Statement of Richard L. Wagner, Jr., Ph.D.
Hearing of the Senate Committee on Homeland Security and Governmental Affairs titled "Preventing Nuclear Terrorism: Hard Lessons Learned from Troubled Investments",
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Chairman Lieberman, Ranking Member Collins, and distinguished members of the Committee, I am honored to be with you today to address the important issues raised in your charge to this panel, which I will summarize below. My brief general biography is attached to this statement, along with some of my experience specific to the topics before us today.

I am currently affiliated, on a part-time basis, with the Los Alamos National Laboratory, although I have lived and worked in the DC area for twenty seven years. I also have the honor to chair the Nuclear Defense Working Group, which is composed of several senior former national-security policy officials and scientists from universities, national labs, and industry. The NDWG is chartered to advise departments and agencies, including DNDO, and the Congress, on nuclear terrorism. It is supported by a grant from a private foundation to the Center for the Study of the Presidency, a non-profit, public interest organization headed by Ambassador David Abshire. Over the past eighteen months, the NDWG has studied many aspects of prevention of, and defense against, nuclear terrorism. I have not, however, had time to discuss what I will say here today with my NDWG colleagues, though my views have been shaped by earlier discussion with them.

Because of this, and because I am partly retired and much of what I do in this area I do on a pro bono basis, I speak to you today as a private citizen.

I will address the three specified topics of interest for this hearing – all related to the efficacy of managing R&D and the transition from R&D to fielded operational capabilities -- both directly, and indirectly. I will start first by saying a few words about the Advanced Spectroscopic Portal (ASP) program. I will then discuss the prospects and programs for developing radiation detection systems more capable than ASP and for a broader array of applications. For both ASP and more advanced R&D, I will address management issues. I will conclude with some comments about the scope and nature of the responsibilities of DNDO, again in the spirit of making the best progress possible in moving the nation toward better capabilities to deal with this threat.

<u>The Advanced Spectroscopic Portal (ASP) program</u>

The first topic posed for this hearing is "DNDO's emphasis on the ASP". One aspect of this is whether DNDO (or DNDO as the implementer of broader administration priorities) has put the right amount of emphasis on ASP – on an absolute scale and relative to other things.

Smuggling nuclear weapons or materials in the cargo flow of international trade is only one way by which weapons could be moved clandestinely. No one can predict what the most likely attack-path or combination of paths could be, especially when both the attacker's and the defense's operational capabilities, and uncertainties in them, are taken into account.

I believe that cargo-flow – and thus ASP -- was the right place to start. Operational aspects of cargo screening are simpler than for other paths, they are close to home and under our control and thus easier to address first, and lessons will be learned from ASP/cargo-screening that will be applicable to other attack-paths. Over the past eighteen months or so, DNDO and others have correctly begun to put more emphasis on other paths. I would have liked to have seen this done sooner, but an opposing argument is that, within then-available resources, that would have slowed ASP, with the possible result of a net delay in overall protection. Thus it becomes a question of the overall level of resources. In general, I believe that the nation should be putting even more resources on this threat, including but not limited to detection and interdiction of attacks, but to support that position is a much bigger topic than can be covered in this hearing.

What about the ASP program itself? I am not fully current on the most recent developments in ASP, but from past looks at it, I believe I can address some of what this committee wants to explore.

The basic technologies embodied in the ASP developments are not new. But widespread application of them to cargo screening *is* new. Isotope identification using gamma-ray spectroscopy, although not new, is complicated in the particulars of its application. So the ASP development should be, and should have been, understood as more developmental in nature than like quantity procurement of an understood item of hardware.

Getting spectroscopic isotope identification of some sort into the field for cargo screening is absolutely essential. Although some marginal improvement in the currently deployed, non-spectroscopic screening detectors might be possible, they will never be able to do the job that spectroscopic detectors will be able to do. Current screening operations appear to be swamped with false alarms. Isotope identification holds the promise of making cargo screening meaningful. And although ASP, even if fully implemented, would be far from "solving" the cargo-screening problem, it would provide a much better basis for continued improvement. Getting operational experience with spectroscopic screening will also inform and shape the entire R&D program for further mid-term and long-term improvements in radiation detection for all applications.

The wide range of possible threat objects and operations is not easily reduced to a simple specification, and even if it were, a specified test program cannot adequately address performance. The main lesson from the long history of operational detection of radioactive objects in the field is that performance of operational detectors gets much better as the operators gain experience -- maybe even somewhat better than would have been anticipated on paper in advance. For these several reasons, it is more important to <u>understand</u> how a detection system performs against a *range* of threats in actual operations than to <u>demonstrate,</u> in tests, performance against a specification.

