



OFFICE OF THE SECRETARY OF DEFENSE
1950 DEFENSE PENTAGON
WASHINGTON, DC 20301-1950

MAY 13 2010

ADMINISTRATION AND
MANAGEMENT

Ref: 09-A-1349

Mr. Steven Aftergood
Federation of American Scientists
1725 DeSales Street, NW, Suite 600
Washington, DC 20036

Dear Mr. Aftergood:

This responds to your September 4, 2009, Freedom of Information Act (FOIA) appeal. You appealed the August 31, 2009, determination by the Office of the Director of Defense Research and Engineering to partially deny your July 6, 2009, FOIA request. After reviewing your request, I am granting your appeal. Mr. Alan Shaffer, Principal Deputy Director, Defense Research and Engineering, has requested that a copy of the memorandum for public release be enclosed with the requested document. This action closes your appeal.

Sincerely,

A handwritten signature in black ink, appearing to read "James P. Hogan".

James P. Hogan
Chief

Enclosure:
As stated



DIRECTOR OF DEFENSE RESEARCH AND ENGINEERING
3030 DEFENSE PENTAGON
WASHINGTON, DC 20301-3030

April 12, 2010, 11:00 am

MEMORANDUM FOR: Public Release

From: Principal Deputy, DDRE

SUBJECT: FOIA 09-A-1349 Appeal to the JASON Summer Study: S&T for National Security

As part of a broader ongoing effort to evaluate and assure the health of the overall science and technology capability of the Department of Defense, the office of Defense Research and Engineering funded a report by the JASONS in the summer of 2008. This report, one of several done externally, provides one perspective based on the JASONS reviewers' professional experience and data collected by the JASONS. Due to the compressed term of the study, comprehensive data on the over 5,000 grants conducted in sponsored basic research could not be provided or assessed, relative to a more complete study. Nevertheless, many of the report's conclusions and recommendations were useful and are being studied or implemented.

Looking more broadly, this JASON report, along with a previous report from the National Academy of Sciences and an upcoming study by the Defense Science Board, are being used by the Department to monitor and address any uncovered weaknesses in its basic research programs. A wide range of direct interactions with Universities and laboratories is also being undertaken.

We within the Office of the Director, Defense Research and Engineering welcome a variety of views and analyses on this and other topics to make sure that we see the challenges and opportunities from all sides, in order to provide the strongest possible scientific foundation for future military success.

A handwritten signature in black ink, appearing to read "Alan R. Shaffer", with a long horizontal flourish extending to the right.

Alan R. Shaffer

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S&T for National Security

Contact: D. McMorrow - dcmorrow@mitre.org

May 2009

JSR-08-146

Distribution authorized to DOD only; Specific Authority; May 9, 2009.
Other requests for this document shall be referred to Department of Defense.

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14. ABSTRACT This study focuses on how best to structure basic research (BA1 or 6.1) within the DoD. The changing national and global context for basic research is reviewed and the rationale for basic research within the DoD is discussed. The present organizational and funding status of DoD research is also reviewed with particular emphasis on the role of DDR&E and observations about the program, personnel, and organization are offered. Recommendations are made aiming at bringing greater visibility and coherence to the BA1/6.1 program, improving the quality and connectivity of the DoD Lab and academic communities, and developing a high-quality S&T workforce.					
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Abstract

This study focuses on how best to structure basic research (BA1 or 6.1) within the DoD. The changing national and global context for basic research is reviewed and the rationale for basic research within the DoD is discussed. The present organizational and funding status of DoD research is also reviewed with particular emphasis on the role of DDR&E and observations about the program, personnel, and organization are offered. Recommendations are made aiming at bringing greater visibility and coherence to the BA1/6.1 program, improving the quality and connectivity of the DoD Lab and academic communities, and developing a high-quality S&T workforce.

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EXECUTIVE SUMMARY

This JASON study was chartered by the DDR&E to consider how basic research (BAI or 6.1) should be structured within the DoD to best meet the challenges ahead.

The context for DoD basic research is changing rapidly because of changing global circumstances, changing National Security missions, the accelerating pace of technology advances, the globalization of technology, the rise and spread of commercial vs. defense technology that dilutes DoD's influence, and improvements in the global technical talent pool. JASON finds that significant changes in the DoD S&T program are required to respond to these drivers.

A vital DoD basic research program is important to advancing a number of DoD-unique fields, to attracting and retaining a high-quality science and engineering workforce, and to maintaining an awareness of (and readiness to exploit) fundamental advances in an increasingly global research enterprise. The common judgements of low return on research investment and "we'll buy results when we need them" are shown to be false.

The present organization of basic research in the Department can be characterized as program management and execution by the services, with certification, representation, and a relatively weak review and coordination provided by the DDR&E. While this allows the services to "own" their individual programs, it makes coordination and synergies less likely, and renders the basic research program susceptible to a "drift" away from long-term imperatives to short-term needs. Indeed, the extraordinarily productive DoD tradition of knowledgeable and empowered program managers supporting the very best researchers working on the most fundamental problems has morphed during the past decade into a more tightly managed effort with a shorter-term and more applied character. In the present program, evolutionary advances are the norm, and revolutions are less likely to be fostered than they should be. While it is gratifying to see that current and projected future budget requests allocate more money to basic research, such increases alone will not fix this problem. Rather, systemic and institutionalized changes in process, organization, and personnel are required.

The study's most fundamental recommendation is to protect 6.1 funding at the OSD level by strengthening and expanding the role of the DDR&E, with a greater visibility in the Department and greater capability to understand and shape the services' 6.1 activities. The creation of a Basic Research Advisory Committee comprised of qualified DoD and external personnel would also help in this regard.

To address some of the endemic personnel issues in the DoD, we recommend that a Research Corps be established within each service. We also suggest that the DoD labs, while focusing principally on activities that are 6.2 and later, should also house some researchers engaged in 6.1 activities that are well-coupled to the broader communities.

The DoD is not adequately participating in the development and maintenance of the S&T educational pipeline. Beyond enhancing the existing mechanisms of graduate student and

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postdoctoral support, the use of training grants and vertically integrated models could be explored.

To improve the coupling of DoD to the academic community, we recommend measures to expand and improve the new National Security Science and Engineering Faculty Fellowship (NSSEFF) Program and other steps to improve connections to DoD-supported university faculty.

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1 STUDY BACKGROUND AND METHODOLOGY

This document reports the results of a study chartered by the DDR&E. We had a broad, informal charge that we interpreted to be:

How should DoD Basic Research be structured to best meet the challenges ahead?

Particular dimensions that we thought useful to discuss can be organized around

- **Program** (Is DoD basic research focusing on the right areas? Is it well-coupled to the forefronts of the various fields? Is there good coupling of the basic research to its applications?),
- **People** (Is the workforce adequate and being used effectively? What are the best ways to generate, attract, and retain the best workforce?), and
- **Organization** (What is the proper relationship between DDR&E and services? Is there proper oversight and coordination of research activities? Is DoD basic research well-coordinated with other research activities within and beyond the government?)

Overall, we attempted to identify what would be required to ensure a robust DoD research enterprise of appropriate scale, quality, and breadth.

Note that we have focused our considerations on "basic research", which has a particular meaning within DoD. Specifically, the first two Budget Activities, of the seven that comprise RDT&E activities, are defined by OMB as:

BA1(=6.1) Basic Research. *...systematic study directed toward greater knowledge or understanding of the fundamental aspects of phenomena and of observable facts without specific applications towards processes or products in mind. It includes all scientific study and experimentation directed toward increasing fundamental knowledge and understanding in those fields of the physical, engineering, environmental, and life sciences related to long-term national security needs. It is farsighted high payoff research that provides the basis for technological progress.*

BA2(=6.2) Applied Research. *Applied research is systematic study to understand the means to meet a recognized and specific need. It is a systematic expansion and application of knowledge to develop useful materials, devices, and systems or methods. It may be oriented, ultimately, toward the design, development, and improvement of prototypes and new processes to meet general mission area requirements. Applied research may translate promising basic research into solutions for broadly defined military needs, short of system development. ... The dominant characteristic is that applied research is directed toward general military needs with a view toward developing and evaluating the feasibility and practicality of proposed solutions and determining their parameters.*

This focus on basic research stems primarily from the study's limitations of time and capability. However, we believe that many of the things we have to say apply to the broader S&T activity as well (*i.e.*, to Applied Research and Development).

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Given the time and manpower available, our response to the charge was rough and ready. No new systematic surveys were undertaken, but rather we relied on discussions (in-person, telecons, and emails) with DDR&E and DoD personnel.¹ We also reviewed a number of prior reports on this subject.² Perhaps most importantly, we relied on our extensive experience as practitioners and managers in the progress and the forefronts of science and engineering research. And we have also drawn upon our knowledge of academia, industry, and the needs, organization, and culture of the DoD and other Federal research agencies.

The report is organized as follows. In Section 2, we review some context for DoD Basic Research. Section 3 offers a rationale for DoD basic research, while Section 4 reviews some of the basic facts and operation of the 6.1 programs within the DoD. In Section 5, we make some observations about DoD basic research, grouped around the themes of Program, Personnel, and Organization, and we conclude in Section 6 with recommendations grouped in the same manner. We have attempted to keep the main report concise, placing supplementary material in appendices.

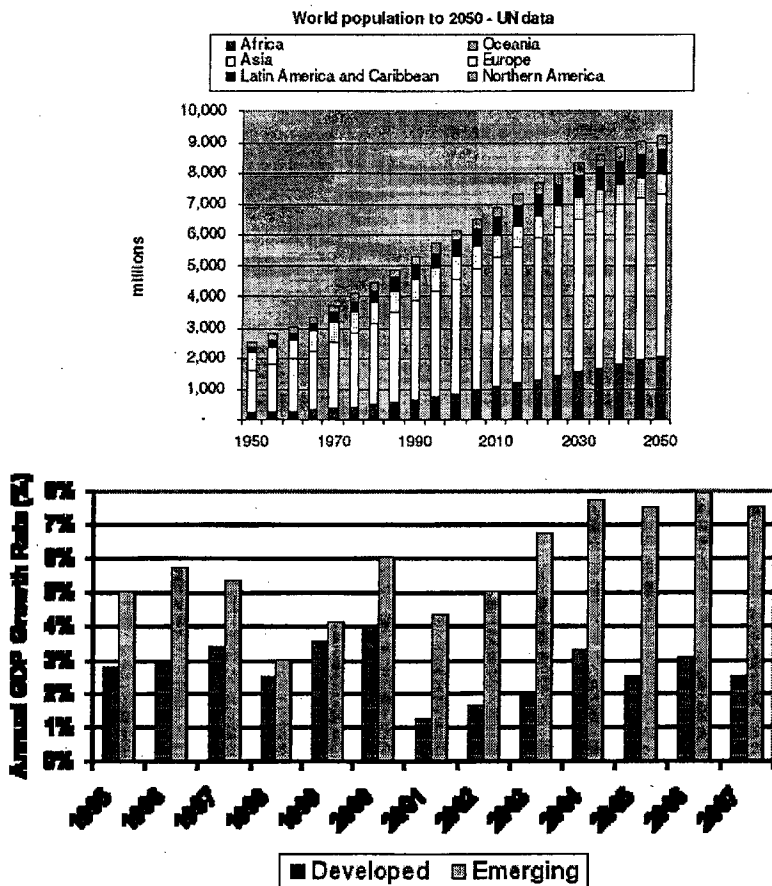
¹ Al Shaffer, Will Rees, and Robin Staffin in ODDR&E, Brendan Godfrey (Director, AFOSR), and Patricia Gruber (Director of Research, ONR)
² *Science The Endless Frontier* (<http://www.nsf.gov/about/history/vbush1945.htm>), the "Welch Report" of 2005 (*Assessment of Department of Defense Basic Research*, <http://www.nap.edu/catalog/11177.html>), DoD Directive 3210.1, and the report of the DSB Task Force on The Roles and Authorities of the Director of Defense Research and Engineering (October 2005) Office of the Under Secretary of Defense for Acquisition, Technology and Logistics Washington, D.C. 20301-3140

2 CONTEXT FOR DOD BASIC RESEARCH

The topic of this report has been treated by others previously, so that there may be little expectation of our saying anything new. However, there are circumstances both inside and outside the DoD that distinguish the present time from the past, and now suggest the need for significant modifications in DoD S&T activities.

2.1 The Changing Geopolitical Scene

As shown in the two graphs below, we are roughly mid-way through an almost-quadrupling of the global population within a century. And barring catastrophe, the next few decades will see a significant economic rise of 1/3 of humanity. With the globalization of economic activity, this evolution will necessarily diminish the fractional US economic, and perhaps political, influence; it is untenable that 5% of the World's population will continue to account for 25% of its consumption and economic activity. The ever increasing competition for resources might lead to conflicts in regions where none are envisaged today.

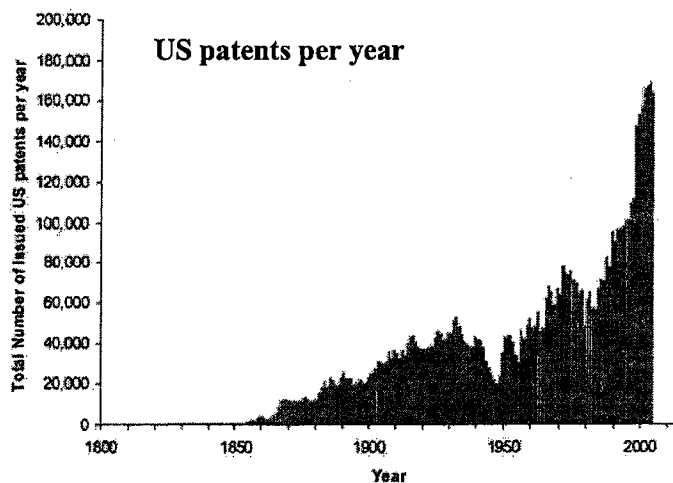


2.2 The Changing National Security Mission

It is trite, but nevertheless true, that the past two decades have seen significant changes in the world: the shift from a bipolar to a multipolar scene, the proliferation of WMD capability, the rise of terrorism, and many situations in which soft power seems more appropriate than hard (although not always more effective). The US armed forces seem to function more often as police and peacekeepers in a coalition of many, rather than as warfighters. And new aspects of national security have emerged (e.g., energy security, climate change).

