before when we discussed Sea Shadow, Sea Shadow, in a sense, was perceived, probably incorrectly, as a threat to other programs. It's true that a new technology is often perceived by of existing programs managers, who are very dedicated to their platforms and to getting their platform successfully into the service. Any new idea that comes along that possibly makes their job harder. Therefore, depending upon how passionate they are, they will fight it strongly. And that's where I ran into a lot of trouble in Sea Shadow.

**I:** Did they come around?
**Fossum:** No. But DARPA had the same problem getting the stealthy airplane started. There were airplane programs around to which the concept of a low radar cross-section airplane—fighter or bomber—was a threat. But you see those highly focused technologies were really not a threat to general-purpose aircraft, whether it's a general-purpose fighter or general-purpose bomber. They were no threat to B-52s and B-1s. Those airplanes, in a sense, will be around forever. B-52 is the greatest airborne truck in the world.

Special, highly focused platforms were, whether aircraft, ships, really couldn't do everything that an ordinary platform would. So they really were not a real threat, but they were perceived as a threat. Some people would say, "Yeah, that's a threat to such and such program... Why do we need that ship?"

For example as I mentioned before that the DARPA airplanes were designed to be stealthy, which caused them to compromise some good aerodynamic characteristics that they might have had if they had not been stealthy. So, physics works. I mean you can't have everything—(chuckles). And if you want to focus, which was important in DARPA programs, you will sacrifice something. And we compromised in each one of those cases.

At any rate, in this other ACAT program, which had the WES simulator, we evaluated not only information technology but other technologies as well. Sea Shadow was probably the main example. But you could evaluate all sorts of things. If you wanted to evaluate a new surface-to-air missile, we could simulate that and put that in. A new, long-range air defense missile? We could do that.

We then found out that when commanders came into the simulation, say Monday morning, and fought the battle for the first time, they lost—(chuckles). The battle unfolded so rapidly, that it was pretty overwhelming in the beginning. But they learned. So it was an excellent training. You might end up Monday by losing two aircraft carriers in the simulation. By Friday, however, most had generally seen how and how rapidly the battle unfolded, became highly skilled again—(chuckles). After the stunning defeats of Monday, to win that battle consistently was exhilarating. It was an excellent training exercise.

Finally this simulator thing was this was connected by a secure link over to the Naval Postgraduate School and Pacific Fleet Headquarters in Honolulu. The PAC Fleet people looked at the DARPA facility carefully and said, "We really need to war game and to check our war plans." And they began to use the facility in that way. Since communications were encrypted, they began to use as a war game simulator to see how their overall battle unfolded, not just the actual air battles but the entire battle. That was serendipitous. They were able to "test" in a simulation those battle plans.

The Navy had a large war game at the Naval War College in Newport — probably still does. But the DARPA was game was at a different level. The NWC game was a big, strategic level where someone even played the President of the United States. Ours was much more feet-on-the-deck, so to speak, and we didn't worry about the lofty—(chuckles)—issues that the Newport people did.

So, we had a different game much to the liking of the warfighters because they could try things in almost the next day, or certainly within the next week. When things didn't work out well, PACFLEET could modify and retest their ideas. So, it was used extensively in that way.

After I left and after some of the people who had done that—probably after Admiral Hayward left—that all went away somehow.

**I:** What'd they find out when they ran their first plan?
**Fossum:** I really don't know too much about that. Those were extremely highly classified sessions, and I felt that war fighting was their business—(chuckles)— not mine.

**I:** Let's talk—about the Internet. ARPA and DARPA had a big role in that.
**Fossum:** Well, there were three basic things that have revolutionized the world. First were the ARPANET and the architecture of packet switches. Charlie Herzfeld signed the first ARPA order to get that started. Of course it expanded all over the place, and it was—it is the modern data architecture that is used.

But at the time I was in, DARPA still had Ethernets. We had all sorts of local area nets—things like that—which could not talk to the ARPANET; and not talk among themselves easily.

