Science Based Stockpile Stewardship

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The FY1994 National Defense Authorization Act calls on the Secretary of Energy to "establish a stewardship program to ensure the preservation of the core intellectual and technical competencies of the United States in nuclear weapons". The DOE asked JASON to review its Science Based Stockpile Stewardship program with respect to three criteria: 1) contributions to important scientific and technical understanding and to national goals; 2) contributions to maintaining and renewing the technical skill base and overall level of scientific competence in the defense program and the weapons labs, and to the broader U.S. scientific and engineering strength; and 3) contributions to maintaining U.S. confidence in our nuclear stockpile without nuclear testing through improved understanding of weapons physics and diagnostics. In this report JASON analyzes the DOE program and makes specific recommendations regarding it.

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Contents

1 EXECUTIVE SUMMARY: CONCLUSIONS AND RECOMMENDATIONS

The FY 1994 National Defense Authorization Act (P.L. 103-160) calls on the Secretary of Energy to "establish a stewardship program to ensure the preservation of the core intellectual and technical competencies of the United States in nuclear weapons." In response, DOE has presented a National Security Strategic Plan for stewardship of U.S. nuclear weapons in the absence of nuclear weapons testing.

The basic principle of this plan is to compensate for the termination of the underground testing program by improved diagnostics and computational resources that will strengthen the science-based understanding of the behavior of nuclear weapons, thereby making it possible for the United States to maintain confidence in the performance and safety of our nuclear weapons during a test ban, in a manner consistent with our objectives of non-proliferation and stockpile reduction.

DOE's plan (called SBSS—Science Based Stockpile Stewardship) recognizes the need for improved understanding and better modeling of the reduced numbers of warheads and fewer warhead types that are expected to remain in the stockpile for at least several decades. In the absence of nuclear weapons testing, improved understanding of the warheads and their behavior over time will be derived from computer simulations and analyses benchmarked against past data and new, more comprehensive diagnostic in-

formation obtained from carefully designed laboratory experiments. Toward this goal the SBSS calls for the construction of a number of new experimental facilities which have applications both in basic scientific research and in research directed towards strengthening the underlying scientific understanding in the weapons program. These include, initially, DARHT (Dual-Axis Radiographic Hydro Test), for advanced diagnostics of the primary implosion up to pre-boost criticality; NIF (National Ignition Facility), for advancing inertial confinement fusion (ICF) to achieve ignition, and for the study of high energy density physics and the behavior of secondaries; a new pulsed power facility, ATLAS, to provide large cavities for hydro studies under conditions of the late stages of primary and early stages of secondary implosion, and of possible flaws and degradations of weapons on a macroscopic scale size; and the continuation of support for LANSCE (Los Alamos Neutron Scattering Center) for neutron radiography of weapons and for material science. There will inevitably and necessarily be major advances in computational ability to go with these instruments to perform experiments of general scientific interest. The purpose of all this is threefold: to enhance our ability to understand weapons physics, to perform experiments of general scientific interest, and to attract numbers of high-quality scientists and engineers to the general areas of science relevant to the weapons program.

We have analyzed DOE's SBSS program and have arrived at a set of conclusions and recommendations regarding it. These are as follows:

1.1 General Conclusions

1. A strong SBSS program, such as we recommend in this report, is an essential component for the U.S. to maintain confidence in the performance of a safe and reliable nuclear deterrent under a comprehensive test ban.

   The technical skill base it will help maintain and renew in the defense program and weapons labs will also be important for assessing emerging threats from proliferant nations and developing possible technical responses thereto.

2. Such an SBSS program can be consistent with the broad non-proliferation goals of the United States. This requires managing it with restraint and openness, including international scientific collaboration and cooperation where appropriate, so that the program will not be perceived as an attempt by the U.S. to advance our own nuclear weapons with new designs for new missions.

1.2 Specific Conclusions and Recommendations

Hydrotests and DARHT

Hydrotests are the closest non-nuclear simulation of the operation of the primary up to pre-boost criticality. They can address issues of safety and

aging, and provide benchmarks for code calibration and a better science-based understanding of the operation of the primary.

Dynamic radiography with core punching is important for the study of properties of the pit at the late stages of the implosion. The Dual-Axis Radiographic Hydro Test (DARHT) facility currently under construction at LANL, and the active γ-ray camera recently developed as a replacement for film, together will provide greatly enhanced capabilities of importance in the absence of underground tests.

Assuming successful completion and operation of DARHT up to design specs, we recommend building a second arm at a relative angle of approximately 90° that would provide important information about the time development as well as the 3-dimensional structure of the implosion. The total estimated construction cost for the additional arm, including contingency, is roughly \$37 M.

Further simulations and analysis, and experience with DARHT, are needed before one can judge the cost/benefit of further improvements in hydrotest capabilities, such as envisioned for a future Advanced Hydro Test Facility (AHTF) at a construction cost of \$400 M that would provide up to six temporal images and six spatial views per shot.

The scientific work in hydrotests is largely classified and will properly remain so, as it involves detailed information of primary design and codes that could be of considerable value to would-be proliferants. The very limited added value of hydronuclear tests that provide for a brief glimpse into the very

early stages of criticality have to be weighed against costs, and against the impact of continuing an underground testing program at the Nevada Test Site on U.S. non-proliferation goals. On balance we oppose hydronuclear testing[1].

The NIF

The NIF is without question the most scientifically valuable of the programs proposed for the SBSS, particularly in regard to ICF research and a "proof-of-principle" for ignition, but also more generally for fundamental science. As such, it will promote the goal of sustaining a high-quality group of scientists with expertise related to the nuclear weapons program. Experiments relevant to the weapons program, particularly as regards the physics of the secondary, can also be done at the NIF at hohlraum temperatures high enough (600 eV) to enable opacity and equation of state measurements to be performed under conditions close to those in the secondary. Both the scientific and the weapons experiments on the NIF will require the development of improved computational capabilities. This will improve the understanding

[1]The arguments leading to this conclusion are developed more fully in a separate part of this JASON study under the leadership of Dr. Doug Eardley. They are based on the assumption that the U.S. will continue to advance our broad, if still quantitatively incomplete, understanding of implosions of the primary stage of a weapon up to pre-boost criticality. These advances in understanding will come from improvements in the weapons codes and diagnostics of above-ground hydrotests that we are recommending in this report for the SBSS program. Together with the other components of SBSS identified here, they should provide for adequate safety and reliability of the stockpile for the foreseeable future. Although we see no need for hydronuclear testing in the near term, the consequences of going as long as 10 years without underground tests are difficult to fully anticipate. Depending on what we learn from the proposed SBSS program, together with future strategic and political developments in the post-Cold War world, the U.S. may find it necessary to review its obligation under a CTBT under a "supreme interest" clause. Should that circumstance arise, it will most likely call for consideration of much higher yield nuclear testing than at the 2-4 lb. level of TNT equivalent yield now being considered for "zero-yield" nuclear tests.

that we need for stewardship.

