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DEFENSE PROGRAMS
SUBCOMMITTEE REPORT

Science and Technology in the Stockpile Stewardship Program

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SUMMARY

Findings: The vision of how NNSA supports national security in the broadest sense and, more specifically, how it is to provide stewardship for the enduring nuclear stockpile has not yet been clearly formulated. The full program to accomplish the three essential tasks of stockpile stewardship – surveillance, science and production – needs to be defined using a credible, bottom-up Planning, Programming and Budgeting System in order to prioritize activities.

Recommendations: **1)** Implement the NNSA Strategic Plan by providing a financial management structure (PPBS) that clearly identifies resources allocated to i) products (warheads) with inputs related to required outputs, ii) broader S & T to meet future challenges to include "unknown unknowns," and iii) other facilities and infrastructure. **2)** Develop a statement of Laboratory missions and integrated goals, and their implementation through a balanced and clearly-prioritized set of programs directed by technically-competent leadership. **3)** Identify a ruthlessly limited set of i) deficiency-related needs to sustain the stockpile, ii) changes to warheads that have expected longer-term benefits, and iii) "unknown unknown" challenges that need priority work.

Findings: Certification is ill defined and unevenly applied, leading to major delays and inefficiencies in programs. Peer review is not leading to full resolution of certification issues, but is itself uneven and problematic.

Recommendations: **4)** Document an agreed upon set of margins for the output-input chain of events in producing the expected yield of each type of warhead in order to define the work required to achieve certification, from components to subsystems. **5)** Ensure that peer review brings timely closure (not necessarily full agreement) on each certification effort. **6)** Use a broader community than has traditionally been the case for peer-review, and employ "Red Teams" in order to challenge ongoing certification efforts.

Findings: There is no clear mechanism for ensuring balance between major facilities or initiatives and smaller-scale activities, with prioritization to guarantee the health of the latter. The imbalance between numerical simulation and experimental validation needs to be addressed.

Recommendations: **7)** Subject all major facilities or initiatives being newly considered to a formal process of critical review that includes: i) a written proposal describing the rationale, and the expected costs and benefits of the intended effort; and ii) an independent and critical evaluation of the technical aspects, as well as other benefits for the SSP (e. g., in maintaining expertise). **8)** Ensure that the Laboratory mission, supported by NNSA, explicitly includes responsibility for proper balance and integration across the S & T programs, including between numerical simulation and experimental validation, as well as between major facilities or initiatives versus small-scale research. **9)** University-Laboratory collaborations in both experimental and numerical simulation should be supported, with the selection based on a rigorous process of independent, critical peer review in order to maintain the highest standards of quality in research and the most positive impact on the SSP.

Findings: Experiments at the Nevada Test Site and ongoing design activities do offer a means of retaining nuclear weapons expertise.

Recommendations: **10)** Perform a full analysis of the costs, benefits, scope and schedule for both i) any required underground nuclear explosion tests and ii) the enhancement of current test readiness based on realistic and detailed scenarios. **11)** Develop a clear-cut process for deciding the conditions under which nuclear explosion testing would be recommended on technical grounds. **12)** New design concepts should be given a thorough, timely vetting with respect to their potential technical, military and nonproliferation value.

I. PREFACE

I. 1. Charge

The Defense Programs (DP) Subcommittee was asked to review the science and technology (S & T) portfolio of the NNSA's Stockpile Stewardship Program (Appendix A). The focus of the review was to be on activities associated with certification of the United States' nuclear weapons stockpile, from individual components to entire subsystems, keeping in mind both short-term deliverables and long-term needs for national security. In addition, the subcommittee was to consider advanced concepts designs and test readiness, especially as means of retaining and recruiting expertise in the U. S. nuclear weapons community. The schedule of the review was set by the desire to inform the NNSA Advisory Committee (NNSA AC) in time for interim recommendations (Nov. 1, 2001 deadline) and a final report (Mar. 1, 2002 deadline) that the AC is committed to making on a combined DP and NN charge.

Given the broad charge, the DP Subcommittee concentrated on research and development at the 3 NNSA National Laboratories, Los Alamos (LANL), Lawrence Livermore (LLNL) and Sandia (SNL), and excluded a number of related activities that warrant attention in their own right. For example, there was far too little time to do justice to the Production Plants of the U. S. nuclear-weapons complex, although we did examine production within the National Laboratories (pit production at LANL, in particular). Similarly, the group did not consider the "Annual Certification" provided by the DOE and DoD to the President, as it is highly specialized compared to (and also depends on) the more general certification procedures that we did examine.[1]

In addition to keeping our task focused, we benefited from -- and avoided duplicating -- a large number of recent studies bearing on our charge. We specifically attempted to update the findings of the Commission on Maintaining United States Nuclear Weapons Expertise.[2] We also performed our review in light of the work to Assess the Reliability, Safety, and Security of the United States Nuclear Stockpile,[3] and took into account a recent report to US Strategic Command's Strategic Advisory Group (SAG) because of that study's bearing on test readiness.[4]

The Subcommittee met on Aug. 13-14 at LANL (presentations from LANL, US Strategic Command and SNL) and Aug. 27-28 at LLNL (presentations from LLNL, the Nevada Test Site [NTS] and the University of California [UCOP]), and several of its members attended a full NNSA AC meeting on Aug. 15 at SNL. We are deeply grateful to the speakers for their briefings, and to the Laboratories for hosting our meetings; the present review would not have been possible without the generous support provided to our committee.