Vint Cerf and Bob Kahn had initiated a program to write all the internetting protocols to make transmission of messages through these gateways between each network transparent. I don't know exactly when it started, but it was started about the time I was the Director of DARPA. None of us realized how seminal that work was. We knew how fundamental the ARPANET was, but we didn't realize—at least I didn't. Kahn and Cerf were visionaries and really did understand the technical significance and absolute need for the internetting project.

I'm not sure anyone ever realized that there was going to be a worldwide web as we see it now. But the worldwide web as we know it would not exist had it not been for the work Vint and Bob were doing at DARPA at that time.

If I had to say what the most significant project DARPA had while I was Director that might be it. If you want to know what was world-changing, that was it.

**I:** A project—your biggest challenge?

**Fossum:** Oh, there're a—(chuckles)—few. (Laughs.)

The biggest challenge as far as the day-to-day management was Sea Shadow. The biggest marginal success—I hate to call it failure—was TEAL RUBY. Sea Shadow, as you recall, was a stealth ship that required almost weekly conferences with the OPNAV and NAVSEA people to keep it going.

And I remember well the engine contractor for Sea Shadow. We had to build a ship which was not just stealthy from a radar viewpoint, but it had to be acoustically stealthy. So it was a very quiet ship. That meant that the engines had to have certain characteristics. As I recall DARPA had to argue forcefully—(chuckles)—with the engine contractor about the way we wanted it done, not the way he was telling us he was going to give us the engines. His idea wasn't going to work.

But mainly the program was argued in the Pentagon. In that sense that took the support of Bill Perry. That was just heavy lifting, you might say. And while I could argue forever but I didn't have the leverage to push that program through. But Dr. Perry did have the leverage and he used it. He liked the program and pushed it through.

The other problem project was TEAL RUBY. TEAL RUBY was a classic. It was underbid by the contractor, under funded by DARPA, too ambitious technically, and was basically a "bridge for" test technology to detect airplanes from satellites. TEAL RUBY was basically to detect airplanes. It was a satellite payload in which we were trying to test a set of concepts. In those concepts were not only the concepts of solid state detector arrays but also different types of solid state technology on the focal plains.

All of this was a lot for the contractor to do, and he had other problems. He was a major contractor in Shuttle and large programs like that, so he didn't pay enough attention to TEAL RUBY. The agent had an enormous amount of difficulty working with the contractor. The agent was the Space Systems Command in Los Angeles and they worked hard to try to make that a successful program, but it never really was.

It failed for funny reasons, though. It failed in unlikely subsystems while I was still in DARPA. It failed in places that it shouldn't fail. So, Murphy was alive and well on this program. (Chuckles) So, it was a technological challenge as well as a management challenge. And it was a budgetary nightmare because it overran and overran and overran.

**I:** Some technologies emerge even when DARPA overreaches.

**Fossum:** Yes that's usually the case with DARPA programs.

I'm sure, what followed me after I left. So, I'm not qualified to say what the outcome was. I can only remember the agony we went through; I don't remember the good things that came out of TEAL RUBY. But I'm confident that good technology did come out of it and good ideas did come out of it.

This was a little too much. It's an example of what I call diffused focus. It's all right to have an advanced technology program, but you'd better focus and not try to do too much. Don't dilute the focus and try to do too much, because the probability of success is reduced, therefore, don't dilute it. Too many advanced technology concepts in the same program or on the same test vehicle are a recipe for failure.

So, TEAL RUBY was an example of that.

**I:** That raises a point of DARPA operating on the fringes of technology. The idea of failure. You have a good story there.
**Fossum:** Oh. Probably the best question anyone ever asked me came from a Congressman. In testimony, you could sort of anticipate what each Congressperson would ask. You knew their interests. But this question came out of the blue and it stopped me cold (chuckles). As Director it was the most penetrating and intelligent question I'd ever been asked.