2.3 The Accelerating Advance of Technology

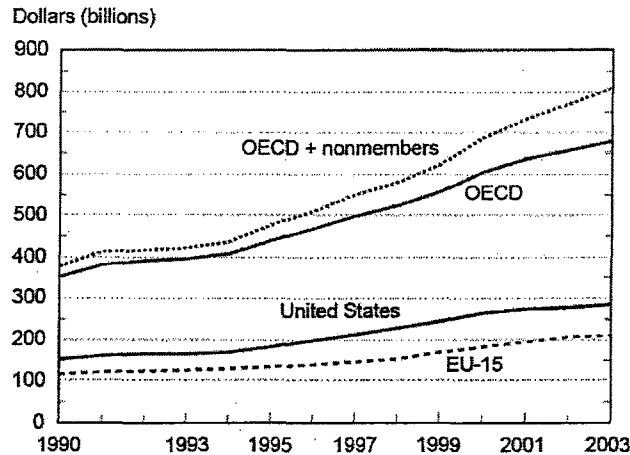
The following graph showing the number of US patents issued each year suggests that technology is not only advancing, but it is accelerating. It is important to keep in mind that most patents do not represent basic research, but are applications that are built on results from basic research.



The forefronts of technology are also changing: Information Technology (the ability to acquire, transmit, process, and store data) has come of age, while the Biosciences and Materials Science are advancing rapidly. Micro- and nanotechnology may yet well harbor basic-science surprises, and informed observers of neuroscience believe that remarkable developments will occur in that field during the next few decades.

2.4 The Globalization of Technology

The investment in science and technology and the fraction and quality of engineering development work done abroad is increasing, as suggested by the trends in R&D expenditures are shown below (OECD is the Organization for Economic Development and Cooperation, comprising the countries commonly taken to be the Developed World). Areas in which the US had no competition a decade ago are no longer US-based monopolies.³

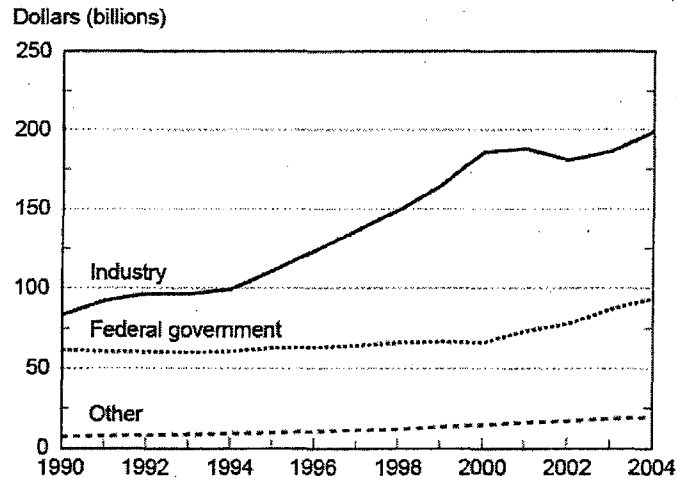


2.5 The Rise and Spread of Commercial Technology

Classified technology is a decreasing fraction of the whole. In the past DoD-supported R&D led to the creation of many new technologies (*e.g.*, supercomputing). But DoD R&D activities (which are the great majority of Federal expenditures) are now so small relative to the whole (see chart below) that in general the Department is much more reactive to, and adoptive of, commercial developments, rather than proactive in seeding and developing them.

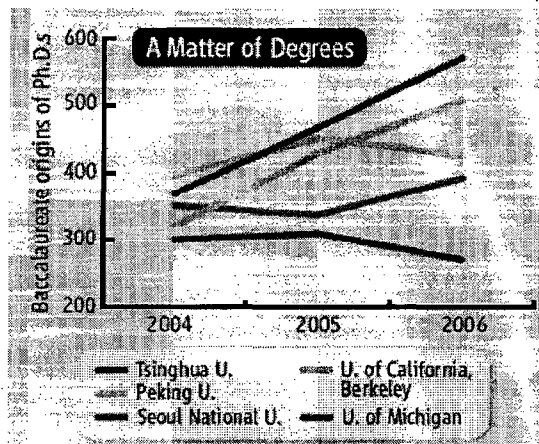
³ In this respect, the recent implementation of the ITAR/EAR environment has arguably hurt the US, with foreign nations forced to develop parallel capabilities they could no longer acquire from the US. Synthetic Aperture Radar (SAR) and large light-weight antennas in space provide interesting examples. Coupled with an increasingly ponderous government contracting and program management, the present environment has led to a decreased space-launch cadence to a perilous level, for example, creating a possibly subcritical US capability in an area vital to national security.

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2.6 The Changing Technology Talent Pool

The decline of S&T interest among Developed World youth is well-documented and much discussed. There are larger numbers of interested students in the Developing World, but even there one sees incipient signs of fall-off. The chart below (from Science magazine, July 11, 2008, vol. 321, pg. 185) shows that Peking and Tsinghua Universities have now overtaken Berkeley and Michigan as the largest undergraduate alma maters of PhD recipients in the US.



These trends are gradual and secular, with few milestones to mark their advance. Consequently, they can be dismissed or ignored as only one version of a highly uncertain future. But they are sufficiently broad and real that they must be taken seriously, for they imply profound changes in US National Security posture, missions, and technology needs.

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It is not within our scope to consider the entire National Security response to these drivers; we can only hope that other groups are doing so. However, we consider this factual landscape as essential context for our considerations of DoD basic research.⁴

⁴ A further perspective on changes since the Golden Age of DoD Basic Research some 60 year ago can be found in Appendix I.

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3 WHY HAVE BASIC RESEARCH IN THE DOD?

Numerous and compelling historical examples illustrate the importance of basic scientific research and technology development to national security. Given the continuing need to remain agile against adversaries that possess ever increasing technical capabilities and to field superior capabilities against such adversaries using ever-smaller manpower, DoD is likely to be increasingly reliant on technology advances in the coming decades.

Technology does not spring into life spontaneously – it begins with a firm foundation of the scientific understanding of natural phenomena, extends through the development of devices that exploit new understanding for practical purposes, and culminates in sophisticated systems engineering and integration in response to user needs. A successful journey from discovery to fielded capability can extend from many years to decades and requires the consistent and persistent application of scientific, engineering, and program-management skills. One can hardly envision DoD systems without lasers, for example, yet it is easy to forget the decades required before military applications of lasers were realized. Examples abound. It is also easy to forget that for every successful example of this kind, there were scores of basic scientific discoveries decades ago that did not lead to a fielded system.

3.1 Rationale for DoD Basic Research

There are important reasons why DoD must have a vibrant basic research program, principal among them being the following:

- a. Some fields of basic research are largely unique to DoD (*e.g.*, hypersonics, underwater acoustics, radiation-hardened electronics for space applications, high-power microwave generators, specialized detectors for remote-sensing systems, netcentric and distributed systems, precision navigation and geolocation systems, and many more). Here, DoD support is crucial to generating new knowledge and researchers.
- b. Practical engineering talent is crucial to the DoD mission. Basic research activities attract talented individuals, many of whom then migrate to more applied studies (such as at DOE and DoD labs). Today, even though an inadequate number of technically educated and trained U.S. citizens are graduating from the nation's universities, many of them are leaving technical fields after graduation for more lucrative professional pursuits. The basic and applied sciences are entry points for young people to careers in technology development in DoD labs and in working towards the DoD's long range needs in the private sector. The shortage of such people, together with the demographics of the DoD's present workforce, does not bode well for the US in the future.

- c. Today, much if not most basic research with potential DoD spinoffs is proceeding outside the aegis and guidance of the DoD, and much of it abroad. The loss in the output of this activity and the potential of technological surprise aside, a serious loss of technically capable personnel to the DoD also ensues. In several areas of vital importance to the DoD, such as in Aerospace, most experts available to national security organizations are past mid-career, with others either already retired or nearing retirement. DoD needs a cadre of basic researchers knowledgeable in DoD problems to scan and couple basic work to DoD applications. Such researchers, who must be at the forefront of their fields, are also important to avoid technical surprise, particularly when the technical enterprise and investment is growing rapidly outside the US.

3.2 Two Common Fallacies about Basic Research

Basic research is almost always long-term, with a measure of reliance on serendipity, and without the obvious and immediate focus of the engineering and systems development activities. It is therefore all too easily short-shrifted in changing budget priorities, missions, and personnel. Moreover, it is facile, but clearly fallacious, to argue that DoD is not, and should not be, a major supporter of basic research, but rather only a user of the science generated by support from other agencies.

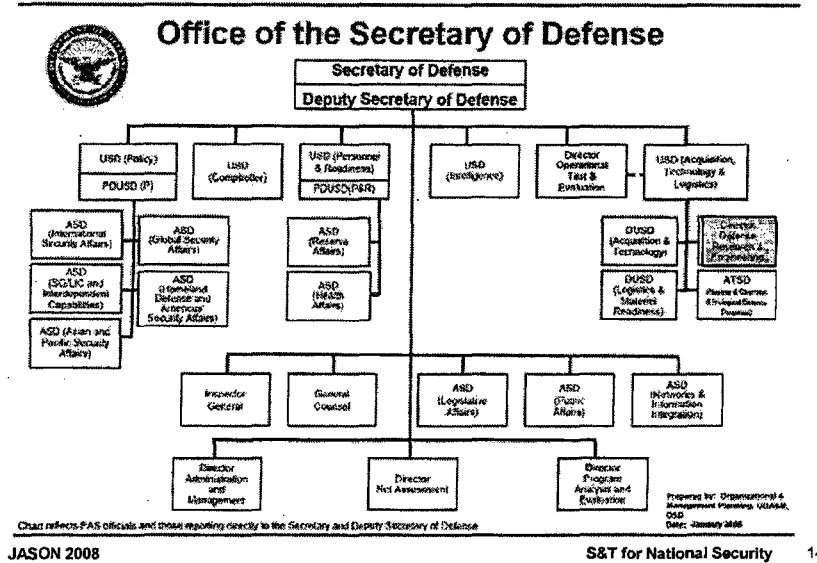
The DoD basic research program has changed significantly during the past decade in response to such pressures. It is therefore worthwhile to briefly recount the fallacies and counter them; a more detailed discussion can be found in Appendix B.

The first fallacy can be summarized as “Why invest when the Net Present Value (NPV) of basic research funding is so low?” The response here is that NPV may provide a useful metric in comparing the outcomes of alternative investment choices. But national security is not fungible with other goals or rewards. Further, *even if* the average NPV of research investments were low, the country needs insurance against worst-case technical surprise.

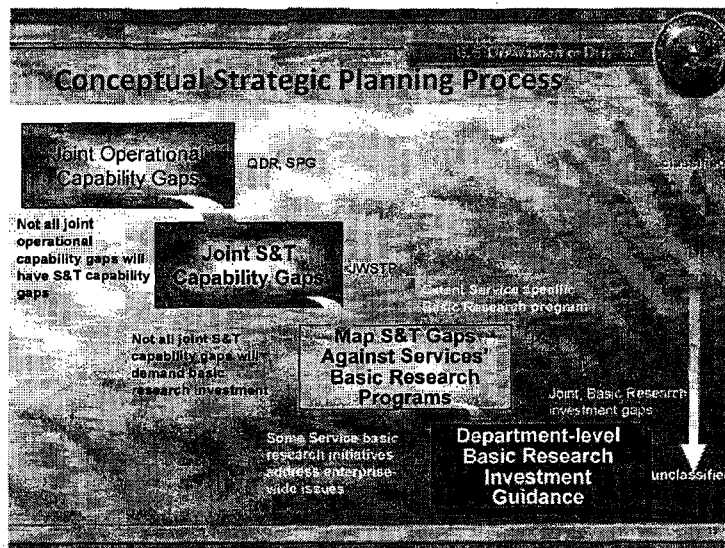
The second fallacy is simply stated as “Let someone else pay for basic research and we’ll just reap the rewards.” While perhaps appealing, it fails in practice, since the global technology market is not “efficient”. Further, first-mover advantages in taking basic research to application are real and many – the people involved have experience that is not easily purchased, physical proximity of the basic and applied work aids the application (“Tech transfer is a contact sport”), and a culture of discovery broadly begets further innovation. Indeed, US industry has not always been successful at a “just reap the rewards” strategy, and there is no reason to suppose DoD would do better.

The DDR&E is housed in OSD, as shown in the following OSD organizational chart:

Placement of DDR&E within OSD



The strategic planning process for S&T research is illustrated schematically below. Identified or perceived operational capability gaps define S&T capability gaps that are then mapped against the services' basic research programs. The DDR&E then issues Department-level Basic Research Investment Guidance for formulating the services' programs.