I. 2. State of Health of the Stockpile and the Stewardship Program

In order to provide context for the subsequent discussion, we begin with a summary of the assessments that the Subcommittee heard about the current state of health of the U. S. nuclear-weapons stockpile, and the NNSA's Stockpile Stewardship Program (SSP).

Briefly put, the overall assessment we were given is that the stockpile is reliable and safe, but the stockpile is showing signs of aging and birth defects are being discovered which indicate that previous reliability assessments were optimistic. The Stockpile Stewardship Program is

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[1] The "Annual Certification" reports on the lack of a need to resume underground nuclear explosion testing (or deviations from such a position), rather than being a direct certification of the weapons or their components. These are now labeled "Annual Assessments," as they have evolved to also include an assessment of the devices and their components for which NNSA has responsibility.

[2] March 1, 1999 Report to Congress and the Secretary of Energy (H. G. Chiles, Jr., Chair).

[3] FY 1999 (November 8, 1999) and FY 2000 (February 1, 2001) Reports to Congress (J. S. Foster, Jr., Chair), along with responses to questions for the record posed following testimony of June 26, 2001. The FY 2001 analysis is being completed in the same time frame as the present report.

[4] Stockpile Assessment Team's April 2001 Stewardship Conference Report (J. Birely, Chair).

expanding knowledge of warhead performance through the development of improved surveillance, modeling, and simulation tools. This greater scientific understanding is enabling the identification of problems and uncertainties which were previously unrecognized. There are specific issues of concern that should not be trivialized, but these are judged to be within the bounds of what can be expected for the large number of diverse and complex systems making up the enduring stockpile.

We specifically did not find evidence at present that a nuclear explosion test is required. Barring the development for deployment of new designs, we can only envision on technical grounds that testing would be resumed in response to an as-yet-unforeseen significant new problem with the stockpile or significant exacerbation of an existing problem. Our conclusion is supported by the current "Annual Certification" indicating that there is no need, at the present time, for underground nuclear explosion testing in order to sustain the reliability and safety of the stockpile.[5] However, this report also indicated that there are issues, not currently requiring testing, that could lead to such a need.

Based on these views, the conclusion is that stockpile stewardship has been effective , in that the nuclear deterrent remains reliable and safe (specifically, NNSA's contribution thereto, which is all that the Subcommittee considered). Future reliability depends on a strong, well-planned and clearly articulated program being in place, however. As elaborated in the following sections, the Subcommittee found reasons for concern in this regard. Nevertheless, it is important to clarify that the context is of a process that has historically been effective, and yet one that can also be improved within the domains of the SSP as currently defined. We concur with the Foster Panel's conclusion that the NNSA organizational structure can be made to work successfully, so the focus of our review is on people and processes.[3]

It is worth emphasizing the Subcommittee's finding that the workers undertaking the stewardship activities remain extremely dedicated to their mission, and committed to high standards. If there is any failing, it is not that the stewards have stopped caring. Instead, the problems reside with inadequate prioritization of deeply-felt needs of many different kinds and at many levels: short-term vs. long-term objectives, and delivery of products vs. development of new capabilities, for example. Finally, the constructive, active engagement of a vital Nuclear Weapons Council (NWC) can only help ensure that these recommendations are implemented in as productive a manner as possible.

II. BALANCE BETWEEN S & T AND OTHER SSP PRIORITIES

II. 1. Priorities in Defense Programs

There are at least three categories of priority issues involved in sustaining the stockpile:

- The adequacy of resources and the balance among resource allocations to accomplish the three essential tasks of stockpile stewardship – surveillance, science and production;
- The balance between defined requirements to maintain stockpile weapons (e. g., through refurbishment, remanufacture and other elements of the "Life Extension Programs"), and building knowledge and tools to address unknown future challenges, including maintaining the necessary infrastructure at the Laboratories and Plants;
- The definition of how much is enough in building the science-oriented knowledge and tools.

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[5] The classified Laboratory Directors' letters (most recently issued in September, 2001) and associated reports on which the Annual Certification is based summarize any detailed issues of concern for each weapon system.

On the issue of resources, there are three essential tasks required to maintain the stockpile: i) surveillance to understand what issues exist or are developing in the stockpile; ii) knowledge and tools to understand what needs to be done and how to do it; and iii) production capability to implement the defined actions.

For the first five years or so of the DOE/NNSA's newly defined SSP, there has been little relationship between the magnitude of the three essential tasks and the defined resource needs. At the outset, it was not entirely clear what would be required to develop the capability to perform the essential tasks. The specific work required to maintain the enduring stockpile weapons is still not adequately characterized, and hence the production workload not adequately defined. NNSA has provided a reasonably credible analysis of the shortfalls in capability, but has not provided a bottom-up picture of a total program. In particular, the lack of a credible Planning, Programming and Budgeting System (PPBS) has been a formidable obstacle to gaining external support for additional resources for stockpile stewardship. This remains a significant deficiency. Until it is corrected, stockpile stewardship needs will not be placed in a coherent priority position with other national-security needs.