The question was simple: "Dr. Fossum, you have told us all these wonderful successes and they're very impressive and we appreciate that. Now, please tell me how many times you've failed, where you failed and why you failed in technology development. And the reason I'm asking this question is because if DARPA does not work close to the frontiers of knowledge—of technical knowledge and technology—there really isn't a need for DARPA. So, you really do need to be close to these frontiers, and a measure of that is how often you fail."

I must admit I did not have in my hip pocket the "failure chart." (Chuckles.) I guess one doesn't go around with a spreadsheet showing where you failed, why you failed and how much it cost to fail.

So, I answered that, "for the record," which was the standard answer you used when—(chuckles)—you didn't know the answer. That was the most penetrating question I was asked in testimony, ever, and it was a very interesting question.

And it's very true—if DARPA does not fail, it's not working close enough to the technological frontiers. The Services' failure is not looked upon as a favorable thing in the Services. It is accepted in DARPA and should be accepted in DARPA—not for failure's sake, but for the fact that it is inevitable if you're working close to the frontiers of technical knowledge. That was one of the best questions I'd ever had.

**I:** That's one of the best answers to "why DARPA?"
**Fossum:** I think so. I think it is.

I think that, naturally, organizations don't like to fail, but we did. It was the Director's job to observe that a program going to fail in some sense and to stop that program. But overall it was DARPA's job to work in that fourth quadrant, where the risk is high. The technical risk is high. Those are the types of programs we should undertake, but things have a reasonable probability of technical failure.

But at the same time, failure's not the—(chuckles)—objective, obviously. They have to have high payoff. And high payoff, even at high risk, is where DARPA should be.

The Services are not comfortable with that type of program—and rightfully so. An officer does not get promoted for leading a failed technical program.

**I:** The competing identifier of evolutionary technology versus revolutionary technologies. Is that appropriate?

**Fossum:** It's appropriate, DARPA works in several different categories, as I mentioned earlier. There are categories where we simply rack and stack existing technology. In a sense, we made an evolutionary system out of existing technology but it was a brand new military mission that no one had ever thought of before. We just put together a set of technologies and did some very interesting things.

But it was high-risk. It was high system risk. We often had to make systems work together that didn't naturally want to do that. That was a system risk. But it doesn't mean that each component subsystem was a high technical risk. They weren't. They were existing technologies in many cases.

That's evolutionary in terms of technology but revolutionary in terms of the combination of technology and military mission.

The concepts that are really revolutionary are both brand new missions and brand new technology. Those are where DARPA would like to work all the time. But those are "inventions," in a sense, and you can't force inventions. You want to be receptive to inventions and to make sure that you fund those inventions when there is a prospect of doing something militarily good.

The high-altitude, long endurance airplane—(chuckles). . . You know, I only wanted it to fly a week. (Laughs.) And, of course, Global Hawk, which was the eventual system, can fly for a long time over long distances. And that's a new mission. Nobody thought that could be done.

In high-altitude, long- endurance, we always wanted to fly from the United States to any part of the world, hover, do a military mission for a couple of days and then fly home. We can't quite do that yet, but that's what DARPA aims. That would be revolutionary, and it would require technology that's—in terms of engines and fuels structures—difficult to come by today.

**I:** Was there anything that surprised you?

**Fossum:** Were there any threats that surprised us? That's what a Sputnik was.

The Alpha submarine was one advancement which DARPA didn't do anything with. That was a threat to the Navy. The Soviet engineers were smart people. They had good technologists. We used to say the "Soviet Union is a country owned by the military but not run by the military." Thus their best technology was usually military. Our perception was that's where all their effort was put.

They did technologically clever things. They were constantly doing clever things, constantly working around difficult technical problems. In some cases, the solutions were brute-force solutions, but in other cases, very elegant solutions appeared, which first-order physics principles especially where they didn't have the technology to do it any other way. Therefore, they simply thought about the problem a lot and put together an elegant subsystem to do a particular job. Very worthy technical adversaries.