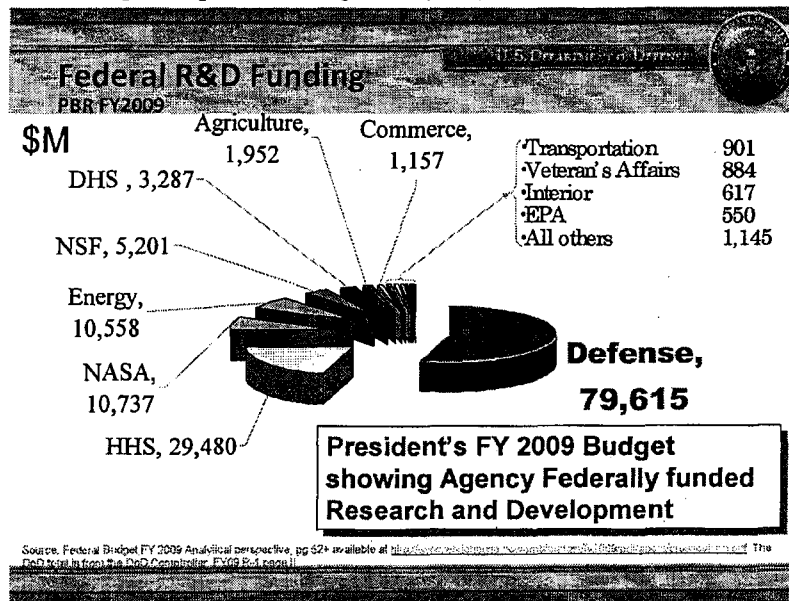


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At the more detailed budget level, the DDR&E reviews the services' initial submissions to ensure compliance with DoD guidance. Those are then sent to the OSD Comptroller function. At this point, the DDR&E can have influence, but exercises "hard power" sparingly; no unilateral changes are made. The Comptroller then submits the budget to OMB for White House input. Congressional hearings and modifications follow, leading to the passage of appropriations. The OSD staff then receives funds and sends them to the services for execution. Here, DDR&E could (but rarely does) recommend reprogramming of up to \$10 million per Program Element or could even withhold funds. However, delays induced by such interventions can hurt execution rates and cause funds to be pulled back by the Comptroller.

4.2 Funding

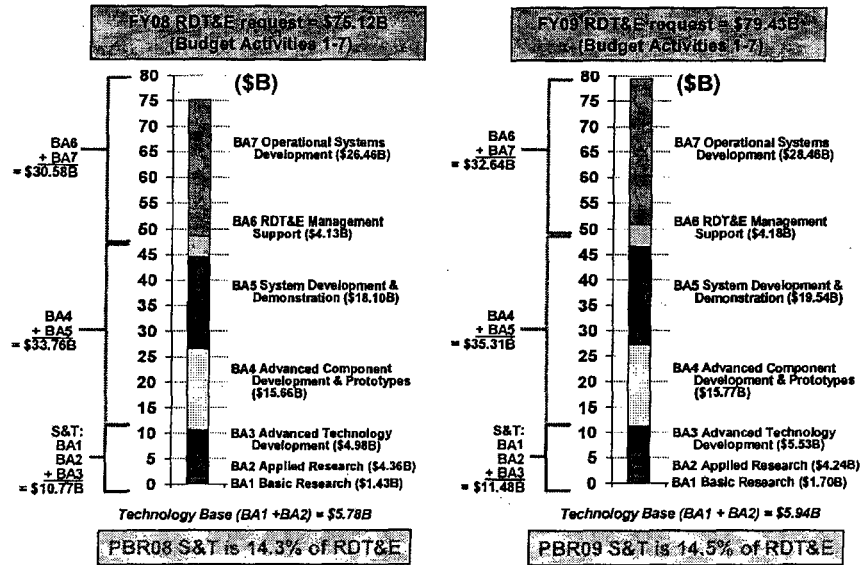
As shown in the chart below, the DoD accounts for more than half of all Federal R&D funding, the other large components being HHS (NIH), NASA, DOE, and NSF.



DoD R&D funding is divided into seven budget activities (BA1-7, sometimes alternatively termed 6.1-6.7) that span the research chain from basic research (BA1) through applied research (BA2 to Operational Systems Development (BA7). The breakdown of the roughly \$80B in R&D funds into the seven budget activities is shown in the following chart. Note that basic research (BA1) accounts for only about \$1.5B of the total, while S&T (comprised of BA1-3) accounts for some \$11.5B.

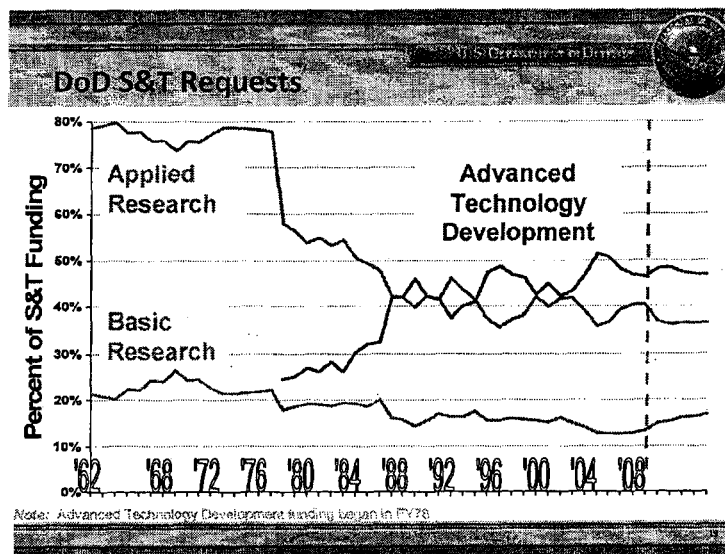
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FY08 and FY09 RDT&E Budget Request Comparison - in Then Year Dollars -



18

The past 45 years have seen secular shifts in the balance of funding within the S&T budget, as shown in the following chart. The fractions devoted to applied and basic research have declined steadily. Both are currently at or near all-time lows; BA1 is projected to increase during the next few years.

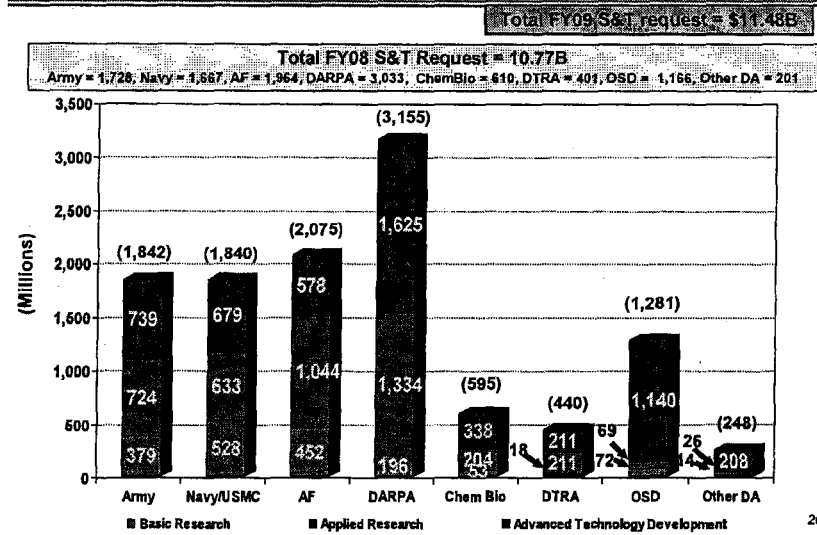


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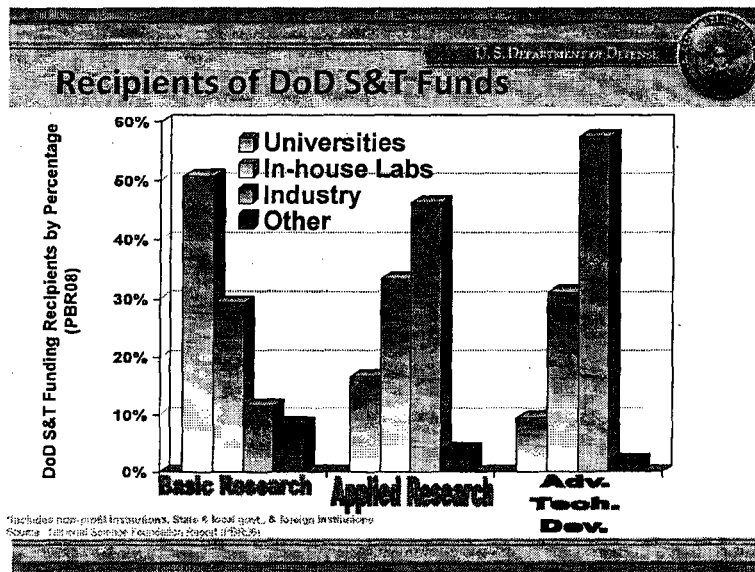
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As shown in the following chart, S&T funds are dispersed broadly across the services, DARPA, and OSD, while 6.1 money is concentrated largely in the services.

FY09 DoD S&T Budget Request

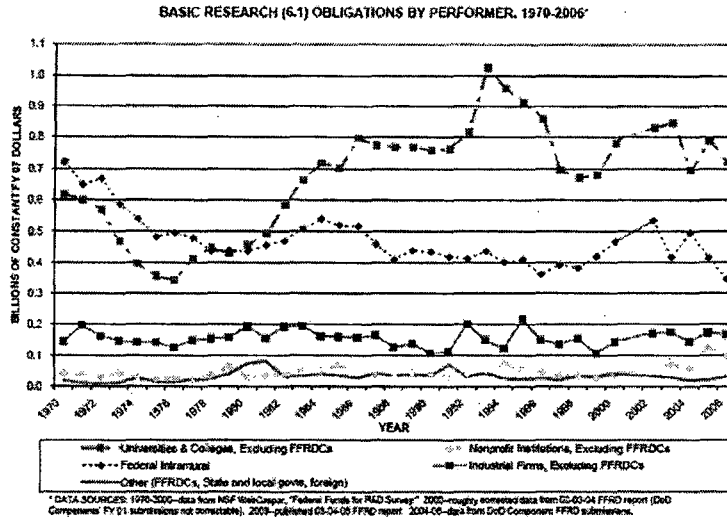


The following chart shows that basic research funding is concentrated at universities, while industry receives a larger fraction of the BA2 and BA3 funds. This is an appropriate balance, in our view. The in-house DoD labs participate at roughly the same fraction in all three funding categories.



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The following chart shows how the 6.1 funds received by various performers have shifted over the past 35 years. Basic research funding for the DoD laboratories has declined in recent years, while that at universities has been roughly constant, but down considerably from a peak 15 years ago.



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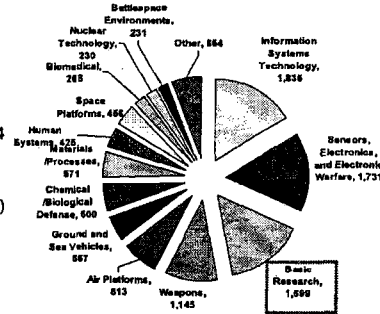
S&T for National Security 22

The following chart shows the breakdown of S&T funds into various functional areas, with the basic research funds shown as a whole. We see no obvious problems with this distribution.

Where is the DoD S&T money going?

Funding

- Current year S&T dollars: \$10.77B FY08 to \$11.48B FY09
- Percent of DoD funding: 2.24% FY08 to 2.22% FY09
- Over 50% of total investment in 4 functional areas:
 - Information Systems (1.8B)
 - Sensors, Electronics / EW (1.7B)
 - Basic Research (1.7B)
 - Weapons (1.1B)



DoD S&T program is focused on "sensing and shooting" But is changing.....

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The FY09 budget request includes an increase of \$270M for basic research, with emphasis areas as shown.

**PBR09 S&T Request Addresses
Capability Gaps (Cont'd)**



- **New technology/emphasis areas**
 - \$270M increase to Basic Research
 - **SecDef initiative to increase peer-reviewed basic research**
 - To develop innovative solutions
 - Enhance the science and engineering personnel base
 - **Increase will support targeted focus areas for**
 - Early to mid-career scientists and engineers with a team of students and post docs
 - Single Investigator awards with larger grants
 - **Emphasis will be on emerging technology areas, e.g.,**
 - Cyber protection and information assurance
 - Biosensors and biometrics
 - Human sciences (cultural, cognitive, behavioral, neural)
 - Software sciences and materials
 - Immersive sciences for training and mission rehearsal
 - Power and energy management
 - **Anticipate about 500 focused research efforts**

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4.3 Tensions

We see two fundamental tensions in DoD Basic Research that need to be at least managed, if not resolved. One is that while the 6.1 budget of some \$1.5B is large on the scale of funding for basic research (for example, comparable to the NSF's Mathematics and Physical Sciences research budget) and critical as the largest source of support in some fields, it is small on a DoD scale. As a result, basic research has difficulty achieving high-level visibility and does not capture management attention. The strategy of combining it in a management and oversight sense with the 6.2 and 6.3 programs gives it significantly more heft (some \$11B), but again risks its neglect relative to these larger activities.

The other tension is that associated with centralized vs. dispersed research activities. This dichotomy is familiar in other organizations, including for-profit corporations. In the situation that we judge to exist today, a dispersed basic research program is managed and executed by the services with the center (DDR&E) providing coordination, certification, and representation to the higher levels of the Department and to the external world (e.g., Congress). Advantages to this arrangement are customized programs close to service needs and personnel, and hence a feeling of ownership by the services. However, the drawbacks are that it is more difficult to coordinate and realize synergies in the program, as well as to resist

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the drift of individual programs away from Department expectations and opacity in the nature and scale of actual activities undertaken as part of BA1/6.1 funding.