The Subcommittee was also impressed by the need for a coherent mission statement and integrated plan at the level of the 3 NNSA National Laboratories. Dual revalidation and cogent (not necessarily uniform) criteria for certification deserve increased attention. Although the present stockpile contains weapons and components with pedigrees linked to individual laboratories, the challenge ahead is to develop constructive competition between the laboratories in order to maintain the current stockpile without costly and unnecessary duplication of effort or wasteful rivalry for resources (see Item 4, p. 27 of the Chiles Commission Report[2]).

In addition, it is an essential responsibility of the laboratories to define the appropriate balance among S & T activities. How this is to be accomplished needs to be made evident. For example, a proper prioritization and balance must be ensured between numerical simulation and experimental validation, as well as between small-scale research and major facilities or initiatives (addressed in Section III.1 below). The emphasis on prioritization throughout the present report suggests that the need for maintaining expertise[2] must include leadership: the ability and willingness to prioritize appropriately. It is a priority for the Laboratories to mentor and otherwise help develop technically-competent leadership capable and responsible for making prioritized decisions for the SSP (e. g., the Sandia Intern Program).

On balance, between known needs for surveillance and refurbishment activities, on the one hand, and the need to be ready to address future unknowns on the other, the focus is currently on the defined needs of the Stockpile Life Extension Programs (SLEPs). This focus makes the assumption that the demands of the SLEPs, as now defined, are the highest priority for the science-oriented program. Notably:

- The current priorities for SLEPs – W76, W80 and B61 are a mixture of addressing defined deficiencies in stockpile weapons and changes believed to have long-term benefits. In each case, the approved SLEP includes changes that go beyond identified deficiencies or even projected deficiencies.

- We do not see an adequate effort to identify the combination of plausible problems in the stockpile that would be both highly consequential and beyond the scope of the knowledge and tools being developed to enable life extension programs.

- The focus on SLEPs to the exclusion of readiness to deal with future unknowns also leads to planned changes to warheads that may prematurely introduce performance risks: i. e., changes that do not address near-term issues, and are made before having the knowledge and tools needed to safely incorporate changes that are believed to provide long-term benefits. This approach also leads to near-term resource consumption. While near-term spending to provide long-term benefits is a generally desirable approach, it is sustainable only if there are enough resources to survive the near-term challenges. At present this is not the case, hence near-term austerity and "change discipline" seem appropriate.

To address the issue of how much is enough in science-oriented knowledge and tools, warhead performance needs to be treated as a series of outputs and inputs with the focus on margins of performance in each step of the process that produces an expected nuclear yield. We need confidence in minimum required outputs of each step in warhead functioning, and an acceptable margin in the output. Defining existing and acceptable margins can provide: i) criteria for confidence in weapons function and performance; ii) prioritization of science-oriented work; and iii) prioritization between science-oriented work and capability to implement the changes defined through science-oriented work – i. e., production capabilities.

Presentations to the Subcommittee did not give the impression of a coherent system of hard-nosed evaluations, in this regard. For example, it is difficult in hindsight to identify why certain SLEP decisions were made and who takes responsibility for them, especially in the light of the downstream costs that those decisions have turned out to imply. Moreover, it appears difficult if not impossible to modify planned Life-Extension activities as new developments arise, often more for institutional reasons (e. g., acknowledging that a task is more difficult or expensive than initially anticipated) than for technical reasons. The finding of the "30-day Review"[6] that the "process for generating program requirements needs attention" (sections 7.2.4 and 7.2.6) still holds true, and the corresponding recommendations have yet to be effectively implemented.

Recommendations

Define the full program needed to accomplish the three essential tasks – surveillance, science and production – using a credible, bottoms-up PPBS in order to prioritize activities within fiscal constraints. In order to accomplish this task most effectively, we recommend that the program be guided by the NNSA's over-arching Strategic Plan (§1, below), as well as by a joint statement of mission and implementation by the NNSA laboratories and plants (§2). We further recommend more intense prioritization of the stockpile life extension activities (§3).

1) The vision -- described in the Strategic Plan[7] -- of how the NNSA supports national security in the broadest sense and, more specifically, how it intends to provide stewardship for the enduring stockpile needs to be decisively implemented. This should be done by providing a planning and financial management structure (PPBS) that clearly identifies resources allocated to i) products (warheads) with inputs related to required outputs, ii) broader S & T to meet future challenges to include "unknown unknowns," and iii) supporting facilities and infrastructure.

The NNSA needs to communicate the agency's vision and basic process for fulfilling that vision -- communicating externally (e.g., to DoD, Congress and the Administration) to win support for the vision and its implementation, and communicating internally to guide prioritization at all levels. It is a document such as the Strategic Plan that should express the rationale for supporting basic science and technology as an intrinsic component of national security (i. e., a "deterrent" based on pushing the leading edge of S & T), and therefore as one of the NNSA's core objectives. It must lead to effective prioritization, which we currently see as too weakly and unevenly applied.