I think of ballistic missile guidance as an example. I think they did an excellent job in inertial guidance systems, and they did it in an elegant way. Looking back on it, it was an obvious way, but it wasn't the way the U.S. did it.

**I:** What was the idea behind DARPA's emerging technology demonstrations?
**Fossum:** The idea behind this was to demonstrate integrated technology. It's one thing to develop technology in the overall scheme, but to push—push concepts ahead as quickly as possible, to get them into the Services, part of the transition issue—they are demonstration programs. They were demonstration programs to show that militarily this was useful.

An example of one of the largest programs we had was called Assault Breaker. The background of Assault Breaker was the following. DoD and DARPA had technology available that could be assembled into a system for putting at risk enemy second-echelon, armored formations in Central Europe in case of a war.

We were conscious of Soviet doctrine of bringing forward second-echelon forces as the first echelon, the main battle, evolved—as reinforcement or as breakthrough forces as the battle unfolded. That meant battalions and regiments of tanks coming forward well behind the main battle zone.

And our concept was to put together a system which would put at risk these armored formations. It was one of the threats that Dr. Perry worried about and the Army certainly worried about.

The first stages of the war were a problem because you didn't necessarily have all the air assets available to you for ground support. The Air Force was fighting the battle suppressing the enemy air and the enemy surface-to-air missile systems. So, a valuable weapon system would be one which would prevent those second-echelon forces from moving forward to the main battle.

That was a tough problem, because we were attempting to attack tank formations 50 kilometers behind the areas where the main battles were taking place.

This was a DARPA, Army, and Air Force program. We established a goal of being able to engage an entire tank company—16 tanks; 16, 20, I don't know what the number was—and destroy in a single attack, at least 30 percent—from 30 to 40 percent of those tanks in a single attack. The Soviet doctrine at the time was when you've lost 30 percent of a force; it's no longer an effective fighting force. And whether that was true or not was not the important thing. The issue was to engage an entire company over a substantial ground area where these tanks were moving forward, put them at risk, and destroy a certain number of those.

Now, to do that would be extremely expensive if you tried to attack the tanks in a one on one attack. Thus the technical problem was to devise a bus which carried a whole passel of smaller anti-tank weapons. The concept was to attack the top of the tank. The top of the tank, in general, even today, is less well armored than the front and sides of the tank.

But what we did was to modify the requirements so that the sub-munitions were transported by rocket launched bus to that area and distributed in a pattern. We knew every sub-munition was not going to hit a tank. But we also knew that if they hit, they would be effective and that there would be a high probability of hitting 40 to 50 percent of the tanks. That's different than a one-on-one battle. We were not trying to hit the individual tank. We were trying to hit the formation and reduce the entire formation by destroying a certain high percentage.

"High percentage" is probably not the right word but enough of the targets to make it difficult for that formation to function effectively as a military unit.

The concept involved booster rockets, airplanes tracking the targets and guiding the booster and dispensing the sub-munitions. It involved clever technology in the sub-munitions. The project engineer was James Tegnelia, who's now the head of Defense Threat Reduction Agency.

Assault Breaker was an assembly of a variety of things of subsystems. That's where the risk was—making these things play together. It was a program which DARPA spent the most money on, but both the Air Force and the Army funded as well. There was good cooperation from both of those Services. And it was tested successfully after I left DARPA.

Was that system deployed? No, but that was an example of technology that found its way into other systems. That was a demonstration of a new concept of attacking an entire formation. And sometimes we'd win that with a certain probability, and sometimes we wouldn't. .That was basically a different concept.

I did like the concept. Except for the radar it was not necessarily the most advanced technology. It was an expensive demonstration system so it could evolve. It could have evolved into an anti-armor system which the Army could have adopted. As it turned out, subsystems formed their way into deployed systems later.