An alternative management arrangement would be a "purple" basic research program that is managed and executed by an organization in the center (perhaps, but not necessarily, through an expanded DDR&E or a DARPA-like agency) with input and oversight from the services. The advantages of such an arrangement would be a greater mass of 6.1 research, more coherence across all of the Department's activities, and an easier recognition and facilitation of synergies. The drawbacks are that the research could more easily become distant, if not disconnected, from the services, evolving into a collection of "sand-box" activities. While all this could be avoided by proper management, it is a danger.

5 OBSERVATIONS

We begin by noting that *a healthy DoD basic research program is essential*. This perhaps hardly needs to be said given the exposition of Sections 2 and 3 above, yet it is gratifying that the present Secretary of Defense agrees:

“As changes in this century’s threat environment create strategic challenges – irregular warfare, weapons of mass destruction, disruptive technologies – this request places greater emphasis on basic research, which in recent years has not kept pace with other parts of the budget.” – Secretary of Defense Posture Statement on the FY09 Budget, February 2008

However, despite the importance of DoD Basic Research, we believe that important *aspects of the DoD basic research programs are “broken”* to an extent that neither throwing more money at these problems nor simple changes in procedures and definitions will fix them. Further, whatever improvements can be made must be institutionalized to endure the vagaries of the personnel involved at any moment in time. We amplify these points in the following sections.

Our observations are based on data provided to the study team. The study team is made up of senior academic scientists who have worked with DoD for many years, and who have closer working relationships with DoD than most academic scientists. Over the years we have seen significant change in focus from long-term basic research to short-term deliverable-based research. While many findings lack hard statistical evidence, the anecdotal examples, provide valuable insight into the key issues.

5.1 Program Observations

Relative to other S&T categories, basic research is longer-term, less immediately applicable, and a smaller amount of funds. These characteristics make basic research vulnerable to being co-opted, with the following undesirable manifestations.

1. *DoD sometimes appears not to be adhering to its own definition of basic research in its use of 6.1 funds.* Rather a portion of 6.1 funding by the services has been subject to short-term pressures and drifted toward more managed research relevant to direct service needs. Such drift has resulted in a net loss of *bona fide* 6.1 activities, inconsistent with DoD goals and directives.
2. *Basic research funding is not exploited to seed inventions and discoveries that can shape the future; investments tend to be technological expenditures at the margin.* A

basic research program driven by operational requirements will produce only incremental advances of existing technologies.

3. *The portfolio balance of DoD basic research is generally not critically reviewed by independent, technically knowledgeable individuals.* ODDR&E has too little time, staff, and authority to do this properly. Such reviews would help identify promising research areas and promote the involvement of the outside research community.

4. *Common/uniform management and reporting of 6.1 with 6.2 and 6.3 funds is bad practice.* It obscures the actual uses of 6.1 funds. Further, as many 6.1 program managers also handle 6.2 and 6.3 activities, the smaller and less urgent 6.1 work gets less attention.

5.1.1 Drift in basic research

There has been much discussion during the past decade about the extent to which 6.1-funded activities conform to the definition of 6.1 research or rather are of a more applied character. The operative definitions are:

BA1 Basic Research. ...systematic study directed toward greater knowledge or understanding of the fundamental aspects of phenomena and of observable facts without specific applications towards processes or products in mind

BA2 Applied Research. ...systematic study to understand the means to meet a recognized and specific need.

This issue of "6.1 drift" was the explicit subject of the NAS Welch Report (2005), which contained the following selected findings:

***Finding 1.** Department of Defense basic research funds under 6.1 have not been directed in significant amounts to support projects typical of 6.2 or 6.3 funding.*

...

***Finding 8.** A recent trend in basic research emphasis within the Department of Defense has led to a reduced effort in unfettered exploration, which historically has been a critical enabler of the most important breakthroughs in military capabilities.*

Finding 9. Generated by important near-term Department of Defense needs and by limitations in available resources, there is significant pressure to focus DoD basic research more narrowly in support of more specific needs.

Findings 1 and 9 would suggest that the DoD has been successful in resisting such pressures. Curiously, the report offers no praise for such fortitude, but rather recommends changing the definition of 6.1 research from ...*without specific applications towards processes or products in mind* to ... *has the potential for broad, rather than specific, application*. ... To our knowledge, no such change has been implemented.

To judge 6.1 drift for ourselves, we undertook an *ad hoc* scan of some 258 synopses of AFOSR grants (with STTR contracts and Congressionally mandated grants removed) and a list of ARO program descriptions (*ARO-in-Review, 2007* from <http://www.arl.army.mil/www/default.cfm?Action=29&Page=172> for the latter; ONR information was not available to us). It appears that a large fraction of the AFOSR synopses seem not to conform to the 6.1 definition. Several of us looked at this list of proposed research titles. Obviously, the titles alone do not allow us, in every case, to judge whether the proposed research conforms to the definition or 6.1 or not. Nonetheless, in a number of cases it appears to us that there is a substantial number of proposals in this list that are not, even by a generous stretch, 6.1 research. JASON judgments were that the proposals ranged from about 25% to 81% non-6.1 research. This, of course, is not a judgment of the value to DoD of the research being proposed. Here are a few examples of titles of research proposals that we judged were not in the 6.1 category⁵:

- *Functional Computer Codes for the Design, Characterization, and Optimization of Phased High-Power Fiber Amplifier Arrays for Airborne Weapons Applications*
- *Characterization of Unresolved Satellites using Optical and Radar Signatures*
- *Analysis of Archival AFRL Data: Psychometric Studies to Improve Selection and Retention of USAF Personnel*
- *Lossless Data Embedding – Steganography*
- *Application of Control Theory to Air Operations*
- *Basic and Applied Research Using Electromagnetic Radiation for Secure Communications and Information Processing*

⁵ By singling out these and the ARO projects below, we do not at all mean to imply that they are unworthy of support; indeed, most seem quite worthy. Rather, we question whether they conform to the definition of 6.1 research: *without specific applications towards processes or products in mind*.

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- *Navigation of SUAVs via Power Lines*
- *Nanocomposites for Lightning Strike Protection*
- *Biomimetic Control Methods for Autonomous Munitions*
- *Cramer-Rao Bounds for Integrated C4ISR*
- *Model-Based Automatic Recognition (MBATR) For AF Missions*
- *Frequency-Agile Detectors for Space Situational Awareness*
- *Plasma Chemistry for Air Force Systems*

Similarly, the ARO 6.1 program descriptions included the following projects:

- *Parameters for Efficient Fuel Cell Catalyst Structure*
- *Automatic Target Recognition Using View Morphing*
- *Ultra-Wideband Impulse Radio for Ad-Hoc Tactical Military Communications*
- *RF Communication Sub-system Integration*
- *Design Optimization of Structures for Blast and Impact Damage Mitigation*
- *Mechanisms and Models for Hydrocarbon and Propellant Combustion*
- *Miniaturized Integrated Atom Chip Technology for Inertial Navigation*
- *Solid State Microwave Devices for Advanced Battlefield Communications*
- *Destruction of Toxic Military Materials at High Temperatures and Pressures*
- *Threat Agent Neutralization Including Effective Non-Corrosive Decontaminants*
- *Handheld Highly Sensitive Explosives, Chemical Agent, and Radiation Detector*
- *Lightweight High Yield Borazane-Fed Hydrogen Generator*
- *Quantitative Study of the Effects of Chemical Additives in Propellant Flames*

Further suggestion of 6.1 drift can be found by analyzing the 2007 ARO 6.1 budget provided to us, as shown below. Of the \$500M listed, only the first item (\$42M) in the list of Core Science Programs (less than 10%) is readily identifiable as supporting basic research.

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- **\$42 M: Core Science Programs**
 - Chemistry, Physics, Math, CS, Electronics, Environmental Science, Life Science, Materials Science, Mechanical Engineering
- **\$14 M: Special**
 - In-House (\$12 MM); HBCU (\$1.2 MM)
- **\$54 M: OSD Programs**
 - HBCU (\$14 MM); Chemical/Biological (\$32 MM)
- **\$374 M: Army Direct Funded**
 - MURI (\$49 MM), DURIP (\$12 MM), SBIR (\$16 MM), STTR (\$11 MM), DARPA (\$98 MM) and Other Pass-Through (\$155 MM)

No doubt some of the other items also support research that conforms to the 6.1 definition, but that is difficult to determine.

The redacted description of the “daily life” of a service research program manager also offers some insight into how basic research programs are assembled:

For my extramural grants, I really have to answer 2 questions before I can consider any proposal: 1) is this more appropriate for a different Federal agency? If it is, then I refer the PI to alternative sources of funds other than this service research program manager. Next, I ask 2) whether there is a "service" customer for the resulting data? Only when I have such a customer, may I consider funding work. To this end, I usually start off with the PI submitting an e-mail idea for work. If that looks possible, we go to a 5 page (max) preproposal stating what is to be done, how, for how much, and what we get for the money. That preproposal I send around looking for the above-mentioned "customer" (my job essentially is matching University capabilities to "service" needs). If I get someone's interest, we can then proceed to a full proposal stage. This saves everyone time and effort, and keeps the emphasis on the research question rather than on any particular technology. The range of work that this program funds stretches across broad areas of this kind of research. Our core funding is normally targeted to essentially "buy a post-doc" (around \$100K or so per year, times 3 or so years). Funding is of course constrained by the federal fiscal year and our budget; funding decisions are generally made around March and April.

5.1.2 Program areas of focus

The President's FY09 Budget Request lists the following Grand Capability Challenges as a focus for increased 6.1 funding:

- Information Assurance
- Network Sciences

- Counter WMD
- Science of Autonomy
- Information Fusion & Decision Science
- Biosensors and Bio-inspired Systems
- Quantum Information Sciences
- Energy and Power Management
- Counter Directed Energy Weapons
- Immersive Science for Training & Mission Rehearsal
- Human Sciences

While we have not undertaken a detailed review of any of these areas, this list seems appropriate. However, we recommend DoD consider adding “Dynamics of Oceans and Atmospheres”, an important basic-science challenge relevant to all of the services in a variety of contexts.

5.2 Personnel Observations

People are the bedrock of a successful research effort, yet the present DoD research program is more about funding projects than supporting the best people. Symptoms of this are as follows:

5.2.1 Personnel within DoD

The military S&T work force is typically in the same pool for promotions as fighter pilots and combat infantry officers. While this makes the S&T officer part of the total team and gives them insight into operational needs, it is difficult to get promoted without extensive operational experience on one’s resume. Acquiring advanced degrees in science and engineering and lingering in S&T billets is viewed as dead-ended and a weak promotion package. It is difficult to conduct long-term research and develop longer-term research contacts with academia in one three-year tour. Civilian S&T workers are allowed to remain in a research area for long periods and now may well get increased pay for performance, but promotion opportunities are limited. In both cases, the pay is often less than what is available outside the DoD.

In the past, DoD Labs were world leaders in science and new technology. Their facilities were robust and the flow of research science, technology development and engineering expertise between the Labs and Industry was a dynamic process. Today, DoD Lab infrastructure has atrophied, for the most part,⁶ to the point where most DoD S&T people are project managers who monitor and fund research in academia, the FFRDCs, and Industry. These people seldom do research themselves and job satisfaction suffers. DoD salaries are not competitive and do not facilitate the acquisition of the best talent. Scientist and engineers

⁶ There are notable exceptions, but these are certainly not the norm.

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working inside the DoD find themselves focused more than ever on following regulations, proving the relevance of their work packages, and trying to convince acquisition program managers to use the new technology than they have developed. While the DoD technology flow model suggests that the acquisition community is eager to acquire new technology to improve system performance, new technology is "high-risk" and routinely avoided. This often cuts the flow of ideas and demotivates the S&T workforce. Prototype experiments like advanced concept technology development programs provide an alternate approach to demonstrate new S&T, but there is insufficient funding to bring new ideas out of the labs. The result is that ideas dry up, S&T people leave the DoD, and young people do not see DoD S&T as an exciting and challenging career path. Senior S&T leaders typically defend current processes that no longer are effective.

A useful metric of the quality and competitiveness of DoD Lab S&T personnel, as well as the quality of individual DoD Labs, would be to compile statistics of DoD Lab personnel who have been attracted away into the academic world, and vice-versa. A comparison with similar statistics from other Federal and FFRDC organizations would also be of interest.

5.2.2 Personnel within academia

DoD should strive to fund the highest quality people and not focus on projects. DoD basic research has a significant presence at many of the nation's leading research universities, as shown by the following chart (adapted from the Welch Report). Indeed, most of the top 50 university recipients of DoD 6.1 funds are members of the elite Association of American Universities and even those that are not house many excellent scientists (for example, the University of Texas Southwestern Medical Center, at the bottom of the list, houses several Nobel Prize winners). Yet the DoD has not effectively leveraged these contacts to help solve DoD S&T problems.

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What universities do 6.1 research?

