KEYNOTE

Science & Technology Challenges

Dr. Donald M. Dix
Director, Advanced Technology
Office of the Director
Defence Research and Engineering

USA

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by

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Director, Advanced Technology
Office of the Director,
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Good morning, ladies and gentlemen. It is a pleasure for me to be here for at least two reasons. First, it provides me the opportunity to get out of Washington -- and for those of us who live and work there, a paraphrase of an old line sometimes seems applicable: A day out of Washington is like a day with sunshine. The second reason, and the more important one, is that I have a chance to talk with members of the technical community about my favorite topic -- technology development and demonstration. In that regard, I'd like to focus my comments on some challenges we face in the technology world, and how these challenges may relate to the efforts which are the subject of this conference -- finding options for halon.

Of course, a keynote address on almost any subject dealing even remotely with the Defense Department poses some challenges of its own these days. As you are all keenly aware, we live in an era of remarkable geopolitical change. One consequence of this change is that we in the Defense Department are in the latter stages -- at least I hope it is the latter stages -- of large reductions in our force structure, our expenditures, and our laboratory infrastructure -- including personnel. By large reductions, I mean reductions in the neighborhood of 40% in most areas. Another consequence of this change is that we are placing greater emphasis on upgrading and/or extending the life of our existing systems, foregoing the expense of new ones. Yet another consequence of this change is that the Department of Defense (DoD) is being called upon to be more environmentally responsible. And if all of that is not enough, we are now looking at a somewhat different political direction in the Congress.

From the standpoint of Science & Technology, the impact of all of these factors is sort of a good news/bad news story. The good news is that the Department's leadership has made a concentrated effort to sustain the necessary long term investment, to maximize the productivity of this investment, and to articulate the need for this investment. And this effort has met with some success -- our Science & Technology investment has not decreased substantially over the past few years. The bad news is that, projecting into the future, it seems that sustaining the current level will be exceedingly difficult -- we must be prepared for reductions in our Science & Technology investment.

One might be tempted to say that this backdrop provides the Science & Technology community with a whole new set of challenges -- do more with less, be more accountable, capitalize on dual-use aspects of technology, be more environmentally conscious, and so on. Indeed, this view may have graduated to the category of conventional wisdom.

However, I believe the broader challenges facing the Science & Technology community have not changed significantly -- the broad challenges are eternal, as it were. At the risk of oversimplification, I believe these broad challenges are only two in number. The first is relevance and the second is application of resources. Meeting the challenge of relevance demands that we demonstrate that our efforts have a high probability of offering significant benefits to some segment of society -- in the Department's case, this segment is reasonably well defined. Meeting the challenge of application of resources demands that we employ the proper resources -- from government, industry, and academia -- in the pursuit of specific endeavors. As I said, I believe these challenges are essentially invariant with time. But certainly our current situation lends more urgency to the task of dealing with them.

For practical purposes, I could conclude my remarks right here -- since the remainder is merely an expansion of some of the details of these challenges. But since I know you want to hear more -- and you're a captive audience to boot -- I'll continue.

The first challenge -- relevance -- can be met by answering four straightforward questions about any technology effort. (Parenthetically, I make no apology to those several of you in the audience who have heard them before.) The first three are the highest level questions:

- What are we trying to do?
- By when?
- What difference will it make?

And at the next level:

- Why do we think we can do it?

For specific undertakings, the first three questions can and should be answered quantitatively. The fourth can be answered in terms of rigorous scientific and/or engineering principles. The answers to the first three questions, of course, both bound the effort and enable value judgments to be made on its potential benefits. The answer to the fourth question enables technical judgments to be made on the likelihood of success. Taken together, the answers to these questions provide the basis for relative investment level.

Although these questions are simply stated, the answers do not come easily. Too often, the first two questions -- what are we trying to do? By when? -- are answered in terms of making something better by some unspecified amount at some unspecified time in the future. Clearly, answers of this nature do not serve any useful purpose, since everyone already knows we are trying to make something better in the future -- quantitative goals and specific timeframes are a necessity.