**I:** Sounds like a budget buster.
**Fossum:** Cost was a big problem—(chuckles). You had to watch it because it had a tendency to overrun. Big programs have that tendency. But Jim Tegnelia did a good job on that program. The Services did a good job. Yes, I was certainly was very pleased with it—as was Dr. Perry.

**I:** Two questions. Because of its organization, the program managers are pretty unique. Where do you find these people, and how—?
**Fossum:** The program managers had certain characteristics. They were middle level managers. They came primarily from industry. They were leaders, as I mentioned before, in their technical area. It didn't mean they were all the top technical guys in their community, though I thought some of them were, but they were well known in their communities and were known as good managers.

The next characteristic was they had ideas. They had their own ideas of how to solve some particular technical problem, some system problem or how to solve some military problems.

The problem of finding them. We always had a net out for those. That was always the question I asked my friends in industry "Who can you send to me to do the following?" "I need somebody to do this."

So, we did have an informal net searching for new DARPA people. Since I left DARPA, I am always on the look out for people that would make good DARPA project engineers and I try to get them interested in DARPA.

Finally, we didn't have to recruit too hard. The pay wasn't great or anything like that, but DARPA was an excellent place to work. We had funding to do technical projects and you were expected to do things. But the program managers had to have a sense of urgency. As you commented before, the program manager had to be a fast track-oriented person. If you wanted simply to sit in the lab and do those important projects, DARPA's not the place.

So, we were able to attract people. We had a good reputation. We could generally bring good people in from a variety of sources. All of our people, by government standards, were well-paid. The lowest grade was GS-14 as a scientific officer. All of our military officers were major or above, all the way to bird colonels.

Having the Ph.D. wasn't the big issue, but it helped. And all were just great guys.

**I:** What are the characteristics of a DARPA director?
**Fossum:** Well—(chuckles)—different for each Director. (Chuckles). I believe there're different characteristics for different times. One of the unique characteristics of DARPA as we've said: fast-moving, high quality, the high risk, and requires risk takers. Somewhere prior to the time I was there and early Director set those standards. I can say I believe there were three directors that set these very high standards—Robert Sproull and Jack Ruina and Charlie Herzfeld. And they established, in my mind, the framework in which all subsequent Directors worked.

We had a customer. It was not simply a research or academic organization whose job was to publish papers. DARPA was a research and development organization whose job was to bend metal and get things out into the Fleet, into the Air Force, or into the Army. We did understand that. Those values were instilled by the early Directors.

So the rest of us that followed in those footsteps—they were hard footsteps to follow on. All people have different management styles, but—(chuckles)—it was my hope that I could give freedom to the engineers in DARPA and that they would do the right thing and do it quickly. And I knew that occasionally we'd have to "pick up the pieces." And, sure enough, you had to pick up the pieces occasionally. But when the program managers have the freedom to do good work, they do it.

But the Director has to have high expectations. I think that was the management style that I was comfortable with. That was the reason that I worked well with Bill Perry, who was in a sense, my mentor. Other people liked to dabble in the details but I loved the technical details mainly because I love technology. I'm a bigoted technologist—(chuckles). But I could not—anywhere near—have the depth of knowledge the program managers had. That's the way it is in doctoral education in the United States. Everybody's a specialist. And the DARPA technical program is wa-ay too broad for a Director to be a specialist in every area. So, we have to accept that, establish a trust with your project engineers.

**I:** Sproull. His DDR&E was Harold Brown. And Brown's philosophy was hire good people, give them good things to do and then leave them alone.
**Fossum:** That's exactly what Harold Brown did as SECDEF. Bill Perry is a prime example.

As I mentioned, I heard Harold's voice on the telephone once—(chuckles)—when it was not for discussion but for direction—(chuckles). But most of the time, he let us do pretty much what we wanted. But on the other hand, if push came to shove, we had absolute confidence that we could get support from our superiors. And I hope our people, the DARPA people that work for me—and I think generally any Directors—I've never heard of a case where directors didn't support the program managers pretty much to the hilt. The trust was there.