The NIF technology is very different from that of a nuclear weapon and does not add a significant risk of proliferation or undermining the NPT. To the contrary, the open collaborations with outside groups of scientists on the scientific programs at the NIF, which we anticipate will be a major use for the facility, should help dispel concern that the NIF is being used to support advanced weapons development efforts. The limited shot rates, small tritium inventory, and low level of radioactivity produced are comparable to those in TFTR presently operating routinely on the Forrestal Campus of Princeton University and present a negligible environmental hazard. We wholeheartedly endorse a timely, positive KD1 for NIF at this time.

LANSCE

The LANSCE facility at Los Alamos is in operation. It provides a valuable vehicle for a large number of scientific experiments in material science research, including inelastic neutron scattering, experiments requiring a large dynamic range of time and wavelength scales, and can be used together with intrinsic short time experiments, such as strong pulsed magnetic fields. For weapons stewardship, LANSCE, through neutron radiography, which can "see" the low-Z elements better than x-rays, can address materials issues underlying high explosive burn and aging, shocks, equations of state, and can also measure cross-sections, among other things.

We recommend continuing near-term support for LANSCE during which an evaluation can be performed of whether neutron radiography, at LANSCE,

or on future smaller facilities, is important for stewardship. LANSCE should also seek to build a strong, high-quality science effort with broad collaboration involving LANL and outside material scientists. Experience with this accelerator complex will also support investigations into other applications of potential interest, like accelerator production of tritium. Longer term support would be based on the progress made toward successfully achieving these near-term goals.

STOCKPILE SURVEILLANCE

A statistically significant fraction of the weapons now being disassembled under the START treaty should be carefully analyzed under an enhanced stockpile surveillance program for cracks, component failure, or other signs of deterioration. One option to be examined is whether the LANSCE facility could play a valuable role in such examinations. Another is the SNL program of micro-sensors embedded in situ for weapons diagnostics.

PULSED POWER

Electrical pulsed power devices reach only to lower temperatures (100-200 eV) than NOVA (250 eV) and as designed for NIF (up to 600 eV), but they have the advantage of providing larger plasma volumes. Up to now, these facilities have primarily been used in the weapons program in the study of nuclear weapons effects. There are, however, many possible scientific uses of those instruments as well, and we recommend that these be evaluated with the collaboration of the relevant scientific community, leading to a stronger, more diverse, and open research program of collaboration in

science experiments carried out jointly with the outside–including foreign–science community.

As to instruments, there is an important mission for the proposed new ATLAS facility which will be unique for doing large scale hydro experiments at high enough temperatures to ionize the material. This is important for understanding and diagnosing late stages of primary and early stages of secondary implosion. It presents a large benefit/cost ratio at a cost of about $43 M and a two year construction period with a 1998 completion. ATLAS will provide a large hohlraum volume of about a cm^3 for modelling and studying the effects on implosion of aging and corrosion that may occur in the stockpile, including high aspect ratio cracks. A positive, timely KD1 seems appropriate; our only hesitancy results from our own limited knowledge of the possibility of modifying existing short pulse (< 300 nsec) facilities to replicate, in part, ATLAS parameters. Any decision on a new JUPITER facility, which is still in the concept development phase and whose importance in the SBSS, relative to ATLAS, NIF, and other facilities, and overall to science, remains to be established, should be deferred for future consideration.

SNM AND PROCESSING

The key SNM manufacturing expertise that the U.S. needs to maintain in its stewardship program is the ability to cast, machine, and finish metallic uranium and plutonium, particularly HEU and WGPu. The technology of cladding and coating these materials in nuclear weapons must also be preserved. The U.S. must also be prepared to replenish our tritium supply if called for.

ADVANCED COMPUTING

In the absence of nuclear tests, and with the advent of above-ground experimental programs such as NIF, the need for theoretical understanding and numerical simulation of weapons-related physics will increase rather than diminish. Advanced computing should be seen as part of the theory program and should be designed appropriately. In particular, computer resources should be acquired and distributed in such a way as to attract the best theoretical minds to the program, and not merely with a view towards the most rapid execution of nuclear-weapons codes.

Trends in the computer market suggest that much of the computing for SBSS will be done on fast networks of high-end workstations rather than supercomputers. Fortunately, workstation performance is increasing exponentially. A conscious effort should be made by the labs to adapt weapons-related codes, which were written for vector supercomputers, to workstation networks. Efforts should also be made to maximize the communications bandwidth of such networks and to devise algorithms that run efficiently on them.

The Labs should determine whether more powerful, advanced supercomputers, or the less-expensive workstations of the near future, offer a more flexible, efficient, and affordable path to achieving the improved scientific understanding on which the Science Based Stockpile Stewardship program relies. If it turns out that advanced supercomputers are required, the Labs should plan to encourage the supercomputer market and should coordinate with other users having similar needs.

Concerning the software we recommend:

1. The SBSS program should prioritize which of its existing codes would benefit the most from being upgraded, and should develop a long-range plan for how to evolve its extensive existing software base toward the computer environment of the future. This should include plans for how to more fully document the contents and the functioning of the most important existing computer codes, so that future generations will be able to use them intelligently.

2. New and actively used computer codes should be written in a scalable manner, so that they can evolve gracefully to new computer architectures.

3. With the trend towards use of three dimensional computations in the future, advanced tools for visualization will become even more essential to understanding of the results of nuclear weapons-related computations. The SBSS program will need to become a leader in this rapdily developing area.

4. A national archive of information from all the past nuclear tests should be created to preserve the historical record of accumulated wisdom as the practitioners of nuclear weapons design and engineering begin to retire. Before embarking on a large and expensive software effort, DOE should call on external experts on archiving for advice and setting priorities.

2 ASSUMPTIONS UNDERLYING STEWARDSHIP

The FY1994 National Defense Authorization Act (P.L. 103-160) calls on the Secretary of Energy to "establish a stewardship program to ensure the preservation of the core intellectual and technical competencies of the United States in nuclear weapons." In addition, when announcing the U.S. moratorium on nuclear testing on July 3, 1993, President Clinton said "to assure that our nuclear deterrent remains unquestioned under a test ban, we will explore other means of maintaining our confidence in the safety, the reliability and the performance of our own weapons. We will also refocus much of the talent and resources of our nation's nuclear labs on new technologies to curb the spread of nuclear weapons and verify arms control treaties."

In response, the DOE has presented a National Security Strategic Plan for stewardship of U.S. nuclear weapons in the absence of nuclear weapons testing. The priority objective of this plan is to "assure confidence that the stockpile is safe, secure, reliable, and flexible without underground testing. Our analysis of the DOE's Science-Based Stockpile Stewardship (SBSS) program is based on this stated objective together with the following four assumptions:

(1) For the near future, perhaps over a decade, the U.S. stockpile will decrease in numbers and variety of warheads, with the remaining weapons of basically the same design as in today's stockpile. Current unilateral

U.S. policy (President Bush, 1992) prevents the development or deployment of new nuclear designs and it is likely that renewal of the Non-Proliferation Treaty (NPT) in 1995 will result in an implicit bargain by the nuclear powers to continue such restraint.