For reasons that may be understandable, the technology community many times has a reluctance to establish quantitative goals and specific timeframes. some of these reasons appear to be: (1) aversion to the risk of failure, (2) fear of stifling creativity, and (3) reluctance to abandon paths of research that would be excluded by total commitment to the goals. I do not believe any of these reasons are sufficient. One of the beauties of technology work is that failure is rare -- the only way you can do it is by not achieving a goal, and simultaneously not understanding why you didn't achieve it. So, we should have little fear of failure. With regard to stifling creativity, I believe that if the goals are set properly, they will in fact foster creativity -- after all, necessity is still the mother of invention. So we have no legitimate worry on this score. Finally, a total commitment to goals will indeed foreclose some paths of research; on the other hand, continued pursuit of such paths would constitute a waste of one's time, and who wants to do that?

The third question -- what difference will it make? -- requires answers of two different characters. First, the difference it will make needs to be expressed in terms that the potential users of the technology will understand. In DoD parlance, this usually means a difference in performance and/or cost characteristics of a system -- more range, more payload, less signature, less cost, etc. -- or, to take the case in point, the elimination of halons.

The second part of the answer is to establish that the technology is likely to be used in a timely manner. Again in DoD parlance, this means that we must provide the opportunity to get usable products into the hands of the warfighter more quickly. This not only requires a suitable transition target, but also that technology outputs are demonstrated to a point where the risk is acceptable for introduction into a system development.

The fourth question -- why do we think we can do it? -- is, of course, a technical question that demands a technical answer. Since all technology efForts involve trying to do what has not been done before -- or stated another way, trying to do what you don't know how to do -- some evidence of possibility must be provided. Again, this is sometimes difficult to extract. And it should not be -- after all, it is at the core of our professional life.

I offer a little side challenge to each of you for the next few days. Judging from the conference program, you are going to hear a lot about current technology efforts and future aims. I would challenge each of you not to let any presenter off the hook until he or she has either answered the four questions to your satisfaction or admitted that they don't know the answers. It will be a mutually beneficial undertaking, because it is important that all of us involved in these efforts understand the answers. Without the answers, we literally do not know what we are doing, and the area is too important for this to be acceptable.

Let's turn briefly to the second broad challenge -- application of resources. Meeting this challenge first requires that all interested parties share a common goal, or goals. This includes government, industry, and academia as appropriate. Without common goals, it is difficult to see how our collective resources can be used wisely. A second requirement is that investments by the participants -- particularly government and industry -- are roughly in accord with the respective benefits. In the Defense Department, this immediately raises the question of dual-use technology. Our challenge here is twofold. First, in those areas where military applications drive the technology, we must ensure that the technology is amenable to its civil uses --only in this manner can we attract the necessary civil investment and, further, capitalize on the larger production base that civil application offers. Second, in those areas where civil applications drive the technology, we must adjust our efforts to ensure that we can capitalize on its use. Clearly, meeting the challenge posed by application of resources requires considerable effort.

Let me move now from the general to the specific -- the Department's search for alternatives to halon. Our major current effort is in trying to achieve the goal of the Technology Strategy that we issued almost three years ago. This Strategy answered the questions of what we are trying to do and by when. Specifically, we are trying to identify and/or develop feasible alternatives that would permit the elimination of halons used in weapons systems for five purposes:

- Fire extinguishment in occupied spaces by 1996
- Fire extinguishment in unoccupied spaces by 1996
- Explosion suppression in occupied spaces by 1996
- Explosion suppression in unoccupied spaces by 1996
- Thrust control in booster rockets by 2000

We define a feasible alternative as one for which the relationship between weight, volume, cost and effectiveness is known, and for which the penalties for use -- as measure by these parameters -- is not "excessive" when compared to halon.

The answer to our third question -- what difference will it make? -- is rather self-evident: it will provide the opportunity to eliminate halons, albeit at some cost. And we have identified potential applications for transition -- for example, in three developmental aircraft: the Marine Corps tilt-rotor V-22, the Navy upgrade F-18E/F, and the new Air Force fighter F-22.