2) There needs to be a joint statement of mission and integrated goals, and their implementation for the 3 NNSA (DP) Laboratories, with the intent of clarifying their overall objectives and specific roles. We suggest that such a plan should engage -- perhaps even emerge

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[6] "U. S. Department of Energy Stockpile Stewardship Program 30-Day Review" (Nov. 23, 1999).
[7] "NNSA 2001 Strategic Plan" (Oct. 5, 2001 Draft).

from -- the lower ranks. A recent white paper[8] describing collaborative activities between LANL and LLNL offers a good starting point for a document that would include all NNSA Laboratories, and cover engineering and manufacturing as well as science. Like the agency-wide Strategic Plan, the statement of Laboratory missions, roles and responsibilities is intended to facilitate definition of stockpile stewardship in terms of a prioritized set of activities. An open, declaratory policy explaining the priorities and processes for accommodating budgets less than anticipated is needed as part of this document.

3) The scope of the science-oriented work needed to support essential weapon-maintenance programs (both as SLEPs and otherwise), and to address a set of the most plausible and consequential "unknown unknowns" must be defined. The need is to identify a ruthlessly limited set of i) deficiency-related needs to sustain the stockpile, ii) changes to warheads that have expected longer-term benefits, clearly documenting the tradeoffs between making such changes and not, and iii) a set of "unknown unknown" challenges that need priority work. A special effort needs to be made to develop a process for identifying and addressing the key "unknown unknowns."[3]

II. 2. Pit Production

The Subcommittee examined the status of pit production at LANL in order to consider how well production of components can coexist with science and technology. In particular, the concern has been expressed that establishment of production at a NNSA Laboratory may fundamentally undermine sustaining premier capabilities in scientific research. Moreover, delays in the program have attracted considerable attention, leading to public expressions that the Nation is unable to remanufacture key components for the enduring stockpile.

We judge this last assessment not to be a fair characterization of pit manufacturing at LANL. Breaking the problem down into two parts, production per se and certification, we address the second in a later section. Indeed, we found that production of "certifiable" units is underway at LANL, and the program appears to be well on its way to being properly managed -- a significant improvement over the past state of affairs. There were discussions about specific technical issues (e. g., relating to Sheffield Gauges), and there remain more general concerns about long-term planning for facilities handling special nuclear materials, but the detailed procedures for essentially all aspects of pit production appear to be in hand. In this sense, we conclude that LANL has clearly demonstrated the ability to build pits.

Given the ability to produce certifiable pits, can they be manufactured in adequate quantities? Production capacity is a matter that we could not address in all of its ramifications, because the numbers involved depend on 1) the future constitution of the stockpile and 2) unexpected contingencies. A study is underway,[9] and the current surveillance program should uncover possible changes (e. g., due to aging) in time for enhancement of production, if such were needed. Of course, decisions to change the stockpile are of a different character than unanticipated aging effects, as they are not imposed by nature but are the result of conscious deliberations that should take into account ramifications for production.

We recognize that a large production capacity would require the construction of a dedicated plant outside the Laboratories.[3] However, the Subcommittee did not find a good case for inferring that the currently planned level of production is incompatible with respect to the mission or activities of a NNSA Laboratory.[10]

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[8] Aug. 20, 2001 letter and white paper "Collaboration between Los Alamos National Laboratory and Lawrence Livermore National Laboratory in the Stockpile Stewardship Program" (J. C. Browne and C. B. Tarter).
[9] J. Freedman study of Modern Pit Facility Requirements for NNSA (October, 2001).
[10] The concern that significant production activity detracts from a science and technology program is not supported by the evidence at hand. Presentations to the Subcommittee described a successful group at LANL that engages in the full spectrum of activities, from basic research to the production of thousands of detonators each year. Far from production undermining the research, efforts across the full gamut of activities are mutually reinforcing, with

II. 3. Certification and Peer Review

Peer review is essential for maintaining a credible certification process, especially of new (e. g., remanufactured) components. We do not consider here the certification of entirely new designs for the nuclear subsystem, as new designs would be expected to require nuclear-explosion (underground) testing before being accepted for the enduring stockpile (the status of nuclear-test readiness is addressed in a later section). But we do include the certification of "functionally equivalent" new components as well as of existing (aging) components and their interactions, which implies being able to reliably distinguish components of a significantly new design (i. e., requiring testing in order to be stockpiled) from those that are appropriately "equivalent" to existing components.

Avoiding incremental changes effectively transforming a component or subsystem to a "new design" over time is a significant concern that warrants ongoing study and peer review. "Change discipline" is essential.

Our finding is that the certification process is ill defined and unevenly applied, leading to major delays and inefficiencies in programs. Peer review is not leading to full resolution of certification issues, but is itself uneven and problematic to the point of being labeled "dysfunctional" in certain notable instances.

Pit manufacturing is a case in point, with certification of remanufactured pits rather than actual production of certifiable pits representing the major bottleneck. This example illustrates some of the most troubling aspects of the current certification processes, with the relevant parties in the peer review having in some cases been unable to reach a consensus of how certification is to be achieved. Under these circumstances, it is hard to understand how experimental and computational programs intended to lead to certification could have been formulated; yet it is these programs that define the timeline for producing the first run of certified pits. Moreover, little time is scheduled between the final (proposed) experiments and the deadline for completing production of the first run of certified pits.