(2) Potential changes in nuclear policy over the longer term may include continued reductions in U.S. reliance on nuclear weapons and changes in delivery systems. Furthermore, new concerns may arise as to the long term aging of nuclear weapons and the need to certify their performance without nuclear test data. A possible response to this circumstance might be to reintroduce into the stockpile already tested warheads that are robust in design and known to be reliable, but which are assembled with modern engineering and manufacturing practices. These would be less sophisticated designs, no longer restrained by Cold War requirements for maximum yield-to-weight ratio.

(3) In the event of further proliferation of nuclear weapons by other nations it is vital for us to retain in our nuclear program people with the skills necessary to predict and evaluate the likely characteristics and designs being used by the proliferator, and to develop possible technical responses to threats that may be posed.

(4) The US nuclear infrastructure under the SBSS will retain a capability to design and build new weapons, which could be deployed should the need arise and lead to the resumption of testing; and to continue to disassemble stockpile warheads safely and to manage the secure storage and disposition of special nuclear materials (SNM) in accord with progress in arms reduction agreements. We note here that the ongoing

warhead disassembly process presents very valuable opportunities to learn of possible aging effects such as warhead corrosion or structural defects. A strong stockpile surveillance program should also be a key part of the SBSS.

Adequate stewardship, under these assumptions, requires the U.S. to retain, or develop, as necessary, the means and expertise to understand and deal with all aspects of nuclear weapons.

3 CRITERIA FOR EVALUATING THE COMPONENTS OF THE SBSS PROGRAM

The proposed components of the SBSS program should be evaluated and prioritized against the following three criteria.

(a) Their contribution to important scientific and technical understanding, including in particular as related to national goals.

(b) Their contribution to maintaining and renewing the technical skill base and overall level of scientific competence in the U.S. defense program and the weapons labs, and to the nation's broader scientific and engineering strength.

(c) Their contribution to maintaining U.S. confidence in the safety and reliability of our nuclear stockpile without nuclear testing through improved understanding of weapons physics and diagnostics.

The order in which these three criteria are listed does not reflect a judgment as to their relative importance. All three are important Individual elements of an SBSS program will contribute with different weights, but the overall program should be developed to fulfill all three criteria. Of course, all the elements of the SBSS program should be consistent with our nonproliferation objectives, and should not constitute environmental hazards. We believe this to be the case for all our recommendations.

4 NON-PROLIFERATION AND THE SBSS

An additional important criterion by which to evaluate the SBSS is connected to the the Non Proliferation regime to which the United States is committed. This implies that the role of nuclear weapons in U.S. policy must be limited and, over time, reduced. Compliance with this objective will support U.S. efforts to secure an indefinite extension of the NPT at the 1995 Review Conference. Therefore the SBSS program implementation must avoid the appearance that, while the U.S. is giving up nuclear testing, it is as compensation introducing so many improvements in instruments and calculational ability that the net effect will be an enhancement of our advanced weapons design capabilities.

This calls for care in designing an appropriate SBSS program that meets two very different, and at times countervailing, objectives. The first, as mandated by the FY94 Defense Appropriations Act and endorsed by President Clinton, is to maintain a strong U.S. nuclear deterrent in the absence of underground nuclear weapons tests. This calls for maintaining high competence in weapons physics and engineering; enhancing the weapons science and engineering programs that underpin our ability for advanced diagnostics, related computations, and ultimately scientific understanding of all aspects of their behavior, aging, security, and safety; and maintaining high competence in the weapons-related disciplines at the weapons laboratories. The second objective, counterposed to the first, is the importance of implementing the SBSS program to support broad non-proliferation objectives, including securing

indefinite extension of the nuclear non-proliferation treaty at the 1995 Conference. The United States, as the world's preeminent conventional military power, has the strongest security motivation to prevent nuclear proliferation, with its "equalizing" aspects.

The non-proliferation regime as codified by the NPT in essence constitutes a three-way bargain which can be paraphrased as follows:

- Nuclear Weapons States (NWS) agree not to transfer nuclear weapons design information, nuclear weapons components and weapons-grade fissionable material to the Non-Nuclear Weapons States (NNWS) and those states agree not to receive them;
- The NWS shall cooperate with the NNWS in transferring science and technology relating to peaceful uses of nuclear energy; in exchange the non-nuclear weapons states will execute their nuclear power activities under full scope safeguards administered by the International Atomic Energy Agency;
- The NWS will reduce their nuclear weapons stockpiles and will reduce, over time, the reliance of their national security policy on nuclear weapons, thereby decreasing the discriminatory nature of the non-proliferation regime.

No technical measure in itself can stem proliferation of nuclear weapons. General design principles of unsophisticated nuclear weapons are well known, as are the principal physical data underlying nuclear weapon materials. Effective barriers to the acquisition of HEU and plutonium can prevent acquisition

of nuclear weapons until such time as a potential proliferator can develop indigenous processes to produce these materials. Ultimately non-proliferation can only be successful if the NNWS are persuaded that their national security is better served without nuclear weapons than by possessing them.

These non-proliferation principles provide the framework which must govern the stewardship program. The weapons physics and diagnostics program should consist of a core activity which maintains confidence in the present stockpile for the foreseeable future to standards not substantially different from those maintained when underground nuclear tests were permitted. In addition, weapons physics, diagnostics and computation can allow for possible changes for the future—including possible adaptation of old more robust designs. While the potential for future developments cannot be excluded, the SBSS activities should not be interpretable as laying the basis for the development of newer generations of nuclear weapons of advanced performance for new missions.

One worrisome aspect of the SBSS program is that it may be perceived by other nations as part of an attempt by the U.S. to continue the development of ever more sophisticated nuclear weapons. This perception is particularly likely to be held by countries that are not very advanced technologically since they are less able to appreciate the limits on advanced weapons design that a lack of testing enforces. Hence it is important that the SBSS program *be managed with restraint and openness*, including international collaboration and cooperation where appropriate, so as not to end up as an obstacle to the Non-Proliferation Treaty.

On the other hand there are two important reasons that support a comprehensive SBSS. The first is that stewardship is an essential responsibility of the declared nuclear weapons states, in that they must guarantee the safety of the weapons and provide security against possible theft or other misuse of them. Second, presumably all underground nuclear tests will be stopped by an eventual CTBT. A CTBT has been designated as a goal in the negotiating history of the NPT and is believed to be necessary to gain support from the NNWS for the U.S. position seeking indefinite extension of the NPT. The conclusion we draw from this is that the declared nuclear weapons states can accept a ban on underground nuclear tests only if they maintain a technical base of both experiments and theoretical analysis in order to discover flaws in the weapons as they age, to analyze the consequences of these flaws, and to correct them. Secondly, we are led to the conclusion that, with a CTBT in place, new facilities must be built to strengthen the science base of our understanding of nuclear weapons in order to at least partially replace the knowledge once obtained from tests.

While important, this argument may not be enough to entirely dispel suspicions on the part of the non-nuclear weapons states. What would go a long way to relieve these suspicious would be to declassify as much of the stewardship program as possible. Following recent declassification actions, a large part of the ICF program and the precursor (NOVA) to instruments such as the NIF are already unclassified. The LANSCE facility is also already completely unclassified. Parts of the pulsed power program at Sandia remain classified but many parameters including hohlraum temperatures are unclassified.