TABLE E-1 Top 50 University Recipients of DoD 6.1 Funding in Fiscal Year 2002

Institution Name	State	Total 6.1 (\$000)
1 Massachusetts Institute of Technology	MA	43,802
2 Pennsylvania State University, All Campuses	PA	35,357
3 University of California, Los Angeles	CA	31,784
4 University of Washington	WA	29,884
5 Stanford University	CA	25,811
6 University of Southern California	CA	25,758
7 Duke University	NC	25,607
8 University of Michigan, All Campuses	MI	24,245
9 University of California, San Diego	CA	24,117
10 University of California, Santa Barbara	CA	19,964
11 University of Illinois at Urbana-Champaign	IL	19,925
12 California Institute of Technology	CA	19,478
13 Johns Hopkins University	MD	18,888
14 Johns Hopkins University	MD	18,888
15 University of Texas at Austin	TX	17,044
16 Carnegie Mellon University	PA	16,527
17 Cornell University, All Campuses	NY	15,209
18 Princeton University	NJ	13,452
19 State University of New York, System Office	NY	12,598
20 Woods Hole Oceanographic Institution	MA	12,493
21 University of Pennsylvania	PA	12,282
22 University of Pennsylvania	PA	12,282
23 University of Pennsylvania	PA	12,282
24 University of Arizona	AZ	10,977
25 University of Wisconsin - Madison	WI	10,389

Not a member of the
Association of American Universities

26 University of Maryland System Administration	MD	10,334
27 University of Colorado, All Campuses	CO	10,303
28 Harvard University	MA	10,081
29 Northwestern University	IL	9,692
30 Purdue University, All Campuses	IN	9,470
31 Ohio State University, All Campuses	OH	9,190
32 Ohio State University, All Campuses	OH	9,190
33 University of Minnesota, All Campuses	MN	8,667
34 University of Minnesota, All Campuses	MN	8,667
35 University of Virginia, All Campuses	VA	8,335
36 University of Virginia, All Campuses	VA	8,335
37 University of California, Davis	CA	7,776
38 University of California, Davis	CA	7,776
39 University of California, Irvine	CA	7,672
40 Brown University	RI	7,491
41 Brown University	RI	7,491
42 Rice University	TX	6,918
43 University of North Carolina at Chapel Hill	NC	6,576
44 University of North Carolina at Chapel Hill	NC	6,576
45 University of Pittsburgh, All Campuses	PA	6,305
46 University of Pittsburgh, All Campuses	PA	6,305
47 University of Florida	FL	5,947
48 University of Florida	FL	5,947
49 University of Florida	FL	5,947

SOURCE: Based on data provided in personal communication from Mark Herbst, Office of the Director of Basic Research, Office of the Secretary of Defense, Washington, D.C., to James Garcia, National Research Council, June 2004.

DoD does not generally focus 6.1 funding on research of the highest caliber carried out by individuals with the potential to provide new paradigms for science and technology. DoD is getting what it asks for in tightly managed and focused research programs, but is reducing the potential for true breakthroughs. We elaborate on this theme in Appendix C.

DoD is not adequately participating in the development and maintenance of the S&T educational pipeline. Relative to the size of its basic research program, the DoD supports fewer pipeline activities than comparable NIH, NSF, and DOE programs. For example, the 150 annual National Defense Science and Engineering Graduate Fellowships are relatively few compared to the 1000 graduate fellowships awarded annually by the National Science Foundation.⁷

⁷ In detail, NSF graduate fellowships in 2008 were 125 in Mathematical and Physical Sciences Division (with a budget comparable to DoD 6.1), 302 in Engineering (with a total budget of some \$700M), 52 in CISE (with a total budget of some \$600M), 167 in Social Sciences, 240 in Life Sciences, and 26 in Geosciences. Of course, graduate student support embodied in research and training grants is not included in any of these comparisons.

5.3 Organization Observations

DoD is not effective in coordinating and overseeing the basic research program and funding across the department. In particular, OSD is structurally weak in determining and maintaining the quality and balance of basic research in DoD's intramural and external research programs. Further, the DDR&E is largely decoupled from the "cash flow" of the yearly budget process, both in the formulation of the research budgets proposed to Congress and in the direction of program funds appropriated by Congress. In some cases, the services have been able to redefine, or effectively eliminate, basic research activities within a single budget cycle. For example, during the past decade, many have come to believe that ONR has shifted its basic research toward a short-term focus that seems inconsistent with both a healthy research program and the definition of basic research.

An historical perspective on the DDR&E position and the ebb and flow of its powers the can be found in Appendix D.

The bureaucracy associated with DoD research has grown to consume ever more time and has diverted program managers into administrative formalities at the expense of scientific program oversight.

The DOE Labs have a higher profile in basic research. This is especially true for LLNL, LANL, and SNL, which are similar to the DoD labs in that they carry out basic research that leads to national security advances. We elaborate on this comparison in Appendix E.

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6 RECOMMENDATIONS

Numerous studies, evaluations and reports have highlighted the loss of S&T expertise within the DoD. There is a common thread in many of these assessments: while DoD values in-house S&T expertise, the various organizations involved so far lack the consensus and the will to make institutional changes to fix the problem. The recent increases in S&T funding are encouraging, but the structural and work force problems are so systemic and deep that increased funding will not solve the problems. DoD needs to think about near-term and long-term fixes to the S&T problem. It has taken decades to produce the current situation and it will not turn around overnight.

6.1 Program Recommendations

Focus on funding people before projects. The "payoff" to DoD is a cadre of people in the internal and external communities who are cognizant of both DoD needs and the forefronts of science, as well as the research itself

By basing funding decisions on peer reviews, NSF tilts support toward projects and not individuals. By contrast, the DoD had a remarkable record of success based upon betting on people to discover the future. DoD proposal funding increasingly relies on peer review that discourages the revolutionary advances that were possible in the past.

DoD funding in no small measure made U.S. science the envy of the world and led to scientific advances that revolutionized the battlefield and contributed to the country's economic strength. A critical aspect of this enterprise was technically savvy program managers who were given considerable latitude in decision making. Such flexibility also made these jobs more attractive than they are at present and was an important ingredient in attracting quality scientists into DoD science agencies.

As noted in many places, our nation's youth these days are not being inspired to enter science. Much of this seems to result from broad societal trends that DoD cannot alter. Some of it, however, occurs because those interested in pursuing science learn that many successful scientists would not enter the field today owing to the difficulty of obtaining funding. The decline in DoD funding of basic research following the end of the Cold War is a major factor in this situation. Previously, much of the 6.1 funding was used for steady funding of leading people in fields of long-term, decadal, DoD interest that were not being adequately supported by NSF or other agencies. Decisions about who should be supported, how much, and whether the recipients were remaining productive in areas at the forefront of their fields were made by program managers.

The greatest successes of ONR occurred under this regime. It, however, is difficult to defend against congressional staff or Pentagon superiors who ask 'Yes, you are doing great things, but why can't you do nearly as well with 5% less?' To defend against this approach being a yearly ritual, higher level DoD managers decided to package increasing fractions of the research into identifiable multi-investigator programs, many of which are focussed on topics of increasing short-term relevance. Following this approach for two decades, coupled with declining budgets, has produced the present situation in which most leading scientists either have no contact with DoD programs or use them to supplement their other support.

Ensure that 6.1 activities conform to the 6.1 definition. There are several steps that can be taken to achieve this goal. For example, accounting can be structured to make the use of 6.1 funds transparent. Further, the DDR&E could certify annually to the SecDef that 6.1-funded activities are basic research as defined by the DoD. (The NSF MPS Division "Committee of Visitors" exemplifies one mechanism by which this might be accomplished.) Finally, non-conforming activities should be moved to other budget lines in subsequent years.

Eliminate large fluctuations in 6.1 funding and schedules. Long-term research efforts cannot be turned on and off with yearly budget cycles and service rotations. Indeed, for a researcher, stable funding is more productive than more variable funding. Pressures to shape the basic research program around the "War of the Month" should be avoided.

6.2 Personnel Recommendations

Establish a Research Corps within each service to address the chronic S&T personnel issues within the services. DoD should develop an S&T Corps to bring in military people outside of the normal line promotion process. Routine rotations across service boundaries should become normal career progress. Promotions should be based on the value of research contributions to national security, beyond service needs. This would more properly value both personnel and research programs. Civilians should also be assigned to the S&T corps and allowed to compete for opportunities across service lines. The goal should be to foster the growth of a dynamic research pool across DoD that is protected from advancement pressures of the operational and acquisition communities. These steps would be analogous to the service medical corps or acquisition corps and so fit the model for joint service that DoD has adopted. The increased professionalism, training, career paths, Defense-wide mobility, visibility, and esprit would all help address the problems of research personnel within DoD

The DoD labs should house some researchers that are well-coupled to the broader S&T communities. DoD needs to get back in the mainline for S&T with people doing hands on research across government agencies. It must develop a culture in which in-house science and

research is valued as critical to the long term health of the Department, on a level of equal importance as buying new weapon systems.

We believe that the labs' focus should be on 6.2 and above, but that they should also house small cadres of high-quality basic researchers. DoD needs to grow in house experts and link them with experts in the academic and for-profit sectors. These in-house experts are essential for identifying the best researchers in the academic community and advocating for their support. Research leaves for lab personnel to work in academic, industrial, and other government labs would help toward this end, as would hosting academic researchers on leave from their university. The Department should recruit nationally from top universities with loan repayment programs, sponsor more in-lab postdoctoral fellowships in the hard sciences and engineering disciplines, and support more work study programs.

6.3 University Recommendations

For undergraduates, we recommend that the Department consider outreach and summer internships rather than scholarships; (e.g., Research Experience for Undergraduates). The DoD has a number of activities that would be attractive to students interested in basic research (e.g., field tours, explosives research, ...)

The DoD should consider other models in addition to PI-driven graduate student and postdoctoral support. In particular, graduate training grants in other agencies and foundations (e.g., NSF, NIH, HHMI) have been very successful in integrating education and research (and hence could provide a tie to the DoD labs). The sense of prestige is very important in putting such a program in place. Vertically integrated approaches that combine faculty, postdoctorals, graduate students, and an undergraduate research experience with teaching training can also be effective in creating a broad network of researchers as graduates move on to industrial/academic/government positions. A discussion of some of these approaches is contained in Appendix VI.

Improve the coupling between DOD supported faculty and DoD S&T needs. In particular, it is most important to build a community and educate them about the issues before a crisis that could benefit from their participation. As discussed in Appendix G, the case of IEDs in Iraq is a good illustration of the failure to build and sustain such a community.

Expand (with improvements) the new National Security Science and Engineering Faculty Fellowship (NSSEFF) Program. Our assessment of this program and some suggestions for improvement are contained in Appendix H.

6.4 Organization Recommendations

Protect 6.1 funding at the OSD level by strengthening and expanding the role of the DDR&E. At the very least, the Secretary of Defense should empower the DDR&E to substantively review and comment on the services 6.1 budget requests before these requests are sent to Congress and to review and reprogram basic research funds appropriated by Congress before these funds are distributed to the services. We understand that the DDR&E already has such authorities formally, but is in practice constrained from exercising them fully because of limitations of time and manpower.

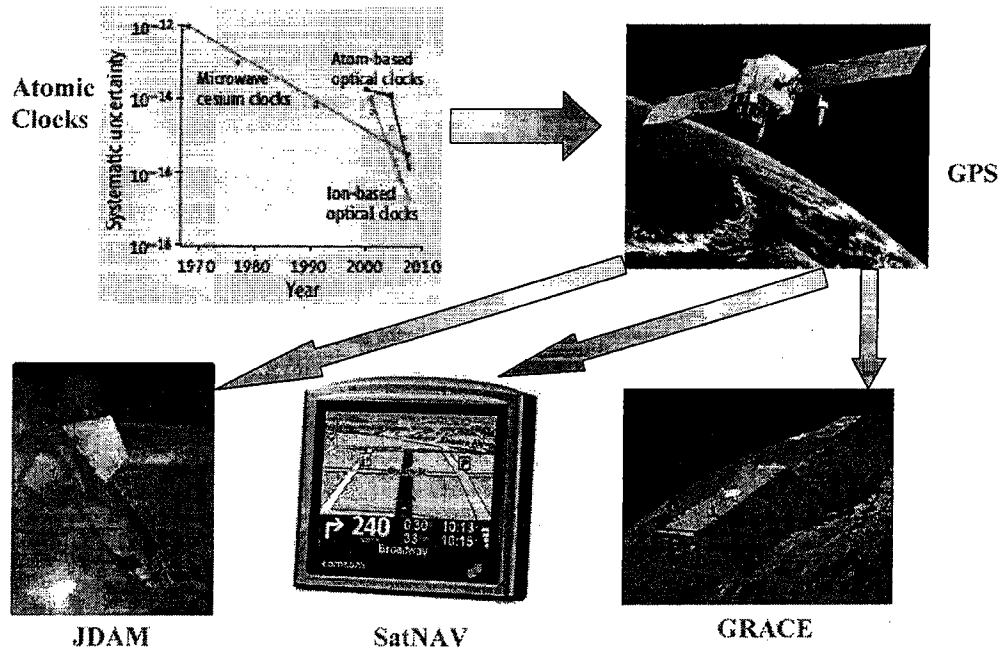
The traditional DoD model for research (*i.e.*, flowing advances from 6.1 to 6.4 and beyond) has been overcome by events, and does not work well today. Indeed, a 6.1 accomplishment may have more relevance and benefit to the 6.2+ programs of another service. Many organizations have fused S&T functions with acquisition functions to ensure close alignment between basic research, advanced technology development and prototype development, and fielding of new systems with new technology, defeating the purpose and original intent. In reality, increased costs of new acquisition systems are financed by reductions in S&T budgets to meet milestones and schedules and to minimize requests for new funds. For example, it is difficult to defend a need to maintain system survivability research or the pace of a new focal plane array technology at the 6.1 level when those funds are needed to avoid a three month slip in a missile test program that will increase the system cost through program extensions. These decisions are made every day, and the DoD S&T programs traditionally lose out to more pressing acquisition or operational needs. Because of this, *we recommend that line acquisition and operational leaders should have input to, but not decision authority over, the 6.1 budget.*

A bolder step than these would be to *redefine and elevate the DDR&E position to that of an Undersecretary for S&T*, separating the research and acquisition functions. We understand the practical challenges involved in making such a change, but the benefits would be many: an informed technical voice at the highest levels in the DoD, a greater visibility of S&T within the Department, and a more focused management attention on S&T.