More generally, one would imagine a well-defined process to arrive at an agreed set of margins for the output-input chain of events in producing the expected yield of each type of warhead to provide the criteria for how much is enough in science-oriented work. This has not **!Unexpected End of Formula**been achieved: margins must be defined an appropriately optimized.

Instead, there remains an occasional (but still excessive) reliance on the traditional approach, in which one individual takes responsibility for each subsystem and exercises what amounts to "expert judgement" in deciding certification criteria. The historical reasons for -- and success of -- this approach are readily understood, but it is anathema to the "science-based" approach of the SSP. Put more bluntly, certification must be defined in a sufficiently coherent manner that the process can be communicated and justified to the next generation of stewards for the stockpile. We expect the next generations to be as sophisticated and engaged as previous generations, and therefore to require an explicit understanding of why a certain approach is used to certifying every component and (sub)system. In this sense, clearly formulating and communicating the certification process is exactly the scientific method demanded of the SSP.

An important means of achieving this level of scientific objectivity in the certification process is to vigorously apply the peer-review process. We understand that the "dual revalidation" procedure applied to the W76 was so thorough and redundant that it became too time-consuming to apply to every other weapon type. Nevertheless, a peer-review process of "dual" validation must remain the underlying means of ensuring quality control, and of enhancing the likelihood that "unknown unknowns" are identified early. For this reason, we

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development of new designs and applications of new diagnostic methods to testing existing units contributing to the intellectual challenge, just as does the basic research.

support the current activities of "dual rebaselining"[11] and advocate that this approach be expanded.

Recommendations

The peer-review process must be strengthened, and must evolve to be in line with the science-based foundation of the SSP. This means that the approaches used for certification must be well documented. A recent white paper[12] offers a starting point, based on defining margins and uncertainties for key components within the weapons, but consensus on the general approaches and their specific implementation must be achieved among all concerned parties.

Documentation does not imply that a given approach to certification is to be permanently established. To the contrary, a key reason for documentation is to clarify how the process evolves with time. One expectation of the SSP, for example, is that ongoing study and the application of improved tools for surveillance and research will lead to a better understanding of the stockpile. As that understanding evolves one would expect certification to evolve as well, with stiff peer review ensuring ongoing excellence in the processes.

The objective of peer review is both to maintain quality, and to encourage different perspectives or new ideas to emerge. That is, peer review is intended to help the certification process to evolve with time. We therefore recommend that there be a conscious effort to increasingly engage a broad community of individuals in each peer-review activity. For example, when reviewing a particular weapon it is useful to involve individuals who have extensive experience or knowledge in other domains, and potentially none regarding the particular system under consideration. A broader, more open review process is a means of challenging long-standing assumptions and of coming up with new perspectives. In particular, it is of paramount importance for the 3 Labs to identify better processes for uncovering the "unknown unknowns."

We also encourage the use of "Red Teams" in order to uncover any weaknesses in the certification process. Such teams would ideally combine both expert knowledge and more distant perspectives (intelligent people not necessarily familiar with the particular issues), and could well involve parties from both design laboratories, all 3 NNSA laboratories and outsiders, as appropriate. We recognize that peer review and "red teaming" require that independent perspectives be offered by the different laboratories, but this can be accomplished both through the traditional inter-laboratory challenges and through teams combining the talents of the different laboratories.

In order for peer review to be effective, however, a discipline must be imposed for achieving consensus in a timely manner (e. g., ~ 1-year timescale or less, depending on specifics). Recognizing that certification is expected to evolve, such consensus might be altogether temporary, thus reinforcing the need for timely closure.

III. BALANCE AMONG SSP S & T ACTIVITIES

III. 1. Major Facilities and Initiatives[13]

The SSP requires a broad range of activities, from Directed Stockpile Work to basic research in science and engineering. The latter is intended to widen and deepen the understanding of the weapon systems -- enhance the scientific foundation of stewardship -- as well as to help maintain expertise through recruitment and retention.

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[11] A less exhaustive joint review by the 2 design laboratories, LANL and LLNL, affording more flexibility and less redundancy than "dual revalidation" for ongoing characterization of stockpile health on a system-by-system basis.

[12] "Certification: A LLNL Perspective" (M. Anastasio, July, 2001).

[13] JASON Summer 2000 Study on "Stewardship Campaigns" (R. Jeanloz, lead) and Summer 2001 Study on "Advanced Radiography" (C. Stubbs, lead).

A key need for balance is between the major facilities and initiatives, such as the Accelerated Strategic Computing Initiative (ASCI) or the National Ignition Facility (NIF), and smaller-scale research efforts (a topic revisited below). The former represent long-term investments in the SSP's intellectual infrastructure, and have a correspondingly large impact on the NNSA's budget. The need for such long-term investment is established in general terms, but it is crucial that each major facility or initiative 1) have a clearly identified role in the SSP mission, and 2) be sustainable without jeopardizing the large number and diversity of other S & T activities. Optimally, a major facility or initiative also enhances recruitment and retention.