There should be a detailed study, taking into account what is already available outside the weapons program, to further reduce the need for classification, both of experimental results and theoretical calculations. Any restraint on making weapons codes available should be justified on clear grounds of preventing proliferation. We should continue to build on existing precedents for experimental and theoretical cooperation and collaboration, at all three national weapons laboratories, including with Russian scientists at their facilities. Only critical parts of the weapons codes that would be used to analyze some of the experimental data or which would be directly applicable for weapons design would remain classified. These codes represent many person-years of highly sophisticated effort. To develop even fairly crude 2D hydro and radiation codes would be a formidable task for would-be proliferators[2]. Altogether, the more open the stewardship program is, the more easily suspicions regarding U.S. intent to use the program as a cover for new weapons development can be overcome.

This issue of suspicions regarding U.S. intent also enters into the decision as to whether to perform so-called"zero-yield" hydronuclear tests as opposed to limiting the testing program to above-ground hydrotests alone. "Zero-yield" hydronuclear tests include just enough SNM to produce a fission yield much less than their high-explosive yield. In recent discussions, fission yields of perhaps two to four pounds of TNT equivalent are frequently referred to. A number of such tests with fission yields under one pound were conducted in shallow underground facilities at Los Alamos during the 1958-61 U.S. Soviet moratorium on nuclear testing. On the basis of techni-

[2]Yet we must remember that the first U.S. nuclear weapons were designed with computing power similar to that contained in today's hand-held calculators.

cal considerations alone, such hydronuclear tests with very low fission yields could be designed and conducted safely in above-ground containment vessels. However, performing such tests above ground would most likely be unacceptable on political grounds in the United States, even if they were to meet the requirements of formal Environmental Impact Statements and Safety Analyses.

With current sensitivities to nuclear dangers, U.S. hydronuclear tests, even though limited to no more than a 4 lb. TNT-equivalent nuclear yield, would likely be restricted to the Nevada Test Site and carried out underground. A restriction to above ground experiments would limit the SBSS program to hydrotesting for advanced diagnostic analyses and benchmarking of more powerful computer codes of the primary implosion. Such a restriction, together with relegating the Nevada Test Site to a stand-by readiness status, would add assurance to the international community that the United State's SBSS was not serving as a cover for advancing U.S. nuclear weapons technology. Since hydronuclear tests would be potentially more valuable to proliferants seeking to check computer predictions for more advanced designs using less fissile materials and with smaller weights and volumes that could be more readily delivered, it would be in our national interests to forego them[3].

[3]As to long term prospects for a restriction to pure hydro testing see footnote (1) on page 5.

5 PROGRAM ELEMENTS OF A SBSS

The FY1994 National Defense Authorization Act spells out (in Sec 3138) the following individual program elements for inclusion in the stockpile stewardship program that it establishes:

(1) An increased level of effort for advanced computational capabilities to enhance the simulation and modeling capabilities of the United States with respect to the detonation of nuclear weapons.

(2) An increased level of effort for above-ground (i.e., not involving nuclear weapons test explosions, which are conducted underground) experimental programs, such as hydrotesting, high-energy lasers, inertial confinement fusion, plasma physics, and materials research.

(3) Support for new facilities construction projects that contribute to the experimental capabilities of the United States, such as an advanced hydrodynamics facility, the National Ignition Facility, and other facilities for above-ground experiments to assess nuclear weapons effects.

An important requirement of the U.S. stockpile stewardship program in the absence of nuclear testing is to provide a more comprehensive scientific base of understanding of nuclear weapons. With the benefit of such understanding, weapons scientists and engineers will have a more solid basis for anticipating, looking for and finding, and solving as necessary, new problems or remedying defects that may arise as the remaining stockpile continues to

age. In the past there were a limited number of cases where tests were needed to validate the "fixes" made to remedy defects or problems that appeared in warhead design or manufacturing processes. Now under a test ban, we will have to rely even more on analysis, improved diagnostics, and enhanced computational capabilities as replacements for testing, and their power must grow to meet the challenge of compensating in essential ways for the loss of underground tests.

Furthermore, with improved analysis and modelling of weapon performance, we will be better able to know to what extent, if any, the proposed "fixes" may require materials, manufacturing, or design changes.

We will, of course, also need to maintain and continually renew a cadre of top caliber scientists and engineers[4] who understand the science and technology on which the sophisticated designs in the current U.S. stockpile are based.

The individual program elements that we will analyze against the three criteria listed in Section II are:

(A) Hydrotesting: the Dual-Axis Radiographic Hydrotest (DARHT) facility and the proposed Advanced Hydro Test Facility (AHTF)

(B) The National Ignition Facility (NIF) as part of the Inertial Confinement Fusion (ICF) program.

[4]This is a small and decreasing as well as aging community. In particular, currently there are 14 designers of weapons primaries and 15 of secondaries at Livermore (compared with 23 and 27, respectively, five years ago) and 12 and 14, respectively, currently at Los Alamos.

(C) The Los Alamos Neutron Scattering Center (LANSCE) and Stockpile Surveillance

(D) Pulsed Power for Weapons Diagnostics and Effects and the proposed ATLAS and JUPITER facilities.

(E) Special Nuclear Materials and Processing

(F) Advanced Computing for Stewardship

Another major laboratory activity that supports stockpile stewardship both directly and indirectly is the collection of activities involving Nonproliferation, Intelligence and Arms Control (NIAC). Some of this, such as exploring the design space occupied by unboosted all-oralloy or all-plutonium systems—a potential design-of-choice by proliferators—is very largely based on the same underlying sciences as is nuclear weapons research and development, and is done either by former weapons design scientists who have transferred to the laboratory divisions involved or by current designers supported by NIAC funds transferred to the divisions in which they are housed. The groups now doing this work are likely to be the only ones at either laboratory who will continue to study new weapons designs in order to understand both what is happening elsewhere and as part of the study of how to counter such weapons in the hands of others. In addition, the nature of the NIAC work means that the members of these groups are often the best informed people at the laboratories in such other areas as special materials production, manned and unmanned sensors, biological and chemical warfare. Collectively, these activities support the continuation of cadres of scientists

knowledgeable in weapons design and fabrication in the same way that the other elements of the SBSS program are supposed to do. And, given that nuclear weapons in the hands of others is becoming our most important nuclear problem, such activities are of great importance in themselves.

6 HYDROTESTING AND SCIENCE-BASED STEWARSHIP

The primary is one of the most crucial, but complex, parts of every weapon in the stockpile. Its properties are central to safety, but they are also important to reliability and performance: if the primary doesn't work, nothing nuclear happens, and if its yield is too low, the secondary won't perform as expected.

Hydrotesting addresses the behavior of the primary and so is central to proper stewardship. A hydrotest is the closest non-nuclear simulation of primary operation, as the properties of a non-fissile pit can be studied up to the point where a real weapon would become critical. Properly designed hydrotests can address issues of safety and aging, as well as provide benchmarks for code calibration, including the development of instabilities and turbulence in the high explosives. Such information will lead to greater confidence in our understanding of weapons and, perhaps ultimately, to a willingness to make relatively simple changes in primary design without underground tests. However, since hydrotesting can only probe non-fissile systems, there are important nuclear aspects that cannot be studied by hydrotests (e.g., Pu behavior at high temperatures and pressures, boost, mix, ...).