Create a basic research advisory committee reporting to the USDATL. The membership of this committee should include the DDR&E and appropriate service personnel, together with an equal number of "external" members with high scientific and technical credentials from academia and industry. The committee would review and advise annually on the health of DoD basic research (program, personnel, organization) and would serve as an institutional memory for 6.1 activities. Such committees have demonstrably played a valuable role in other organizations.

6.5 A Final Thought

Despite the critical tone and content of much of this report, it is important to acknowledge some of DoD's truly outstanding S&T achievements over the past decades. These include High Performance Computers, the Internet, various satellite and detector technologies, and netcentric warfare. But perhaps none exemplify the link between basic research and operational capability better than the GPS system illustrated in the figure below. In that work, a multi-decade DoD program of basic research in fundamental atomic physics resulted in continual improvements in clock capability. These advances enabled a system having revolutionary impact on military capabilities, civilian life, and basic sciences. It is an example to be inspired by.



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A APPENDIX: Some Thoughts on the Golden Age

The environment in which DoD must execute its basic science mission has changed considerably in the 63 years since Vannevar Bush's "Science, the Endless Frontier of July 1945. An overarching factor of those times was the wonder and gratitude for the scientific and technological accomplishments of the atomic bomb and radar, and the faith of some inspired leaders that the support of basic research would surely, in some way, support the national security. Beyond the decline of that faith, we have identified three major aspects of change as discussed below.

1. Shift in Scientific Topics of Interest to Society

Then: With the end of WWII and the beginning of the cold war, the greatest threat to our society was perceived to be military attack from the USSR. As such, scientific topics relevant to the DoD mission were perceived as being of importance to society (nuclear physics, aerospace engineering, electrical and mechanical engineering, etc.) Because of their perceived importance, these topics tended to attract many talented scientists, some of whom became participants in the DoD basic science community.

Now: With the end of the cold war, many issues facing society have shifted. This shift will continue, as challenges facing society focus scientific minds on the ever-increasing needs of the developed/developing countries of the world. These issues involve Energy, Food, Water, and Medicine. Even though these translate into national and global security issues and are therefore indirectly relevant, they are not *directly* relevant to the DoD mission and will make it increasingly difficult to attract talent into the DoD basic research community.

2. Decline in Industrial R&D

Then: The US manufacturing infrastructure was unscathed by WWII, and US manufacturing industries thrived in the post-war environment. Additionally, the success of science and technology in helping to win WWII resulted in dramatically expanded federal support for R&D and explosive growth in the sciences. During this period, many US companies established R&D laboratories that conducted basic research and many of the fields of research were directly relevant to the DoD mission. DoD basic science benefited from this foundation of industrial R&D through the flow of people and ideas.

Now: Increased competition in the modern global economy has streamlined many US industries. Basic research that is generally perceived as a high-risk, long-term investment, has not survived this streamlining. Most of the high-profile, industrial R&D organizations no longer exist, or exist in the shadow of their former stature. DoD basic science can no longer depend on this foundation of industrial R&D. This magnifies the challenges associated with developing a DoD R&D workforce and transitioning the results of basic science to application.

3. Challenges in developing the scientific workforce

Then: Sixty years ago, access to higher education often required access to personal financial resources. Because a career in science provided sufficient upward mobility to talented but

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economically challenged students, fellowships and scholarships provided an excellent vehicle for drawing these people into scientific careers in fields relevant to the DoD mission.

Now: Today, the best and brightest in our country, even those with limited financial resources, have access to higher education through the successful growth of both public and private assistance programs. Additionally, those students often find much greater potential for upward mobility in fields other than basic science (finance/banking, business, medicine, law). DoD fellowships and scholarships may continue to attract people to DoD relevant sciences, but they generally no longer attract the best and the brightest because of these additional, more attractive, opportunities.

The perceptive JASON Walter Munk, long involved in DoD basic research, sounds similar themes:

- The Secretary's proposal of additional investment by DoD in 6.1 research is most welcome. At this time it may seem revolutionary, but in fact it goes back to the earliest history of U. S. Government support for basic science. ONR was authorized by President Truman in 1946 for "planning, fostering, and encouraging scientific research in recognition of its paramount importance ... to the preservation of national security." At the time, ONR was THE national Science Foundation and its charge accordingly very broad. In 1950 when NSF was established it took many of its cues (and the Director) from the four-year old ONR, leaving it with a more restricted, but still remarkably broad, interpretation of its mission.
- By then the Cold War was in full swing. The start is usually identified with Churchill's speech on 5 March 1946 in Fulton, Missouri, "...an iron curtain has descended across the continent." On 11 October 1986, Gorbachev and Reagan met in Reykjavik, Iceland, signalling the end of the Cold War. During these forty years ONR fulfilled its mission in an exemplary way. What were the hallmarks of ONR then, and how do they differ from ONR (and more generally from DoD-supported science and technology) today? (I will speak from an ONR point of view because I am more familiar with its history.)
 - i. Decisions were made by powerful and independent project officers. These included some leading people. (Roger Revelle was an ONR Project Officer before he became Director of Scripps.) The Project Officers were intimately familiar with the work they supported; I remember instances in which they participated in sea-going experiments.
 - ii. I think the decisions were more oriented by WHO was making the research proposal than WHAT the proposal was about. As it turned out, the project officers chose well; many of the young scientists so supported became leaders in their field.
 - iii. The scientists so supported became familiar with Navy problems, available for offering meaningful advice on various advisory committees, instantly available in times of crisis (e.g., H-bomb dropped off the cost of Spain), and generally knowledgeable about the goals of the Department.

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- iv. The question of "Navy relevance" was always present but broadly interpreted. Relevance could be debated between the Project Officers and a cadre of scientists familiar with, and often critical of, existing Navy goals.
- v. The general "time constant" of support was decadal rather than annual or multi-annual.

These features (strong and independent project officers, people-oriented support of university scientists informed on Navy matters, a broad interpretation of Navy-relevance, and a tolerance of long-term risks) were critical to the mission. I think equivalent consideration would hold today.

- Perhaps I am painting too rosy a picture, but let me review the development of the SSBM program, the centerpiece of the Cold War. The marriage of two evolving technologies, nuclear-powered submarines and ballistic missiles, was proposed in 1956 (ten years into the Cold War) during Project Nobska (chaired by Woods Hole Director Columbus Iselin). This placed new and unparalleled requirements on our knowledge of sea floor bathymetry, and to a lesser extent on marine gravity and magnetic fields. The development of the SSBM system overlapped in time with the development of plate tectonics, a revolutionary change in the science of the Earth. Here again the requirements were for better bathymetry and magnetism. It is astounding to what extent the military and scientific requirements overlapped. This led to an unavoidable conflict between requirements for classification and publication.

One would have expected this to lead to a conflict between the naval and University communities, and to some extent it did (and still does). Many of the leaders of the plate-tectonic revolution, Menard, Dietz, Hess, ..., had close Navy connections. Menard and Dietz were employed in Navy Laboratories; Hess, a Princeton Professor was on active duty and retired as an Admiral in the Naval Reserve. Some of their uniformed colleagues participated in the scientific discoveries. It was not unheard off to hear scientists plead for more secrecy and Navy Officers for more transparency. This science-Navy connectivity was a highly desirable by-product of the underlying ONR culture.

- This special relation is now gone. Our students and young investigators do not know, nor care, about Naval requirements. And the developers of new Navy systems question the need for further basic research towards the understanding of the ocean environment. NSF has moved increasingly towards reviews by committees, generally averse to risk taking.

I have learned from many German and Russian colleagues that there never was much of a symbiosis between their military and university establishments. Perhaps our joint effort during the thirty Cold War years is not a singularity but a unique product of the American culture. I was greatly heartened to hear the DoD talk about a revival of this point of view.

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B APPENDIX: Two Fallacies about Basic Research

From 1945 until approximately the end of the Cold War, the support of basic scientific research by the U.S. Government in general, and by the Department of Defense in particular, seemed amply justified by the straightforward logic of the Vannevar Bush 1945 report, "Science: the Endless Frontier". Four literal quotes summarize the argument:

1. New products, new industries, and more jobs require continuous additions to knowledge of the laws of nature, and the application of that knowledge to practical purposes.
2. Similarly, our defense against aggression demands new knowledge so that we can develop new and improved weapons.
3. This essential, new knowledge can be obtained only through basic scientific research.
4. Moreover, since health, well-being, and security are proper concerns of Government, scientific progress is, and must be, of vital interest to Government.

Given the U.S. post-war dominance in essentially all areas of technologically enabled economic expansion, and given the U.S. emergence after the Cold War as the sole military superpower on the planet, it is hard to find fault in hindsight with the logic of the Bush report.

Nevertheless, since the 1990's, the syllogism has been challenged, somewhat successfully if one goes by an informal sense of "conventional wisdom", by a set of quasi-economic arguments often termed "neo-conservative". These are not unintelligent or unsophisticated arguments. If we believe that support of basic research by DoD is a good thing, we must be prepared to say what is wrong, or inapplicable, about the neo-conservative challenge. Much of that challenge is captured in two propositions:

1. *The net-present-value objection.* Because of its necessarily long time horizons (to 20 or 40 years), basic research, even when successful, is a poor investment when compared with other near-term investments. A continuously renewed portfolio of near-term industrial investment with 15% real return on investment (ROI) for 40 years yields a factor 267 real return. Few, if any, studies claim this magnitude of return for the average portfolio of basic research done in the year 1968, forty years ago. An equivalent form of this argument is that we should be willing to pay today only 1/267 of any payoff from basic research that is 40 years in the future – and that much only when the payoff is guaranteed (which is never the case).
2. *The globalization objection.* Because of the increased speed and efficiency with which pure knowledge is disseminated in a globalized economy, the profits from basic research do not accrue preferentially to the country that pays for it. In such a situation, it pays to let everyone else do the basic research, and to focus one's own investment on the transition from already-acquired knowledge to actual products – in other words, reaping the rewards from the other guy's research. Of course, if everyone takes this

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view, then all may be worse off – a classic “tragedy of the commons”. But, even foreseeing such a tragedy, it is not in the self-interest of any individual country to invest other than short-term.

These two distinct propositions are sometimes combined into the seductive proposal, “Let’s just buy the results of basic research when we need them.” The implication is that an efficient market will drive industry to invest, with its own capital, in just the right portfolio of basic research to meet future needs, and in a lean and efficient manner.

What is wrong with these assertions? Has the world changed so fundamentally as to invalidate the Vannevar Bush syllogism? Let us look at each objection in turn.

FALLACIES IN THE NET PRESENT VALUE OBJECTION

The first flaw in the net present value objection is that long term goals are often not fungible. An example may be useful. 50 years ago, the world-dominant Swiss watch industry ignored small-scale electronics and quartz technology. Were it not for small niche, high-end market it now plays in and trendy “swatches,” it would not even be a player in the wrist-watch scene today. A Net Present Value, or equivalent, criterion applied at the time would have indicated they should not be investing in electronic timepieces, and they did not. National Security must be bet on other methods and metrics.

Net-present-value calculations are, by definition, shorthand for the comparison of different routes to the same future goal. It is not meaningful to compare value now with value 40 years from now: Such a comparison depends on the individual’s personal utility function, and differs from person to person. What we can and do compare are two different strategies for getting to the same goal: (i) invest now in basic research and reap the rewards in 40 years (for this example), or (ii) alternatively, invest in other things instead of basic research, let the investment compound, and after 40 years cash out and convert the cash to the same or greater quantity of rewards that the basic research might have produced.

Implicit in the shorthand, but explicit in the long-form comparison, is the fungibility of the rewards. Net present value has no meaning if there is no way to convert the alternative strategy’s cash into the desired reward. For example, if the desired outcome of basic medical research is improved longevity and health delivered at lower cost, then this outcome is not fungible with any amount of compounded cash in the absence of the enabling research. The pure economics path forward, which we do not advocate, would be to assign a dollar value to the value of human life (its quality, length, and so forth) and proceed with the optimization. In a world with many non-fungible dimensions of progress, trade-offs evolve over time by a slow, often generational, social consensus. On shorter time scales, governments are expected to pursue a portfolio of goals, and not necessarily the portfolio that would be economically most efficient if there were complete fungibility at agreed-upon rates of exchange.

National defense, and national security generally, is an equally good example of long-term non-fungibility, and it is the example relevant to this report. A small country, Poland say, might take the view that there is a world-wide fungible market in national defense – arms,

materiel, personnel, intelligence, and so forth. If Poland sees a better return investing in other economic sectors than defense basic research, it should (arguably) do the former, reap the rewards, and buy its national defense on the world market. This is not an option open to the U.S. Because of its size, and prominence in world affairs, it is by far the market mover, not the small customer on the market margin.

The returns on investment for the strategy alternative to defense-related basic research are thus largely irrelevant: If we pursue the alternative strategy in the hope of profitably cashing out in the future, the desired level of national security will simply not exist at any price, and there will be no market to supply it. The situation is much closer to the example of medical research than it is to the Polish air force. As in the case of health, it is highly doubtful that the U.S. voter would accept an outcome along the lines of, "the U.S. is now fundamentally insecure, but you are somewhat richer and hence on balance better off".

The second fallacy in the net present value objection is that it confuses the expectation value (or average) outcome with the actual realization of a single unknown future.