Future projects must therefore be vetted for their potential contributions in both research and maintaining expertise (relative emphasis on the two may vary from one case to the next). Because of the large budgetary impact, it is all the more important to expect a high standard for establishing a major new facility or initiative. Multi-disciplinary and multi-laboratory critical analyses offer one mechanism for ensuring that the required standards are maintained. This recipe is not much different from the peer review process described above (perhaps with increased involvement of individuals from outside NNSA).

Another balance that must be achieved is between numerical simulation and experimental validation. ASCI has been successful in achieving a new level of numerical simulation, but it is crucial (and recognized by ASCI's participants) that renewed emphasis be placed on experiments essential to calibrate and validate modern simulations, as well as to properly define specifications for the SLEPS. Some (e. g., hydrotest and subcritical experiments) require large facilities, but many others involve smaller-scale laboratory research.

A more general concern is that because of the higher visibility and budget impact of major programs, they inevitably become strong driving influences on the overall program. As a result, the essential smaller-scale efforts are more difficult to fund, due to low visibility compared with "flagship" major facilities or initiatives, and their very existence is threatened by even (relatively) small budget overruns in major programs. "Benchtop" studies are important not only for the high value of knowledge returned, but also because of their role in maintaining expertise: many researchers are motivated by pursuing independent ideas through small-scale studies.

It is thus crucial that the SSP support such smaller-scale research, along with the major initiatives, and it is the responsibility of National-Laboratory management to ensure that this is the case. The Subcommittee was not impressed by complaints that worthy research goes unsupported -- this is typically the case for government-supported research, and is what one expects for a healthy program (i. e., there are so many good ideas that not all, but only the most worthy, can be funded). Prioritization and integrated planning is the key.

Recommendations

1) Major facilities or initiatives being newly considered must be subjected to a formal process of critical review that includes: i) an explicit written proposal describing the rationale, and the expected costs and benefits of the intended effort; and ii) an independent and critical evaluation of the technical aspects, as well as other benefits for the SSP (e. g., in maintaining expertise). The priority importance of a major new facility or initiative must be demonstrated relative to other needs across the SSP (i. e., including but not limited to the S & T component).

2) The Laboratories, supported by NNSA, have responsibility for ensuring proper balance and integration in the S & T programs, including under conditions of contracting budgets. This includes a balance between numerical simulation and experimental validation, as well as between major facilities or initiatives and small-scale research. The NNSA should provide a broad context for this balance through its Strategic Plan, and the Laboratories should implement prioritization based on the joint mission statement described above [Recommendation 2 of Section II. 1].

III. 2. University Programs

We did not review programs supporting university–laboratory collaborations in detail because these have mainly existed at a small scale in the past. Several years ago, ASCI put in place university programs that are yielding new fundamental knowledge and methodologies, such as material properties and simulation technologies, and a cadre of people with expertise and interest in areas that are critical to the long-term success of SSP. Dozens of University–Laboratory collaborations have been sponsored by these programs, and some 46 participants in the ASCI university programs have become permanent Laboratory employees. A new program for supporting experimental collaborations between university and National-Laboratory researchers has just been initiated by DP,[14] and we believe that this has considerable potential for benefiting both NNSA and the academic research community.

Specifically, such a program can help support basic research that is relevant to, though not immediately required of, the SSP. Such research is an essential supporting activity for the Laboratory researchers uncovering "unknown unknowns" regarding the aging stockpile, thus offering an important contingency for ensuring the success of the ongoing stewardship endeavor. However, there is so much basic research that is ultimately relevant to the SSP, that there are simply not enough staff at the National Laboratories to make the required progress on their own. Researchers at universities can contribute significantly, however, and do so without engaging in sensitive (let alone classified) research in such diverse areas as high energy-density physics, metallurgy, the aging of organic and ceramic components, surface physics, hydrodynamics and high-pressure studies.

By participating in such collaborations, students and other researchers at universities become introduced to the range and quality of work performed in National Laboratories. This is important for recruitment of future expertise. By the same token, researchers at the National Laboratories can maintain an active and visible participation in their own area of expertise, which is significant for retention of top talent. The collaborations play a further role of ensuring that researchers within the National Laboratories remain at the state of the art; that is, that high quality is maintained.

We caution that the only way such benefits can be accrued is through a competitive awarding of support based on rigorous, independent peer-review, as was done for the ASCI and high-energy density programs supporting university research. To the degree that less-than-top-quality work is supported, not only is the product less than it could be but the contribution of such a program to recruitment and retention of top talent is severely undermined. In many regards, this conclusion parallels our comment regarding major facilities and initiatives. The DOE has longstanding experience in using peer-review to provide support for the best research in highly competitive areas, so there is every reason for NNSA to be able to follow suit.

Recommendations

University-Laboratory collaborations in both experimental and numerical simulation should be supported, with the selection based on a rigorous process of independent, critical peer review in order to maintain the highest standards of quality in research and the most positive impact on the SSP.