The answer to the fourth question -- why do we think we can do it? -- is imbedded in our TECHNOLOGY DEVELOPMENT PLAN, or TDP for short, with which many of you are familiar. It identifies potentially viable alternatives for total flooding, streaming, and thrust control that we are evaluating through laboratory and full-scale testing.

Some of the more promising alternatives under current investigation:

- o FE-25 for aircraft engine nacelles (V-22 and F-22 applications)
- o FM-200 for ship occupied propulsion equipment spaces
- o Inert gas generation for unoccupied aircraft spaces (V-22 and F-18E/F applications)
- o Fine water mist for ship occupied propulsion spaces, and unoccupied aircraft and ground combat vehicle spaces
- o Powders/hybrids for ground combat vehicle engine spaces
- o Perfluorohexane for streaming agents and thrust control (the later involves Minuteman applications)

Will all of these alternatives eventually prove to be successful? Probably not. But I am confident that a sufficient number of them will prove to be successful.

So I think it's fair to say that with our Strategy and TDP, and the current state of execution, we have met the two broad challenges, and the prospects for success are high. However, the goal of these efforts is to find alternatives for halon quickly, and we remain committed to that goal. Simultaneously, we recognize that it may be possible to develop technology for more optimal solutions, given more time for research.

So we have taken the first step beyond our current efforts. We have begun constructing what we call a Next-Generation Fire Suppression Technology Program, or NGP for short. Dr. Dick Gann from NIST will be presenting this in more detail later in the conference, so I am not going to steal his thunder. But I will briefly address how we think we are meeting the two broad challenges.

First, the four questions, which by now I assume you all remember. We are trying to identify and/or develop fluids, processes, and/or techniques that will result in explosion suppression and fire extinguishment that is approximately as good as Halon **1301** when used in weapons systems. And we are trying to do it by 2004. The difference we expect to make is that we will have alternatives that not only have less weight, volume, effectiveness, and cost penalties than the alternatives that will emanate from our current TDP efforts, but will be more easily incorporated into existing weapons systems. A difficulty with the alternatives likely to result from the TDP activities is that they may not be readily amenable to incorporation in existing systems -- they tend to require more volume for the same effectiveness, and such volume is not generally easy or inexpensive to obtain in an existing system. Inasmuch as we anticipate that many of our existing weapons systems will be around for a considerable time, it is important that we make elimination of halon via retrofit as easy as possible.

Why we think we can do it is best answered by looking at the major elements of the **NGP.** There are six elements:

- o Risk Assessment and Selection Methodology
- o Fire Suppression Principles
- o Technology Testing Methodologies
- o New Suppression Concepts
- o Emerging Technology Advances
- o Suppression Optimization

What these elements mean to me is basically threefold in nature: (1) that we believe there may be new compounds that potentially have the same effectiveness as halon; (2) that through a greater understanding of fire suppression principles we may be able to come up with "designer" compounds that are as effective as halon; and/or (3) that maximizing the efficiency of fire suppression may result in system solutions that are as effective as halon-based systems, though the compounds used may be less effective.

Hence, I believe we have answered the four questions adequately -- although the answer to the fourth one should undoubtedly be strengthened. Hopefully, your activities of the next few days will contribute to a stronger answer to why we think we can do it.

The second broad challenge -- application of resources -- we intend to meet by ensuring that our best and brightest researchers -- hopefully this is not a pejorative term -- have the opportunity to participate in achieving the common goal, and also ensuring that government, industry, and academia efforts are well integrated.

Obviously there are several pitfalls -- technical and otherwise -- along this road. I don't believe it is necessary to dwell upon them. However, one pitfall I would like to emphasize is that of considering our Next-Generation Plan a generic fire research program -- because it is definitely not that. I am sure that the need for generic fire research will continue long after the year 2004. But our NGP is not pursuing the perfect truth; rather we are pursuing that relevant truth, or imperfect truth, or practical truth that will enable us to achieve our goal. This will require commitment and focus on the part of all of you who participate. I am sure that you will be able to meet this requirement -- after all, it is only a matter of answering four questions.

You have my best wishes for a successful conference, and beyond that, for developing alternatives for halon.

Thank You.