The theory of portfolio management is by now well understood by economists and others. An enterprise seeks not merely to maximize its average rate of return, but to do so while quantitatively managing risk. Risk comes in two flavors, uncorrelated risk, which can be minimized by portfolio diversification, and correlated risk, which can be minimized only by reducing expected return along some risk-return trade-off function. Risk can be quantified in various ways. A relevant one here is "gambler's ruin probability". A small startup company, willing to take risks, might accept a 20% chance per year of going bankrupt in exchange for the spectacular return on investment of, say, 50% per year. It is unlikely that DuPont Corporation management would accept so large a gambler's ruin probability; they expect their company to be around in 30 or 50 years. What gambler's ruin probability is acceptable to the United States as a nation? If the American Civil War is our one near-miss in 230 years and if we observe its continuing social repercussions even today, we might conclude that the risk acceptable to most Americans for national ruin must surely be something rather smaller than 1% per year.

The characteristics of basic research make it an outstanding portfolio management tool at the national level. There is not one kind of basic research, but rather as many kinds as there are identifiable fields and subfields of basic science. These are independent, in the sense that almost any one of them could produce discoveries of high economic value, albeit with different likelihoods. They hedge against many identifiably different kinds of future risk, ranging from the emergence of military peer competitors, to climate change, loss of energy supply, and so forth. Even if a diversified portfolio of basic research were to produce, on average, a lower rate of return than betting the farm on some set of near-term applied goals, its benefit in portfolio management would be to diversify and thus reduce long-term risk; that is, to mitigate through diversity uncorrelated, but potentially individually disastrous, future scenarios.

A further point, somewhat tangential to this argument, is that the product of basic research is undervalued if it is determined by the impact on the company or the Service. The scanning tunnelling microscope, invented by IBM scientists, arguably did nothing for the company but has revolutionized S&T throughout the world.

FALLACY IN THE GLOBALIZATION OBJECTION

The globalization objection to U.S. Government, or DoD, funding of basic research is that the rewards of the research don't accrue to the country or agency that funds it. The fallacy in this proposition is that it flies in the face of a huge body of evidence exactly to the contrary. It is a statement about the theoretical economic construct of a perfectly globalized, perfectly efficient market, and not one about the present or foreseeable real world. Roughly speaking, while the Earth may have become "flat", it has not shrunk to a point, nor is there any reasonable prospect of this happening.

The "pipeline" metaphor, where technologies "flow through" a stage of basic research and "into" a development phase does not capture the reality of how basic research leads to economic advantage, and still less to economic benefit. Basic research is not linear. Rather, it spins off discoveries and technologies that are ready for development at unpredictable times. Each time such a spin-off occurs, a new opportunity is created for (what is taught in business schools as) "first mover advantage," defined (by Wikipedia) as *the advantage gained by the initial occupant of a market segment ... stemming from the fact that the first entrant can gain control of resources that followers may not be able to match.*

What are some first mover advantages as regards the products of basic research? At the top of the list: people. Notwithstanding accepted scientific standards for the publication of results, the published literature never can include the myriad of details, context, blind vs. productive alleys, experience base, and so forth, that is provided by direct access to primary researchers.

Next on the list: physical proximity. There is no coincidence in the fact that the U.S. Aerospace Industry was initially largely built in Southern California in close proximity to Caltech, that Silicon Valley surrounds Stanford, that Route 128 encircles MIT, that Qualcomm is in San Diego, that there is a cluster of high tech industry in Minneapolis-St. Paul, or ... (we could extend this list almost indefinitely).

Also important, and on the list, are subtle aspects of regional and cultural "style" that lead to large variances. By this we don't mean how people dress, or what they eat, but rather how they think about doing science and transitioning its output to development. Discoveries and technologies originating at American research universities and national laboratories bear the strong stamp of the common culture of the American system of higher education, and often of the common infrastructure of national facilities (for example, accelerator light sources). Transition to development is fastest and easiest in that shared cultural context.

None of these factors, as well as other factors that can dominate the first-mover advantage, are easily exportable off-shore. In failing to recognize this, the globalization objection to support of basic research commits the error of imagining not today's actual globalized economy, but rather an intellectual abstraction ("unlimited free movement of labor, capital, and ideas") that simply doesn't exist.

Discovery begets discovery. The U.S. has benefited enormously from a culture that values youthful creativity, iconoclasm, and individual entrepreneurship. If globalization were completely efficient, these American characteristics would now be universal in the world.

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But they are not. They remain our competitive advantages, and we should nurture them, while trying to inculcate them in the world at large so that we can benefit from the fruits of such work elsewhere.

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C APPENDIX: The Changing Character of the DoD's Basic Research Program

Over the past decade, there has been an exodus of scientific and technical expertise from the US government, and in particular, from the DoD research enterprise. For example, basic research programs in the physical sciences at the Office of Naval Research have disappeared, despite the long list of research achievements at ONR over past decades that have provided critical capabilities for DoD and US society more generally. Gone are many of the technically literate program officers who plied the streets of the scientific community to find those remarkable people who could help shape the future. Gone too are many of the scientists and engineers in the academic community whose research was supported by ONR, ARO, and AFOSR, and who contributed to revolutionary advances that changed the landscape of modern war fighting. And most importantly, lost is the opportunity to develop the next generation of scientific talent who would otherwise have been trained and capable of carrying the research enterprise forward.

In place of this community, we now have a new vision for the future, as expressed, for example, at the ONR website http://www.onr.navy.mil/sci_tech/

"You may have noticed that our list of science and technology departments has changed. The Office of Naval Research is reorganizing to better align its resources toward achieving Navy and Marine Corps science and technology goals and capabilities."

In other words, "We know what we need ('goals and capabilities') and implicitly "Funding is available for those who can follow instructions."

The vision expressed by this statement (which is but one example from many) is a recipe for a mediocre future for the DoD and our society. If such policies had been in place in the first 40 years following World War II, many current military capabilities that are fundamental to our national defense would never have been imagined, much less achieved. Indeed, DoD has largely eliminated basic research and redefined "product development" as the new, improved version.

Certainly, the applied sciences and engineering are critical components of the DoD mission to advance its capabilities. But what we have seen over the past decade is nothing short of a paradigm shift to a top-down organization of research managed by individuals with insufficient scientific literacy and with an outlook of short-term expedients. This situation is detrimental to our long term survival as a great nation.

The ONR website further lists the Nobel Laureates who had been supported. Such individuals do not generally fit the mold of research driven by BAAs. Rather, they were selected by a diverse set of remarkable program officers with a keen eye for people who might change the future (and many of whom did). In this context, we should remember Luis Alvarez who, apart from a Nobel Prize in Physics, also pioneered the use of radar for landing aircraft (in response to which BAA?). In his autobiography (1987), Alvarez states that

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In my considered opinion the peer review system, in which proposals rather than proposers are reviewed, is the greatest disaster to be visited upon the scientific community this century. ... I believe that U.S. science could recover from the stultifying effects of decades of misguided peer reviewing if we returned to the tried and true method of evaluating experimenters instead of experimental proposals. Many people will say that my ideas are elitist, and I certainly agree.

For decades, many DoD research offices carried forward the legacy that Alvarez championed. There were quirky program officers who walked the fields of science to find the people, rather than the PowerPoint, that would shape the future. Rebuilding such an infrastructure in the DoD is no small task. There are by now forces in opposition to the premise that great things can come from basic research and that these outcomes can provide powerful (unexpected) new capabilities to DoD. Resources that had been directed to sustaining a research community now go elsewhere.

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D APPENDIX: A Personal History of the DDR&E

This section was contributed by JASON Herb York, who was the first to hold the title of DDR&E.

THE CREATION AND EVOLUTION OF DDR&E

The Office of the Director of Defense Research and Engineering (ODDRE) was one of several radically new institutions created by the Eisenhower administration in response to the severe national angst engendered by the launch of the first Sputnik. Others included ARPA, PSAC (President's Science Advisory Committee), and NASA. The idea grew out of conversations among Defense Secretary McElroy, Deputy Secretary Quarles and Presidential Science Advisor James Killian. The idea was fully endorsed by the President himself and was incorporated in the Defense Reorganization Act of 1958. The basic purposes were to consolidate the management of all defense RDT&E in one place and elevate the new manager to the level of Under Secretary of Defense, the same level as the three Service Secretaries -- none of whom had the phrase "Under Secretary" in their actual titles.

The actual implementation of the idea was delayed until December 1958 -- fifteen months after Sputnik -- first by some delays in getting the legislation through the Congress, and then by further months of delay while the search for the DDR&E himself went on without an immediate result.

I was already in the Pentagon as the Chief Scientist of ARPA when they finally settled on me to be the first incumbent.

My authority included approval, disapproval, or modification of all RDT&E programs in the three military departments and all of the defense agencies, including DNA, ARPA, and NSA. Additional *de jure* responsibilities included advising the Secretary on all high tech procurement. Additional *de facto* responsibilities included review of all intelligence about foreign high technology, as well as all high tech means for obtaining such intelligence. And because I had moved to the Pentagon after four months of full time work in the White House, I retained my connections with Killian and the President throughout this entire period. I also continued to be a (now "*ex officio*") member of PSAC itself. In addition I frequently accompanied the Secretary of Defense -- or substituted for him -- on the Committee of Principals (all matters concerning Arms Control), the National Security Council, and the National Space Council. Typically I saw the Secretary virtually daily, I visited the West Wing weekly, and consulted directly with the President about once a month.

Such a remarkable set of powers and freedom of action could not possibly persist very long, and it didn't.

The diminution in DDRE's role took place in two distinct steps. The first, and lesser, was in 1961, when Robert McNamara became Secretary of Defense. He asked me to stay on; I did but only for four months.

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McNamara brought with him a lot of theories about proper management, including the notion that experts in general should primarily serve as advisors to their bosses and not exercise any substantial authority of their own. He also did not like the kind of free wheeling independent access I had to other power centers, such as the White House and the CIA. Even so, when Harold Brown replaced me as DDR&E in May 1961, McNamara recognized the great talent and knowledge that Harold possessed, and Brown remained a very influential member of his Staff, with only slightly diminished *de facto* authority.

The second and bigger step was in 1977. Harold Brown became Carter's Secretary of Defense, and recruited Bill Perry to be his DDR&E. After a few months, Brown changed Perry's title to Undersecretary of Defense for Research and Acquisition. (At the same time Brown also created an Under Secretary for Policy) With that new title, Perry continued to have all the special authorities of the DDR&E plus new and greatly expanded authority over all Defense procurement. For a time, the title DDRE was discontinued, but it was eventually reinstated, this time as a staff position reporting directly to the Under Secretary. Given the exceptional experiences and talents of both Brown and Perry, this arrangement worked very well for a time, but eventually the position of Under Secretary R&A was filled by people who knew much less about R&D and whose natural tendency was to focus principally on the larger part of his authority (the part of much greater interest to the Congress and the Military Industrial Complex as a whole), namely procurement.

Hence the current situation, in which the responsibility (but not the authority) over R&D is in the hands of people at a much lower rank than was the case before the creation of the new under secretary in 1977.

Given the current structure of titles in DoD, this could be rectified by dividing the responsibilities of the Undersecretary for R&A between two separate Undersecretaries, one for procurement and one solely dedicated to Research and Engineering with authorities and access like those exercised by the original DDR&E.

THE RELATIONSHIP OF DDR&E AND ARPA

In the beginning, DDR&E had the same authority over ARPA as he had over all other R&D in the Department of Defense, specifically the authority to approve, disapprove or modify all of ARPA'S R&D programs. However, as in the case of the Service Secretaries, the head of ARPA reported administratively to, and was appointed by, the SecDef. The one important difference was that in those earlier times he was nominated by the DDR&E.

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E APPENDIX: On the DoD and DOE Laboratories

There is, understandably, no direct connection between DoD laboratory research and DOE laboratory research and between the laboratories in each department doing this research. The 6.1 research interests of these two departments have substantial overlap, but their labs and research structures exist in parallel universes interacting only tangentially. There are indirect connections that we argue are important to understand, and it is also worthwhile to know something about the successes and failures of these laboratories in the DoD research context.

Our particular concern is the NNSA weapons science laboratories, Los Alamos and Livermore, which not only do their main mission of nuclear weapons stewardship but also do some work for DoD. There are also some laboratories concerned with both basic and applications-oriented research, such as Oak Ridge, Argonne, and Lawrence Berkeley Laboratory. As one last point in favor of making a comparison, it is perhaps worthwhile to point out that there is always some talk going around about how it would be better (for reasons we will not go into here) if NNSA and its laboratories were to be incorporated into DoD. Should this ever happen, DoD would have to understand the NNSA laboratories at a very deep level.

Although the weapons labs' primary concern is in the highly-classified area of nuclear weapons stewardship and research into the underlying science of these weapons, over the decades—and especially in the last decade—these labs have made a substantial impact on other research areas that we could loosely classify as 6.1, and in so doing have developed several major world-class facilities. Some of these facilities are available for unclassified research by outside users; some are not. On the current Top500 list of the world's most powerful supercomputers, number one is the Roadrunner (~1 petaflop/s) at Los Alamos and number two is Blue Gene/L at Livermore. Generally, these computers are available only for weapons research, but their offshoots will be available at other laboratories. Major facilities open (to some extent) to users include the National Ignition Facility at Livermore, as well as facilities such as the Spallation Neutron Source at Oak Ridge and the LANSCE neutron-scattering facility at Los Alamos.

With the help of such major equipment and other less well-publicized resources, the DOE laboratories have developed world-class reputations in a number of open science fields, including not only computers and high-power lasers, but lasers more generally, high-energy-density physics and several branches of materials science, inertial fusion, and turbulence. In areas more directly related to DoD interests, the weapons labs are strong in high-explosive research and related conventional weapons, as well as in certain intelligence applications. As a result, laboratories do a fair amount of non-nuclear national security work (now called "work with others" rather than, as in the past, "work for others") for DoD and DHS that has resulted in specific applications in such areas as radiation detection and in advanced conventional munitions.