[14] Federal Register, Vol. 66 - No. 167 (Tuesday, August 28, 2001); a text version of the notice can be found at: http://frwebgate.access.gpo.gov/cgi-bin/getdoc.cgi?dbname=2001_register&docid=01-21666-filed

IV. SPECIAL TOPICS

IV. 1. Test Readiness

Test readiness refers to the U. S. commitment, under the current nuclear test-ban moratorium, to maintain the ability to resume underground (explosive) nuclear testing[15] within a period of 24-36 months, should that be deemed necessary. This commitment has been misrepresented in public discussion through claims that the U. S. would require *at least* 2-3 years in order to be able to test. In fact, the Subcommittee was clearly told that the Nation would be able to perform a test in as little as 3-6 months. It is worth noting that maintaining the ability to test is not prohibited by the proposed Comprehensive Test Ban Treaty (CTBT), but is actually among the U. S.' attached "Safeguards,"[16] and that were a decision made to resume testing in the future this would not in itself be an indication that the SSP has failed.[4]

Any enhancement of the current state of test-readiness must be responsive to political and technical factors. For example, a delay of (as much as) 2-3 years is long relative to the political cycle, such that a President might be faced with requesting a test that would then take place in a future Administration (or a new President might inherit a decision to resume testing from a previous Administration).[3] The technical issues revolve mainly around maintaining expertise, but also include concern about how well SSP could respond to a decision that testing is urgently needed in order to address a previously unforeseen problem that is discovered in the enduring stockpile.

The requirements for test readiness cannot be further evaluated without considering specific scenarios. Both costs (in funding as well as commitment of personnel) and benefits (e. g., in training) need to be worked out for each scenario. Therefore, a wide variety of scenarios must be explicitly considered in order to 1) identify the testing program that would be called for, including its scope and schedule; 2) identify the bottlenecks to resuming testing; and 3) identify the means and associated costs of avoiding (or at least minimizing) these bottlenecks.

The current program of subcritical experiments[18] does exercise many of the functions associated with underground nuclear testing at NTS. The design laboratories (LANL and LLNL) use these experiments to train new scientists in, and remind experienced researchers of, many aspects of carrying out underground nuclear tests. Our sense was that more emphasis could be placed on this type of training and, more generally, on carrying out a vigorous program of subcritical experiments: these efforts have yielded useful new data bearing on the weapon subsystems, as well as offering training in underground-test procedures.

The judgement expressed by the National Laboratories was that significantly enhancing test readiness at NTS beyond the subcritical experiments would not contribute meaningfully to training in S & T; we concur. That is, the most important remaining bottlenecks are not of a technical character, but involve logistics and the regulatory environment. In particular, work crews (and associated infrastructure, including security) would need to be engaged in order to prepare test rigs and specific sites. This is expensive and generally does not enhance maintaining (or training) special skills, but needs to be done in order to significantly reduce the time required to conduct a set of underground nuclear tests. Even so, it is unclear to what degree such work can be performed before one knows in some detail what tests are to be carried out.

The regulatory constraints are real, but are not readily addressed due to uncertainties in the nature and timing of possible future tests. Were NNSA to start forthwith addressing regulatory constraints on underground nuclear testing, it is unclear to what degree addressing current constraints can provide any assurance that future regulatory constraints would be diminished in any way.

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[15] "Underground nuclear testing" is used to denote nuclear-explosion tests, as distinct from the subcritical tests that are also performed underground but do not create self-sustaining nuclear reactions (and are permitted under the current moratorium as well as the proposed CTBT).

[16] http://usinfo.state.gov/topical/pol/arms/ctbt/factsafe.htm

Recommendations

A full analysis of the costs, benefits, scope and schedule, must be performed for both i) prospective underground nuclear tests and ii) the enhancement of current test readiness based on realistic and detailed scenarios. There must be a clear-cut process for deciding the technical conditions under which testing would be called for. The tradeoff between costs and benefits of proposed actions must be explicitly worked out: for example, political-timing is a sufficient reason to enhance test readiness, as long as Congress and the Administration support this political decision.

IV. 2. Advanced Concepts Designs

Maintaining the ability to design new nuclear weapons, from individual components to subsystems, is an integral part of the SSP. Pursuing design activities and correlated diagnostic tests is viewed as an important means of retaining nuclear weapons expertise, both within DP and NN programs, even if the newly designed nuclear subsystems could not be incorporated into the stockpile without nuclear explosion testing.

As far as exercising design capability and training new designers, such activities are fully in line with the SSP. Claims in some of the Laboratory presentations that constraints have been placed "from above" in recent years do not make sense, as the law and the defining components of the SSP both clearly support the development of new designs[17] (no action on the part of the NWC is needed). The degree to which such activities are viewed as high priority would then be reflected by the relevant budgets.

What were presented as "advanced concepts" to the Subcommittee did not involve any radical departures from previously considered (or even implemented) systems. Whether design efforts are devoted to "advanced concepts" or to lesser problems, design exercises should serve to retain nuclear expertise and train new designers, as well as focusing on relevance to potential military requirements. Concepts that have been discussed quite forcefully in recent times have yet to be examined in sufficient technical depth to determine that their potential military benefits justify the costs involved.

Recommendations

The Subcommittee supports ongoing design activities as one of the appropriate means of retaining nuclear weapons expertise, including training new designers. Until a specific need is identified, and found to justify the budgets involved, such work can only be viewed as advanced exercises -- albeit important for training. In particular, we recommend that NN becomes more engaged in these activities.