Several techniques are available to study the non-nuclear implosion of a primary. Pin shots (thin conducting needles through which an induced current flow measures implosion velocities), and optical diagnostics (cameras

and interferometers) are sufficient only during the initial phases of the implosion. The properties of the pit at the late stages can be addressed only through dynamic radiography, and in particular core punching. It is this latter class of measurements that is the most difficult and requires the largest facilities.

The idea of core punching with dynamic radiography is quite simple. The source is an accelerator producing a precisely timed burst of high-energy (10–30 MeV) electrons that, in turn, impinge on a high-Z target to yield a burst of gamma rays through bremsstrahlung. These photons (a broad spectrum with a mean energies of several MeV) penetrate the imploded pit from one side and are detected on the other to produce an image. Among the several technical issues are the size of the electron spot (which is a major factor in the spatial resolution achieved), the contrast in the image (limited by the difficulty in penetrating some 100 gm/cm^2 of heavy metal), the efficiency with which the transmitted γ-rays are detected, and the adequacy of the single-time/single-view capabilities of existing facilities.

Today's most capable dynamic radiography facilities are FXR (LLNL) and PHERMEX (LANL). In response to the acknowledged need for an increased radiography capability with greater penetrating power and sensitivity, the DOE is constructing the DARHT (Dual-Axis Radiographic HydroTest) facility at LANL. This will be two electron accelerators at right angles, each with a design intensity comparable to FXR and a spot size of roughly[5] 2-3 times smaller than currently available. It will allow two views

[5]The gaussian half-width is 0.75 mm. For a uniform spot size, the MTF falls to $\frac{1}{2}$ value at a radius of 1.2 mm.

of an imploded pit at two different times. One axis of DARHT is being constructed (at a cost of about $80 M and expected to be on-line in 1997), with approval of the second axis (additional total cost, including contingency, of roughly $37 M with close to 3 years to complete) pending successful operation of the first. The properties of DARHT and other radiography facilities are summarized in the attached Tables 6.1 and 6.2 (from LLNL and LANL).

The design community has properly judged that improved hydrotesting capabilities are important in the absence of underground tests. The ultimate goal would be a tomographic movie of the late stages of the imploding pit. Achieving this goal requires improvements in both accelerators and detectors. A first step in the latter process is the gamma-ray camera developed by LLNL for producing a radiographic image. In this device, light from multiple scintillator elements is transmitted through a fiber optic reducer to a microchannel plate for intensification and recording on film. Successful operation has already been demonstrated, and a dual imaging capability is being planned by replacing the film with a CCD (active gamma-ray camera). Relative to existing film techniques, the gamma ray camera has a much improved sensitivity, leading to superior spatial resolution.

A proposed $5M upgrade to the FXR accelerator will allow double pulsing (and hence, when coupled with the active gamma-ray camera, dual images separated by several microseconds) in 1997. This advance will be at the expense of a decrease by a factor of 7 in dose, which is anticipated to be more than compensated for by the higher sensitivity of the gamma ray camera. LANL also expects to double-pulse PHERMEX in its FY95 operations with

Table 6.1 Radiographic Machine Comparisons.

	FXR	FXRU	FXR Double Pulse	Phermex	Phermex Double Pulse	DARHT	AHTF
Electron energy (MeV)	17	19	9.5	30	30/26	16	20
Maximum useable dose (Rads)	285	400	55	200	31/22	350	650
Spot size (mm)	2.1	1.5	1.5	1.8	1.7/2.3	.75	.7
Spatial views available	1	1	1	1	1	2	4-6
Temporal views available	1	1	2	1	2	2	4-6
Alignment	passive	passive	active	passive	passive	passive	active
Image recording	film	film/γ-ray camera	film/active γ-ray camera	film	film/active γ-ray camera	film/active γ-ray camera	film/active γ-ray camera
Pulse width (ns)	65	65	80	200	45/45	60	adjustable

Table 6.2 The FXR Double Pulse Upgrade Definition.

- Replace the current velvet cathode injector with a thermionic cathode that can be multiply pulsed with 1-5μs spacing
- Install a second triggering system and configure so that only half of the FXR induction cells are used for each of the two sequential pulses
- Upgrade the injector induction cells to achieve the same or higher injector output voltage for each pulse
- Complete the gamma-ray camera active image recorder (CCD)
  - — Provide dual image recording capability
  - — Increase recording sensitivity to more than compensate for the loss of dose from using half of the machine induction cells

parameters as shown in Table 6.1.

The next major step in improving hydrotest capabilities would be an Advanced Hydrotest Facility (AHTF shown schematically in Figure 6-1 from LLNL), which would offer multiple views (six, in one realization) at multiple times. This could be done by directing pulses from a linear induction accelerator among several different beamlines converging on the experiment. The physics requirements for this device, which is estimated to cost some $400M and take a decade to construct, are currently being defined by a committee with LANL, LLNL, SNL, and UK participation; a preliminary report is expected by June, 1995.

The crucial question in considering improved hydrotest capabilities is the cost/benefit trade. How useful is a given level of spatial resolution in assessing primary performance?, or how many views at how many different times are required to diagnose a 3D implosion and adequately benchmark a 3D computation? For considerations of nuclear safety, the time-dependent neutron multiplication rate, $\propto (t)$, is what really matters. Questions like "How accurately can $\propto (t)$ be deduced from radiographs?" can be answered by computer simulations of both implosions and the radiographic process. A program of such simulations apparently has been started by LANL and LLNL, and we would urge its completion and assessment as a prerequisite for any decision to construct a new facility such as the AHTF.

While it is clear that improved hydrotesting is crucial to continued confidence in the safety and reliability of nuclear primaries, it has significantly less impact on basic scientific issues. Apart from some questions of hydro-

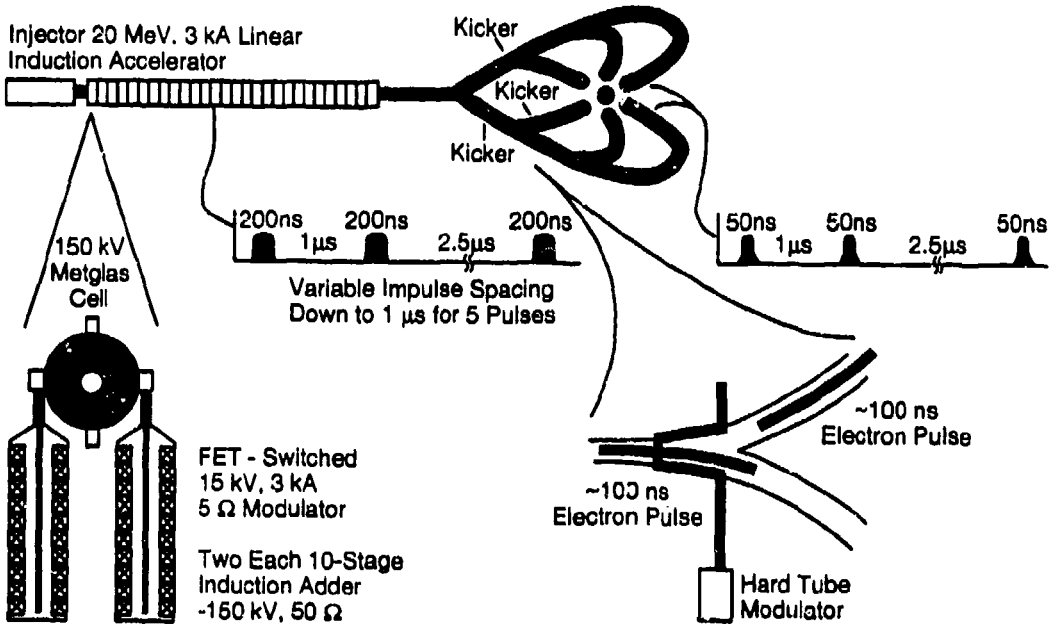


Figure 6-1. Design of the proposed advanced Hydro Test Facility (AHTF) illustrating multiple time-dependent image capability.