Yet with all these strengths at the NNSA laboratories, they have significant weaknesses. Over the last Administration, these labs have lost much of the trust, and many of the lines of communications, that they used to have with decision-makers and, indeed, with DoD itself. They have had problems with budget overruns and with understanding how to run user

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facilities for unclassified research at laboratories whose primary mission is highly classified. Their "work with others" is more expensive per FTE than elsewhere, in part because of a legacy of fixed-cost but inefficient facilities stemming from Cold War days. Current budget cuts threaten the scientific pre-eminence of the labs in both nuclear weapons stewardship and non-nuclear areas, and hundreds of employees, including scientific staff, have been fired recently. Security breaches at the weapons laboratories have been treated particularly harshly and compliance with security and safety regulations has cost the labs much money, time, and wasted effort. Nevertheless, the laboratories remain the crown jewels of NNSA and of DOE.

How does this relate to DoD with its very different organization, structure, and missions, not to mention disparate size? Here are several questions that might be asked:

- The NNSA laboratories' strength in 6.1 research with possible defense applications flows directly from their strengths in weapons science; there is no NNSA or DOE equivalent of DARPA. In DoD, 6.1 research is flowing the other way, for the most part, from universities and laboratories to weapons, and much of this flow is organized through DARPA. Is the role of 6.1 research in the DoD laboratories appropriately balanced with such research flowing through DARPA?
- There is a role in these DoD laboratories for large facilities for 6.1 research open to outside users. Certain existing facilities, such as the David Taylor Model Basin and the Naval Waterways Experimental Station, have supported 6.1 research, but in a small way. If this role of user facilities were to be enlarged, what would these facilities be? Gas guns for materials research, hydrodynamics facilities, supercomputers, or other?
- Which are the "crown jewel" laboratories of DoD, and what have been their particular accomplishments entitling them to this role? Is there a useful way of comparing them to DOE crown jewel laboratories?
- Are these crown jewel labs of DoD suffering from the same problems facing the DOE/NNSA labs—stultifying regulations, large legacy cost burdens, heavy budget cuts, and the like?
- Should DoD and DOE/NNSA mutually seek for improved relationships in areas concerning 6.1 research? What advantages would be sought in new relationships, and what disadvantages overcome?

F APPENDIX: Novel Models for Student Support

Training Grants

The creation of prominent graduate education and research training programs would help DoD (re)build research partnerships with the academic community. Based on the success of graduate education and research training grant programs at other agencies (*e.g.*, NSF and NIH) and foundations (*e.g.*, HHMI), these awards bring significant prestige to faculty, departments, and universities. Such programs could help build a stronger DoD pipeline and enduring relationships with various universities.

Graduate education and research training programs could be promoted to foster cross-disciplinary graduate education or disciplinary graduate education. The programs should strive to enhance the graduate experience in ways traditional graduate programs cannot. The programs could be multi-institution and should facilitate diversity in participation and global connectivity. The programs could have strong connections to the DoD laboratories to expose the students to DoD research activities.

Typical graduate training programs at NSF and NIH are configured as follows.

- Most awards are five-year, renewable. Some very successful programs have been in existence for over 20 years.
- The awards annually provide 10–15 graduate fellowships and tuition. Tuition is typically capped at \$12–15k/year per student. Students are supported for 2 years on the training grant fellowships and move to sponsored research or institutional support for the remaining years, hence the concept of “training”. Some programs fund students for 4 years.
- The awards involve multiple faculty, frequently cross-institutional. Some faculty support is included for the educational activities, including curriculum development; outreach; administration of the programs; and other special activities created to meet the goals of the program.
- These awards almost always carry reduced indirect cost rates. The prestige of these awards out-weighs the burden of the reduced overhead rates to the institution.
- Nominal, one-time, funds are made available for infrastructure such as specialized equipment. This must be tied to the educational experience.

The NSF IGERT program is an excellent example of a cross-disciplinary graduate training program (<http://www.nsf.gov/crssprgm/igert/intro.jsp>). This program is designed to accelerate cross-disciplinary collaboration in the STEM areas. This program restricts the students funded with NSF funds to be US citizens, but encourages other student participation by leveraging funds from other programs within the university. Both pros and cons exist to this approach.

Disciplinary training programs exist within various disciplinary programs across the NSF and within the NIH. The goals of the programs are very similar, looking for revolutionary breakthroughs in education, strong integration of research and education, and expanding the pipeline of students that will pursue research careers in STEM areas.

DoD would need to define its goals and objectives for such programs. The DoD laboratories could be a critical component of the programs. That could bring a unique flavor to a DoD graduate education and research training program. (Note that the NIH and HMMI have some laboratory connections, but not fully integrated into all of their awards or as a required component of the awards.) The most important thing is for the DoD programs to be viewed as prestigious.

Vertical Integration Programs

NSF has a program model in the mathematical sciences that has been highly successful and is worth considering replicating in other areas. It is the Vertical Integration of Research and Education (VIGRE) in the Mathematical Sciences program (<http://www.nsf.gov/mps/dms/awards/vigreawds.jsp>). This program supports postdoctoral associates, graduate research traineeships; and research experience for undergraduates. The program targets integration of research and education and building a pipeline of future scholars at the earliest possible stages. The postdoctoral experiences under VIGRE include both research and teaching. Successful programs have substantial teaching training programs. One of the byproducts of this program is the creation of a broad network of scholars and broad dissemination of the educational, outreach, and research successes in the individual programs. What happens is that the postdoctoral associates leave the program, joining other academic departments or research groups, taking with them their experience and connections. DoD could leverage such programs to quickly build a strong academic infrastructure.

G APPENDIX: IEDs in Iraq

As an example of how a technical community could have been exploited to solve an urgent DoD problem, we recount the history of counter-IED (Improvised Explosive Device) technology development, in which previous JASON studies have played a role.

Several years ago, events on the ground demanded new tools and methods for tackling the very difficult problem of countering the IEDs plaguing coalition forces in Iraq. The early DoD response focused on rapid deployment, but premature introduction of hardware in theater generated a backlash against promising technical directions. As a result, technical roadmaps, training, and assessment functions for solving the IED problem became a national imperative.

The Joint IED Defeat Organization (JIEDDO), has since provided essential leadership and technical direction. But it was formed and funded far too late in the story and did not have links in place to the relevant research communities. Creating those links took a major effort by senior JIEDDO staff, further delaying access to expertise. JEIDDO funding was drawn from existing programs and many of its early activities were a "re-labeling" of existing work, rather than coordinated efforts that could be directed toward most promising areas. The opportunity costs of continuing dead-end projects were substantial.

We believe that the lesson here is that well-established links between Acquisition and Research and between DoD and the research community might have rapidly established a strategic plan necessary for an effective R&D effort and enabled a much needed triage of project ideas that had no serious chance of success.

These remarks should not be taken to imply that DoD-supported basic researchers should have already been working on the very applied problems of counter-IED. Rather, had such a community existed and been asked, it could have rapidly applied its skills to the roadmapping and triage tasks.

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H APPENDIX: Remarks on the NSSEFF program

DoD has initiated a program of grants to university researchers at US universities, called the National Security Science and Engineering Faculty Fellowship Program (NSSEFF) under the National Defense Education Program (NDEP). NSSEFF is described in a Broad Agency Announcement (BAA) administered by the Naval Postgraduate School. First proposals are due 3 October 2008.

The National Defense Education Program "supports both basic science and/or engineering research within academia as well as education initiatives that seek to create and develop the next generation of scientists and engineers for the defense and national security workforce." The stated purpose of NSSEFF program is:

to provide faculty and staff scientists and engineers from U.S. accredited, degree-granting academic institutions with a career enhancing opportunity through their association with DoD while at the same time they are conducting unclassified basic research in critical areas of interest. Outstanding researchers selected for award and granted Secret security clearances will participate in all NSSEFF activities that are designed to enhance their understanding of critical research needs and interact with DoD senior leaders.

The "critical areas of interest" are defined to be:

(1) astronomy, astrophysics and space sciences; (2) atmospheric sciences and meteorology; (3) aviation science, astronautics; (4) behavioral and social sciences, including psychology and training; (5) biological and medical sciences, including biochemistry and biotechnology; (6) chemistry (physical, organic, polymer) and chemical engineering; (7) communications and networks; (8) computer and information sciences; (9) earth sciences and oceanography; (10) materials—functional materials, including electronic and bio-inspired materials, textiles, adhesives, etc.; (11) materials—structural materials, metallurgy, ceramics, refractory materials; (12) mathematics; (13) mechanical, industrial, electrical, civil, and marine engineering; (14) physics, including acoustics, fluid mechanics, optics, spectroscopy, nuclear physics, etc.; (15) propulsion, engines, and fuels; (16) robotics, science of autonomy; (17) weapons and military sciences, countermeasures, including counter-WMD and counter-directed weapons science; and (18) other.

Prospective applicants are advised that "proposed research should focus on innovations that enable revolutionary advances rather than evolutionary improvements to the existing state of practice."

The BAA defines the criteria by which proposals are to be selected, listed in order of importance to be:

Criterion I: Relevance and potential contributions of the proposed research to the advancement of Desired Capabilities S&T Investment areas and Enabling Technology

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Investment Areas or DoD missions, including the advancement of other areas of interest;

Criterion II: Scientific, engineering, and technical merit of the proposed research;

Criterion III: The qualifications of the Proposer, ability to perform the proposed work, and history of performance;

Criterion IV: Sound technical and managerial approach to the proposed work, including a demonstrated understanding of the critical technologies, challenges, and strategy to address those issues, including a risk mitigation strategy.

The BAA estimates that ten awards of up to \$600K will be granted, each typically renewable up to a maximum of five years. The document describes the objectives and potential impact of the NSSEFF program to be:

The NSSEFF program ensures that our Nation has an active, long-term and aggressive research and engineering portfolio that attracts the foremost creative, innovative and productive university faculty scientists, engineers and their students. Objectives of the program are to:

- *Recruit and retain highly innovative, results-oriented, university researchers that support military needs*
- *Create a forum to recognize and reward outstanding researchers*
- *Demonstrate DoD commitment to high quality university researchers*
- *Familiarize select university researchers and their students with DoD missions, operations, and technology to enhance their understanding of DoD's current and future challenges*
- *Foster long-term relationships between science and engineering faculty members and the DoD*
- *Increase the number of exceptionally talented and cleared technical experts that are contributing to DoD's mission and upon which DoD may draw from to serve on advisory boards, panels and groups.*

There are many attractive aspects of the new fellowship program. In particular, we enthusiastically endorse the program objectives listed above. Indeed, we view successful attainment of these objectives to be of paramount importance to the technical future of DoD and, indeed, of the Nation. However, we judge that the program, as put forward in the BAA, is unlikely to meet its overarching objective of ensuring *that our Nation has an active, long-term and aggressive research and engineering portfolio that attracts the foremost creative, innovative and productive university faculty scientists, engineers and their students.* Our reasons behind this negative assessment are:

1. **The scope of the program is inadequate.** The size and duration of the individual awards seem reasonable, but the program lacks strategic reasoning on what might

constitute a critical mass of researchers needed to meet its objectives. The long-term plan for continuing the program beyond the initial phase described in the BAA is not specified. We estimate that the minimum number of first-class researchers needed at any given time to form a critical mass needed to achieve DoD's laudable "networking" goals is at least five in each of several areas of science and engineering critical to national defense needs. Further, it is hard to imagine that any serious impact will accrue from supporting less than, say, twenty general research areas. Thus, if ten new awards are introduced every year, the program will eventually reach a level about one-half the minimal program we envisage.

2. **The implied narrow focus on potential subject areas is unwise.** This is likely to reduce the attractiveness of the program to the top researchers being sought and, in any event, is too short-sighted for DoD and national research needs, as discussed elsewhere in this report.
3. **The selection process is not credible.** In order to attract proponents at the levels needed to satisfy the program's crucial goals, the selection process must be transparent and perceived by all to be carried out by researchers of the highest stature.

The advice given in the BAA that the "proposed research should focus on innovations that enable revolutionary advances rather than evolutionary improvements to the existing state of practice" strikes us as being directionally correct, but operationally naïve and potentially damaging.

Near the bottom of the section describing eligibility requirements, the BAA states:

Proposers invited to submit full proposals and not holding a current DoD clearance must apply for a security clearance by 5 September 2008 and must be able to obtain and maintain a DoD security clearance at the Secret level.

A requirement that all awardees must actually obtain and maintain a DoD security clearance could well be counterproductive to the goals of establishing links between DoD and the most productive researchers in technical areas of interest to DoD. Such a requirement also has the potential to force some universities to opt out of the program. We suggest that ability to obtain a clearance be made a desirable attribute to be considered in evaluating proposals, not an absolute requirement.

Recommendations

Recognizing that the initial program is underway, we recommend that a concurrent reassessment be launched immediately to address the defects identified here and to respond to lessons that will be learned during the first round of proposal reviews. The changes that we recommend are:

- Increase the number of fellowships awarded each year to reach a steady-state community of at least 100.
- Define strategic areas broadly with consideration of the critical mass of researchers within a general area that will be needed to achieve program objectives.

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- Change the order of selection criteria to emphasize the quality of the people proposing the work and technical merit of the proposed research.
- Devise a selection process that will be trusted by the target research communities.
- Eliminate the requirement that all awardees obtain and maintain DoD security clearances.

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