As with major new facilities, any new design concept should be thoroughly vetted by a critical and independent review that takes into account technical and other (e. g., military, training, etc.) considerations. Costs, schedules and scope must be established and accepted, with the DoD providing the required backing.

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[17] There exists a congressional prohibition on development of new designs with yields below 5 kt [National Defense Authorization Act For Fiscal Year 1994, Sec. 3136, "Prohibition on Research and Development of Low-yield Nuclear Weapons"].

APPENDIX A

Charge

The following extract from the NNSA AC's charge is the portion relevant to the DP Subcommittee:

> I charge the NNSA-AC to review the current NNSA research and development portfolio and make strategic recommendations for strengthening NNSA leadership in science and technology. Initially, this study should focus on the experimental programs in Stockpile Stewardship... with particular attention on:
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> - *Stockpile Certification* – NNSA/DP programs for certifying systems and qualifying components as part of the stockpile life extension and refurbishment programs (including W88 pit certification) and long-term S&T efforts needed for annual assessments.
> - *Nuclear Weapons Expertise* – NNSA/DP programs for recruiting, retaining and certifying the next-generation stewards through advanced concepts research and test readiness.
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> This study should address the following questions:
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> 1. Are the programs appropriately integrating requirements-driven, needs-driven and basic research?
> 2. What unique role do these programs play in supporting U.S. national security policy?
> 3. What unique role do these programs play in supporting leadership for science and technology for the nation?
> 4. Do these programs have the appropriate strategy and tools (including large-scale and laboratory facilities, diagnostic and analysis capabilities, and human resources) to address both near-term mission requirements and long-term science and technology leadership?
> 5. Is the philosophy and approach for developing university and industrial partnerships sufficient to meet both near-term and long-term mission needs?
> 6. Are these programs utilizing and optimizing interlaboratory partnerships (e.g., joint programs and programmatic peer review)?

APPENDIX B

Responses to Questions

Although the questions posed in the Charge to the NNSA AC are addressed throughout the present Report in general terms, a summary of specific responses bearing on DP programs is offered below for the sake of clarification. The text of the Report provides further elaboration. In line with the Charge, "programs" is taken to refer to those making up the NNSA's science and technology portfolio, with emphasis on the experimental programs in Stockpile Stewardship.

1. It is impossible to determine whether the programs are appropriately integrating requirements-driven, needs-driven and basic research because of the lack of a clearly formulated strategy for the NNSA. The NNSA needs to establish an overall Strategic Plan that clearly describes its mission and goals, with more specific documents fleshing out the implementation. For example, the report recommends that the National Laboratories' mission in science, technology and engineering be clearly spelled out, along with an implementation plan.

Many of the specific programs do appear to the DP Subcommittee to be appropriately integrating the three types of research (the group producing detonators at LANL provides but one of many examples). However, other aspects of the S & T portfolio are -- or at least appear to be -- far out of balance, suggesting the need for aggressive attention to the balance between short- and long-term priorities versus desiderata.

2. The NNSA has not yet offered a clear vision of how the programs support U. S. national security policy overall, let alone in a unique way, the result being a less-than-coherent contribution in this regard. There is much opportunity for describing the unique and far-reaching contributions to national security of NNSA's S & T portfolio: experimental and other programs of stockpile stewardship do contribute significantly, for instance. However, the NNSA's lack of an over-arching vision, strategic plan and implementation procedure leads to significant potential imbalances. As one example, the Report notes the inadequacy of processes for vetting proposals for major facilities, and questions the basis on which Life Extension Program activities have been formulated.

3. The lack of a Strategic Plan describing the NNSA's vision and expected implementation again hampers answering this question. It is important for the NNSA to identify what it is *not* doing in supporting leadership in S & T, as well as what it *is* doing for national security via the S & T programs. For example, the NNSA must avoid duplicating or attempting to compete with programs dedicated to pure S & T, such as the research of universities and pure-research institutions, because this detracts from the agency's national security mission and does not provide value added to the Nation. In contrast, the translation of basic and applied research into national-security applications, and the support of S & T appropriate to this task, do represent nationally important activities for which the NNSA is uniquely suited. A case can be made that NNSA's strong support of S & T is in itself a contribution to national security (i. e., a kind of "intellectual deterrent"), but that case has not yet been made.

4. The people carrying out the stewardship activities have a variety of tools that are appropriate to address both the near- and long-term S & T needs of NNSA. An appropriate strategy does not exist, at present, resulting in insufficient prioritization among programs, and inadequate communication of what these programs can or should contribute to national security.

5. The philosophy for developing university partnerships appears sufficient for meeting currently perceived near- and long-term missions needs. The approach must be based on critical and independent peer review. Partnerships are being initiated, or at least expanded, at the present, so should be reviewed for their effectiveness as they become better established. Partnerships with industry are more variable and, in many respects, more problematic (e.g., because of NNSA's highly specialized needs); these were not reviewed in detail.

6. These programs are utilizing interlaboratory partnerships to varying degrees but, in many important cases, not effectively. As described in the Report, the peer-review process is highly uneven, overall, being labeled as "dysfunctional" even by its participants in a variety of key areas. There is evidence of progress and, more significantly, a desire for improvement, but much work is still needed in this domain.