Therefore, the approach to ASP should be, and should have been, an "expanding spiral" approach – time-phased procurement and field operation of, at first, small, and then increasing quantities of units, coupled with spiral improvement of the identification software. The early stages of this would do two things: start a process of improving performance of fielded systems, and provide lessons that are fed back into improving the software of succeeding spirals. To make an expanding-spiral approach work, two things are needed: developers must work closely with users in the field in doing real screening operations (and modifying operations as experience is gained), and provision must be made for rapid upgrade of the software in succeeding spirals. I am agnostic on whether the initial, small-scale deployments I suggest should have been started a year ago, or rather should be started perhaps a few months from now.

My impression is that during the past year or so, the ASP program has morphed into something a little like what I suggest here. It would have been good to have understood at the beginning that it should be done this way.

It seems to me that the routine, quantity-procurement-like nature of the ASP contracts has hindered the ability to carry out the approach I describe. The "First Law of R&D" is that one cannot specify -- in advance and together – performance objectives, cost, and schedule. If one could do that, it would not be R&D. The early stages of an expanding-spiral process will be more R&D-like; and the latter stages, more procurement-like. Flexibility in contracting is essential, and I will discuss it more, later. The disparity between 1) the expectation of a one-time, test-then-buy-it-all approach for ASP and 2) the expanding-spiral approach that the realities dictate has introduced both heat and noise into the interactions between DHS and the cognizant committees of Congress, to the detriment of both program progress and Congressional oversight.

An obvious question, and one that has been asked, is whether detection systems with better capabilities than ASP will come along soon enough to make ASP procurements wasteful. I will address that in a minute, in the context of my next topic: detection R&D in general.

For ASP and beyond: Managing advanced development of future radiation detection technologies in general

The role and prospects for radiation detection

Before addressing the *management*, per se, of development of advanced radiation detection technologies, let me say a word about the role of radiation detection in general in defending against smuggled weapons.

Improvement in detectors is very important, but many other things contribute a lot to overall defense effectiveness, including intelligence and law-enforcement operations, surveillance with other types of sensors, preparing to augment defense operations in response to warning, covert/special operations and other military operations, both US and others, overseas, at sea, and at home, and much more. For short, I will call these other means nuclear counter-terrorism (NCT) operations.

There can be a two-way reinforcing synergy between improved radiation detection and these other means of contributing to discovery and interdiction of these threats. Structuring NCT operations properly can make radiation-detection operations more effective, and better radiation detection can make NCT operations more effective. For a variety of reasons, I cannot go into any detail about such operations here, but understanding them is essential to an understanding of the utility of detector R&D.

Furthermore, improved detection and operational capabilities can increase the very real possibility of discouraging attacks by creating uncertainty about the prospects of success --or, better, certainty that attacks will *not* succeed – in the mind of the (prospective) attacker. ASP itself seems to me to significantly raise the level of uncertainty for an attacker who is trying to assess the performance of a cargo-screening system. Not only will performance be better, but assessing it requires knowing something about the software, which is complicated, with many variables and options. In contrast, any competent physics undergraduate should be able to assess the performance of non-spectroscopic detectors. There is a lot more to be said about discouraging attacks, but – as with operations -- it is beyond the scope of what I can say here.

Three to five (or ten) years ago, when a handful of us were arguing for an expanded national program focused specifically on dealing with this threat, the nation's S&T community had never been harnessed to the detection problem (in part because the threat had seemed to be real only in some conceptual realm outside the actual world, especially before 9/11). Our sense was that, if that community – in universities, national labs, and industry -- were to be energized and supported, a lot might come of it. (We also felt that we would never know until we tried – in part because the other, more operational aspects of detection and interdiction put the prospects beyond the ability of analysis to predict, at least at the time.) Today, DNDO, NNSA in DOE, DTRA in DOD, and others have gone a long ways toward re-vitalizing and expanding the S&T community in this area – a big accomplishment in its own right. There is still a lot more to be done, but a good start has been made.

I believe that large improvements in detector performance beyond ASP are possible, and there is ample supporting evidence for this in the advanced development programs that DNDO, DoD, and DOE have underway. By large improvements, I mean factor-of-ten-ish, although quantitative metrics for detector performance are not yet well developed.

So, overall, my general sense is that sufficient improvement can be made in detectors themselves so that the mutually-reinforcing combination of detector- and operations-improvements can raise the level of defense capabilities high enough so that many prospective attackers will be discouraged, and there will be a substantial capability to interdict those that are not. This is of course not a guarantee, but the combination of the stakes involved and the likelihood of success make a substantial expenditure worth the bet.