dynamic instabilities (which are probably best studied by other means), the processes and physical situations studied by simulated implosions are unique to nuclear weapons. As a result, these activities are unlikely to (and, indeed, should not) attract a broad spectrum of non-weapons participants.

The fact of hydrotesting and the use of dynamic radiography in assessing primary performance and safety have been unclassified for many years, and we would expect them to remain so. But in contrast to ICF, hydrotesting can be of great use to a proliferator designing a first weapon, or refining an existing device. Therefore, detailed information about hydrotest techniques and their results, as well as the actual radiographic images themselves, should remain classified to inhibit proliferation.

To go beyond hydrotesting and get a first glimpse of the very early nuclear stages of boosting requires hydronuclear testing. This subject is discussed more fully in a separate JASON report, and its proliferation implications have already been discussed in Section 1. (See footnote on page 5).

Beyond hydrotesting the primary, the detonation system that initiates the implosion of the primary is also a key element to be addressed by the SBSS program. Here we are talking about maintaining security against unauthorized or accidental introduction of arming and detonation signals, and strengthening use-controls to prevent detonation of weapons that may be stolen. The continued effort to diagnose, test, and as possible improve these non-nuclear components is an important component of an SBSS program. It can be pursued independent of a ban on underground tests, but it also

requires a high-level of technical expertise. We have not covered this topic in our study, nor the related one of ensuring physical security of the weapons against theft.

7 THE NATIONAL IGNITION FACILITY (NIF)

As the most scientifically exciting program proposed by the national laboratories for Science Based Stockpile Stewardship (SBSS), we feel that the NIF has an essential role to play in maintaining "the core intellectual competency" mandated by the 1994 National Defense Authorization Act (PL103-160). In our judgment, it contributes substantially to the three evaluation objectives listed in Section 3 and does not represent a significant proliferation risk.

Nuclear weapons operate under conditions of extremely high energy density similar to those in stellar interiors and hence of great interest to astrophysics. A science-based stewardship program should seek to simulate these conditions in the laboratory without nuclear testing. The inertial fusion program (ICF) represents the closest laboratory approach we know of to a number of critical parameters in the weapon environment. In particular, the NIF will make accessible Hohlraum temperatures roughly twice those available in NOVA, and at the lower end of the range of weapons interest. If the NIF reaches its goal of fusion ignition, then temperatures of 10 kev will exist in the ignition core.

The NIF (see Figure 7-1) will deliver a laser pulse of about 1.8 MJ energy content as compared to the 40 kJ in the blue spectral region now available on NOVA. Further, the NIF is designed with 192 beamlets (48 indepen-

The National Ignition Facility — 192 Beam

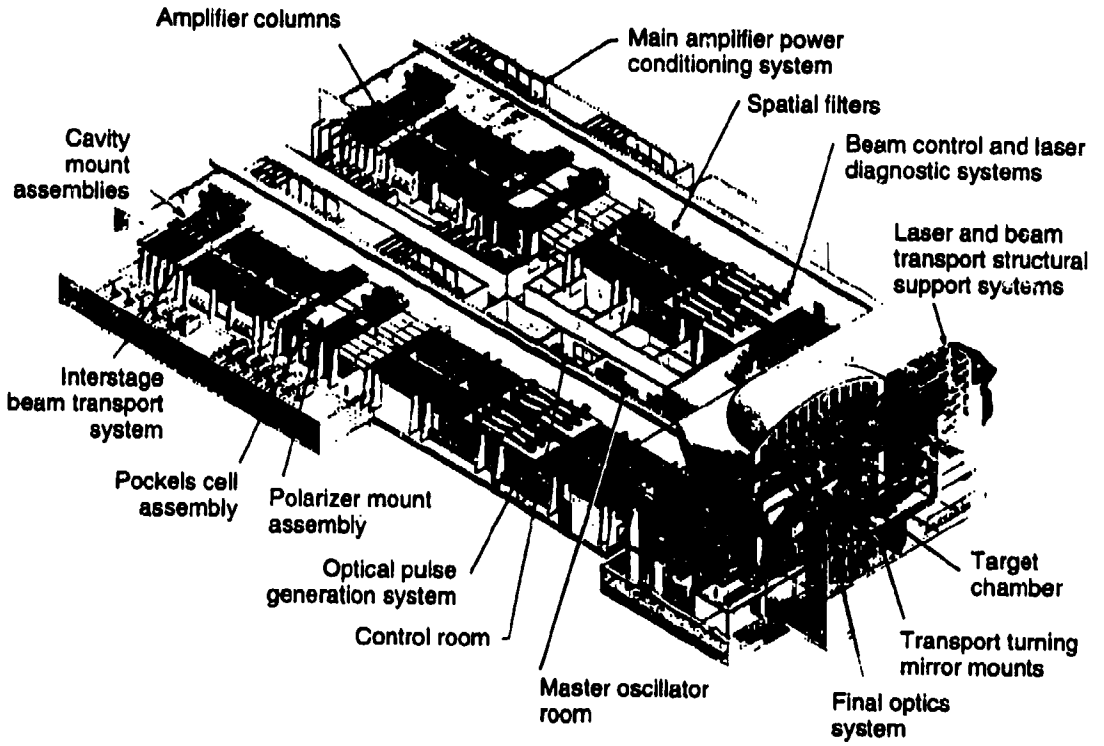


Figure 7-1.

dently pointable beams with provision for spatial and temporal smoothing) as contrasted with NOVA's 10 beams. This will allow for greater implosion symmetry than is available on NOVA, an important consideration for the attainment of the necessary high compression.

We now discuss the role of NIF in light of the evaluation criteria discussed in Section 3.

7.1 Inertial Fusion Energy

The most important non-weapons rationale for the NIF is as the scientific "proof-of-principle" experiment for Inertial Confinement Fusion. Thus its goal is to produce more thermonuclear energy than that injected by the driving laser pulse. There is no doubt that the objective of controlling fusion for an energy source has been over the years the magnet attracting many very bright people into the ICF program. This is not only because of the potential societal benefits but also because of the scientific challenge involved. The fuel must be compressed to densities of the order of 1000 g/cc with temperatures in the central hot spot of 5 kev. Success in achieving such a high convergence implies very symmetrical energy deposition on target as well as avoidance of the well-known hydrodynamic instabilities (Rayleigh-Taylor, Richtmyer-Meshkov, and Kelvin-Helmholtz) whose understanding is also critical to weapons design. The prospects for success depend on continuing the impressive advances of the ICF program in three important areas: (1) understanding of the underlying physics, (2) development of 3D codes capa-

ble of treating the radiation transport and hydrodynamics, and (3) perfection of diagnostics capable of resolving events on the tens of picoseconds-microns scale.