So will even better detector technology come along soon enough to make ASP procurements wasted? I hope so, and the agencies involved should be working hard to make that happen. (Contributing to its happening could be experience gained from the early stages of ASP expanding-spiral deployment.) *But I would not count on it.* One merit of the expanding-spiral approach I suggest for ASP is that it can be interrupted whenever it becomes apparent that something better will be along soon.

<u>Managing the progression from advanced research to fielded operational capability.</u>

Whenever better technology comes along – and there will be three or four future "generations" of improvement -- it will be important to get it to the field as quickly as possible. This will require a business model that effectively spans the progression from advanced research to procurement.

Spanning this range is not a new problem, nor is it unique to detector R&D. It has been a constant theme through my career in the science and technology of national security. And it exists for every application of science and technology, within and outside the area of national security. Libraries could be filled with what has been written about it. There is no good way to "solve" it, but there are some approaches that are better than others. The preferred ways are almost always case-specific, but there is a menu of approaches that can be used in different mixes for different cases. That menu includes:

- Involving the work-performers as fully as possible in defining the work to be done.
- Making the work-performers an integrated team, including spanning the range from research to manufacturing.
- Ensuring close connections between the technical developers and the operational users, so that technologies and operations can be improved together.
- Enabling the teams mentioned above to tailor the problem to be solved to the feasibility of solving it.
- Ability to change directions quickly.
- Willingness to carry parallel approaches simultaneously, knowing that most of them will be abandoned and the money spent on them will (seem to) be wasted.
- Sole source contracts; long-term contracts; level-of-effort contracts.
- Being willing to spend money to shorten time – for example by concurrency in component development

Some or many of the things I list – and certainly all of them together -- might come under the rubric "bending the FAR" (Federal Acquisition Regulations). And using tools such as these effectively requires a special kind of government oversight. The government must lead more and manage less, and it must allow others to help it lead and manage.

People often talk about the need for "a Manhattan Project" to solve some particular big national problem, and the term has sometimes been used about what we are talking about here today. It would take a book (and many have been written) to describe what made the Manhattan Project particularly effective. The kinds of things I have listed above are among them, I believe. (I was in grade-school during the Manhattan Project, but some of my mentors during my early days at Livermore had worked in that world, and the national labs in general were started in that model.)

The way the nation used the national labs, during the first decades of their existence, comports with much of what I am trying to say here. Use of the national labs *as national labs* is eroding – and at an increasing rate -- but some of the art is still there. It is still there in the Navy's nuclear programs, and in a few other places, but it is eroding almost everywhere government is involved.

Some of the things I list above appear to be cost-inefficient. This may be true in the short run. But in the long run, one will get to an ambitious long-term goal sooner and probably cheaper this way. And if one demands short-term cost efficiency, one is likely never to get to where one needs to be. (Similar things could be said using the useful terminology of risk and risk-management.)[1]

A savvy government program manager, today, is hesitant (to say the least) to use such tools because he will judge, usually rightly, that today's "culture of compliance" will extract a price from him and his program that is greater than what he might gain from using them.

(On a personal note, when I say such things about the management of R&D and transitioning it to procurement, as I often do in a variety of contexts, I am usually accused, politely but somewhat dismissively, of living in the past. Today's managers believe that today's realities – the pervasive culture of compliance – are simply unable to accommodate such approaches. That may be true, and no doubt the past was not as rosy in this regard as I may be remembering it. But….)

But I refuse to believe that nothing can be done, and in the area we are addressing today, this committee may be able, in a variety of ways, to help managers who want to manage the transition from advanced research to fielded capability better. I do not have a detailed prescription for how you can do that, but one way is simply to provide top-cover for managers who want to use such tools.

One charge to the panel of which I am a part here today was to address "*near-term* options for improving the outcomes of DNDO's major investments in radiation detection technology". I don't know whether the expanding-spiral approach for ASP can be implemented, contractually or programmatically, in the near term. I hope it can. Nor do I know how long it would take to begin to allow the entire ensemble of R&D programs to

[1] Although it is not the topic for this hearing, I believe, from looking at a small but representative sample of individual R&D projects sponsored by DNDO, DTRA, and NNSA, that this area of R&D is under-funded. Government program managers and principle investigators outside government are too often forced to select among approaches before each has been carried far enough to determine feasibility or suitability, and once a particular approach has been selected, the components needed for it are too often forced to be developed in series rather than in parallel, which hinders design integration and delays the final product.

be managed in the ways I have suggested. But I do believe that it's important to start to try now.

Turning more specifically now to DNDO, one of the three topics announced for this hearing was "the extent to which DNDO has improved government-wide management of federal research and development to improve radiation detection technologies". If this means improved management and contracting *methods* of the sort I have discussed above, then I neither believe that DNDO was charged with this responsibility nor am I aware of any contracting or management innovations that DNDO has developed. But if the question is simply whether radiation-detection R&D is going better now, government wide, because of DNDO, the answer is "Yes". DNDO has been pro-active and generally effective in promoting this R&D across government, in gaining funding for it from the Congress, and in coordinating it among the departments and agencies involved.

<u>Metrics, models, and system analysis</u>

In addition to the right business models and approaches, greatly improved metrics and systematics for detector performance, and better concepts and models of how detector performance plays in the larger array of approaches to preventing successful attacks, including operations and architectures, are needed urgently. DNDO, NNSA, and DoD are working on such things, but it seems to me to be coming slowly. Once again, there is a lot that could be said here, but it differs in tone from what seems to me to be the main thrust of this hearing.

Over the coming years, I believe that acquisition and procurement of detection capabilities will naturally improve – will get more regular -- and thus more "over-seeable" by the Congress. This will happen as system analysis improves, making performance of advanced detectors more predictable, and as detector technology itself gets better, making detector performance less dependent on operator experience.

But this is a very difficult area, and I think that the Congress should expect a substantial fraction of the programs and projects not to be successful. If they were all successful, then they would not be reaching far enough.

<u>**The scope and authorities of DNDO**</u>

As I saw it at the time, the main reasons for setting DNDO up as it is were to:

- Have an organization in the government focused exclusively on the nuclear terrorism threat.
- Increase the attention paid to that threat.
- Get new technology to the field quickly by spanning the range from exploratory research to procurement, operations, and architectures.
- Connect DHS activities in this area to other departments

I agreed with these goals and with the structure of DNDO at the time. Three or four years later, I see nothing in the performance of DNDO, or in the larger arena it is a part of, that changes my judgment.

Just as contracting models for spanning the range from research to procurement are difficult to optimize, so there is no really *good* way to organize responsibilities for doing it. But I believe that there is more advantage in spanning that full range in a single organization than in splitting it up into several organizations. Of course, in an immense enterprise like DoD, there is a place for organizations like DARPA, which focus mainly on the early stages of R&D. But most acquisition organizations in DoD span most of the range. At the other end of the spectrum, a very small enterprise in a large department might profit from having its researchy things "done for it" by an organization separate from itself which can integrate its R&D with R&D for other enterprises. But DNDO's enterprise is not that small. So, from looking at the problem in this general way, DNDO was correctly set up to integrate the range from research to procurement, and to include architectures as well.

There have clearly been problems in getting the detection-and-interdiction enterprise going. There have also been significant successes, both in R&D and in operations. It is an extremely tough job. (Defense against this threat is conceptually more difficult to grapple with than, for example, missile defense, with which I have been closely involved several times over the past forty years). It is also an extremely important job, and I'd like to see it coming along faster and better. So, is there a way to aggregate or disaggregate responsibilities better in this particular area?

I have refrained, today, from mentioning the views of the NDWG because I have not had time to vet what I am saying here with my colleagues. But on the point of the scope and responsibilities of DNDO, I will mention some of the thinking we have done together. We have discussed various possibilities for re-shaping DNDO or something like it so as to get the overall job, government-wide, done better. Alternatives we discussed include 1) narrowing the focus of DNDO to only advanced development, or perhaps to R&D in general, with reduced or no responsibilities for procurement and/or architectures, and, conversely, 2) retaining DNDO's responsibilities for the full range of R&D, and expanding its authorities for procurements, operations, and architectures. Although we are not unanimous in this, our general consensus is that either of these alternatives loses more than it gains, and that the way DNDO was scoped initially is still about right, at least for the next few years.

From a national perspective, there are serious problems in integrating programs and operations in the area of nuclear terrorism – especially operations -- among/across all of the departments involved. One approach to helping with this, within DNDO, is to populate DNDO with people from other departments. This, and DNDO's efforts in general, are helping, but the problems and approaches to government-wide integration generally lie above the level of DNDO. And many of the integration problems are not unique to countering nuclear terrorism, per se. I have some opinions on how to deal with all of this better, but they do not involve re-structuring DNDO, and this important subject may lie beyond the scope of this hearing.