A program of experiments on NOVA, together with supporting computer simulations, has been carried out over the last three years in response to a Technical Contract recommended in 1990 by a National Academy of Sciences Review Committee [1]. This program has thus far achieved its milestones addressing target physics issues, and there is good reason to believe it will meet its several remaining challenges in the next two years as scheduled. There has been a generally good agreement of the experimental results with calculated predictions based on new integrated 2D codes developed independently by LANL and LLNL scientists. Since the same computer models are used in the calculations of target performance on NIF, the demonstrated ability to predict and diagnose NOVA implosions has strengthened our confidence in the prediction that the NIF laser of 1.8 MJ, with appropriate power pulse shape, will be able to produce ignition.

One further element of science is known to be required for the success of the NIF powered by a 1.8 MJ laser. The non-linear development of laser plasma interactions, which could lead to excessive light scattering, undesirable fast electrons, and implosion asymmetries must be understood and controlled. For almost a decade, these instabilities impeded progress in ICF. In the latest round of NOVA experiments, experimental conditions (laser intensity, wave length, plasma dimensions, and characteristics) relevant to NIF were shown to lead to tolerable laser plasma interactions. Work is still

necessary on this issue, and we emphasize the importance of continued participation by both LLNL and LANL scientists in further work that probes the sensitivity of integrated calculations to the precise laser conditions, and that further clarifies the laser-plasma interaction with NIF targets.

On the technology side, it should be noted that the Beamlet laser, one of 192 identical subsystems from which the NIF will be constructed, has now been successfully tested both at the basic laser frequency (1.06 μ) and with frequency doubling in agreement with design predictions. Work is now in progress toward achieving the power and energy goals with frequency tripling as designed for the NIF. Assuming that the Beamlet laser achieves this goal and demonstrates the soundness of the NIF laser design — and the work seems to be going well — the remaining major technical issue will be the ability to build to the required specifications (symmetry and surface roughness) the cryogenic targets required for the ignition experiments. We know of no fundamental difficulties that might prevent this from happening. It is planned to address this technological challenge as part of KD2.

In summary, the attainment of ignition in NIF will demonstrate an integrated mastery of forefront areas in hydrodynamics, radiation transport, computational physics, atomic physics, and plasma and non-linear physics. It is this overall challenge that is so exciting scientifically.

While we believe that there is strong evidence that NIF can obtain ignition (probably about as much evidence as existing facilities like NOVA can provide), nonetheless, the NIF will be exploring uncharted regions of high compression, and energy densities unique for a laboratory experiment.

Unpleasant surprises cannot be ruled out In the worst case scenario, NIF will come close to ignition with adequate diagnostics to determine accurately what would be the best design and critical minimum size pellet for both direct and indirect drive. Tests of such advanced ideas as the fast ignitor could also be made. Many defense and other science applications would be largely accessible even on a sub-ignited NIF. Naturally we expect continued progress in further evaluating ignition prospects from experiments on NOVA and on OMEGA upgrade, a direct-drive laser facility at the University of Rochester, and particularly from the ever more sophisticated computations in the coming years.

Of course, attainment of ignition on NIF is only a critical first step in the development of fusion as an energy source. For a reactor, the driver would have to be efficient (> 10 percent), capable of high rep-rate (> 1 Hz), and have a long lifetime. Many believe that solid-state lasers such as those utilized in NIF will not meet these requirements. Alternative drivers (KrF lasers, pulsed power driven light ion diodes, and heavy ion linacs) have been proposed as reactor drivers. In particular, the heavy ion accelerator seems very promising. However, the basic target physics of the pellet implosion would be pretty much the same for any of these drivers, and we concur with the judgments of previous reviews [1] [2] that the first priority for inertial fusion energy must be to verify the target physics and establish the minimum driver energy required for a successful implosion. This is best done with the most highly developed driver—the solid state laser, whose capability for several pulses a day is adequate for other scientific and defense applications. It should be noted that NIF will be designed to utilize both direct and indirect

drive.

Even after demonstrating ignition and developing a suitable driver, many practical issues will remain for commercial reactor development—in particular, target fabrication at reasonable cost and protection of the wall against damage from the high frequency microexplosions. But these issues can only be faced realistically, and power costs estimated, after the basic science and energy scale is established in a device like the NIF.

A similar situation exists in the other possible route to fusion energy: magnetic confinement. Again the key issue is to delineate the physics of an ignited plasma, in particular, to find its minimum size. This is the principal objective of ITER. While ITER is more ambitious than NIF in that many relevant engineering issues will also be addressed, many issues will still remain to be optimized. It is our belief that only after the physics issues have been experimentally resolved will it be meaningful to make a relative assessment of the costs and engineering difficulties of magnetic and inertial fusion, and hence to decide which (if either) should be pursued. (Along either route this pursuit is expensive.)

7.2 Other Science at the NIF

NIF's unique importance in establishing a SBSS program is further enhanced by the wide range of basic scientific problems that this facility could investigate. The credibility of the potential for cross-linking weapons sci-

ence with basic research has been established by the accomplishments in recent years of the national inertial confinement fusion (ICF) community using NOVA, OMEGA, and facilities elsewhere. This community has not only made notable contributions to classified problems in the weapons arena, but has also established an international reputation for unclassified work published in the open literature in a regime of high-energy and density, including contributions to atomic and plasma physics, to hydrodynamics, and to the development of novel spectroscopic sources (e.g., the x-ray laser) and diagnostic techniques. Furthermore, in conjunction with these experimental programs, remarkable computation capabilities have been developed, with an interplay of numerical simulation and experiment being a hallmark of the overall ICF program. Indeed, it is precisely this demonstrated ability for quantitative comparison between theory (via numerical modeling) and experiment (via extensive diagnostic capabilities) which makes the ICF program such an attractive avenue for coupling the stewardship program and the broader scientific community.

Potential areas for scientific applications of high-energy lasers and of the NIF recently have been reviewed[3] with an emerging sense that the NIF could provide significant opportunities for "external users" from diverse fields of science. In this regard, an obvious candidate is astrophysics since the NIF will allow the creation of hot dense plasmas under conditions relevant to numerous astronomical objects and processes (e.g., primordial nucleosynthesis, stellar evolution, and hydrodynamic instabilities in supernova). Measurements across a broad front will provide information about equations of state and opacities for matter with temperatures from several keV to 10 keV (with

ignition) under conditions found in the center of stars. In addition, there is potential relevance to emissions seen from the core of our galaxy and other violent events in high energy astrophysics. Apart from scientific convergencies, the remarkable range of diagnostic instrumentation developed to date for spatial, temporal, and spectral resolution provide a powerful basis for detailed quantitative studies of these diverse phenomena, and would thereby provide important validations of numerical codes for which observations are otherwise sparse from both the astronomical and weapons perspectives.

Beyond the astrophysics community, it is reasonable to anticipate and to promote ties with othe fields of research, such as is suggested by the "thumbnail sketches" of possibilities for several areas listed below [3].

1. Material science—The NIF would enable quantitative investigations of materials in a Gigabar pressure regime with rates of change of 10^{20} bar/s. Data obtained unde: these conditions would be quite valuable in the validation of theoretical models for certain aspects of ICF and could be relevant to understanding the physical properties of planetary interiors such as conductivity at high pressure.

2. Plasma physics—The most extensive studies of nonlinear wave-plasma interactions have beer performed on NOVA and will undoubtedly be continued at NIF. For these and other investigations, the NIF will provide a hot, dense plasma of relatively good homogeneity free from large spatial and temporal gradients and thus will allow better quantitative characterizations of electron and ion temperatures and densities, as well as plasma flows. In addition, if NIF includes a beam-line for generating

short pulses with high power, experiments to investigate a variety of less "programmatic" problems would be possible, including the study of relativistic plasmas and pondermotive effects near the energy density for electron-positron production.

3. Radiation sources—A very impressive accomplishment of the NOVA program has been the demonstration and systematic exploitation of x-ray lasers. For example, the recent demonstration of the capability for interferometry at 155$\AA$ is spectacular. With the NIF, this work should continue with an emphasis toward yet shorter wavelengths and new concepts for pumping mechanisms. Apart from the x-ray laser work, an impressive array of sources has been developed (ranging from continuum to atomic line sources) and has been employed both for (passive) diagnostics and for (active) initiation of various processes. The development and implementation of these sources requires detailed knowledge across a wide front (laser absorption characteristics, material equation of state, hydrodynamics), and hence illustrates a healthy interplay of multiple disciplines that typifies much of the research in ICF.

Beyond examples cited above, it is reasonable to anticipate and to promote ties with other fields of research, including nonlinear physics, geophysics and planetary science, hydrodynamics, and atomic and optical physics. Indeed, there have already emerged impressive track records of accomplishment within the ICF program that provide a credible base for the establishment of "user communities" at the NIF. A recurring theme in these potential scientific applications is the need for hot, dense plasmas of improved uniformity as would be provided by the NIF. By contrast, many investigations with

existing facilities (such as NOVA) are hampered by spatial inhomogenities and edge effects as well as by transient, as opposed to equilibrium, conditions. Further note that relative to existing ICF facilities, the NIF offers the singular advantage of the potential for investigation with an ignited (thermonuclear) plasma. In more practical terms, the instrumentation at NOVA (and presumably also at the NIF) has been developed with an eye toward flexibility and agility to enable the use of powerful diagnostic capabilities for "routine" measurements, which is precisely the mode of operation necessary for exploitation by an external community.

Although the impressive scientific possibilities associated with the NIF are beginning to be recognized by the university community as a result of recent declassification, the growth of this nascent enterprise needs to be further encouraged by way of the vigorous dissemination of information about the capabilities and accomplishments of the ICF program and about the scope of activities to be undertaken at the NIF. On the whole, the ICF community has a laudable record with respect to publication in the open literature and participation in the meetings of various professional societies. However, if scientific goals are to be a significant component in the justification of the construction of the NIF (as we strongly believe they should be), then the ICF community bears a special responsibility in fostering an "out-reach" program to a university community that currently faces a rather uncertain funding future. Succinctly stated, the NIF represents a credible and powerful opportunity to strengthen otherwise disjoint efforts in the weapons, the ICF, and the university communities.

7.3 The NIF and Competence

The challenge of ICF has attracted an outstanding cadre of young scientists and engineers. Successful stewardship will rely heavily upon keeping such people engaged and skilled in disciplines relevant to defense programs. The NIF and its goals are an ideal vehicle for achieving this goal. Both the scientific challenge and the energy goal will attract first-rate scientists.

The excitement of the NIF has attracted broad support and tangible participation from all three weapons laboratories, as well as from a broader U.S. and international scientific community. This latter has been stimulated by recent declassification.

Should the NIF be constructed at LLNL, which is likely since that is where the Nation's primary expertise with large lasers resides, this need not be construed necessarily as a commitment to a continuing weapons program there, because of the scientific opportunities and long-term goals of the NIF. Since NIF will be a national facility, weapons physics at NIF could in principle be carried out entirely by scientists from laboratories other than LLNL.

7.4 The NIF and Weapons Science

Adequate stewardship implies retaining the means and expertise to understand and deal with all aspects of nuclear weapons.

Much of the residual uncertainty in primary behavior arises from the detonation properties of HE (particularly the effects of aging) and the materials science of Pu relating for example to spalling. The NIF cannot contribute to the understanding of these issues, which are best studied by hydrotests. However, another class of uncertainties relates to the generation of mix at the various interfaces and its effect on booster burn. The effects of lower tritium concentration also need to be quantified. Here the experience gained from the development of the precise computer codes needed for NIF, as well as the diagnosable experiments on NIF burn, can probably be transferred to the understanding of primary behavior. The ICF program expects to develop 3D codes, to be benchmarked against NIF experiments, which would be essential for a better understanding of asymmetries that might arise in accidental or non-optimal detonations.

The NIF target physics is closely related to that of secondaries. Radiation transport and hydrodynamic calculations will have to be perfected to a high level to attain ignition. The ability to perform frequent implosions, to vary factors such as surface finish or laser pulse shape, and to diagnose implosions precisely will allow careful benchmarking of the codes which predict implosion and burn. It should be noted that in the ignition regime neutron spectroscopy and radiography will add a powerful new "weapon" to the diagnostic "arsenal."

NIF Hohlraum temperatures of 600–700 ev should be accessible, which will enable opacity and EOS measurements to be performed under close-to-secondary conditions. The effects on the implosion of such defects as

cracks in the radiation case can be directly studied both numerically and experimentally with tools developed for the NIF program. (See Figures 7-2, 7-3, and 7-4.)

Without underground tests a key tool for stewardship is sophisticated computation, benchmarked against laboratory experiments. The challenge, flexibility, and frequency of experiments on the NIF, together with the remarkable diagnostics already demonstrated on NOVA, will calibrate, exercise, and refine design codes. Such improvements, combined with 3D capabilities certain to be developed during the next decade, could remove much of the empiricism of present modeling and give added confidence in our predictive capabilities. Even today one of the most sophisticated weapons effects codes, LASNEX, was developed by the ICF program. These capabilities in the hands of informed scientists, are essential for monitoring and understanding the stockpile, and for responding to (if not anticipating) concerns about its aging, effectiveness, and safety.

7.5 Implications of the NIF for Non-Proliferation

In Section 4 of this study, we discussed the important problem of balancing non-proliferation objectives of the United States with responsible stewardship under a SBSS program. The question this raises relative to the NIF is whether, considering its size, cost, and technical sophistication it will contravene U.S. non-proliferation objectives by making it possible to advance our knowledge of nuclear weapons and thereby enhance our nuclear capabil-

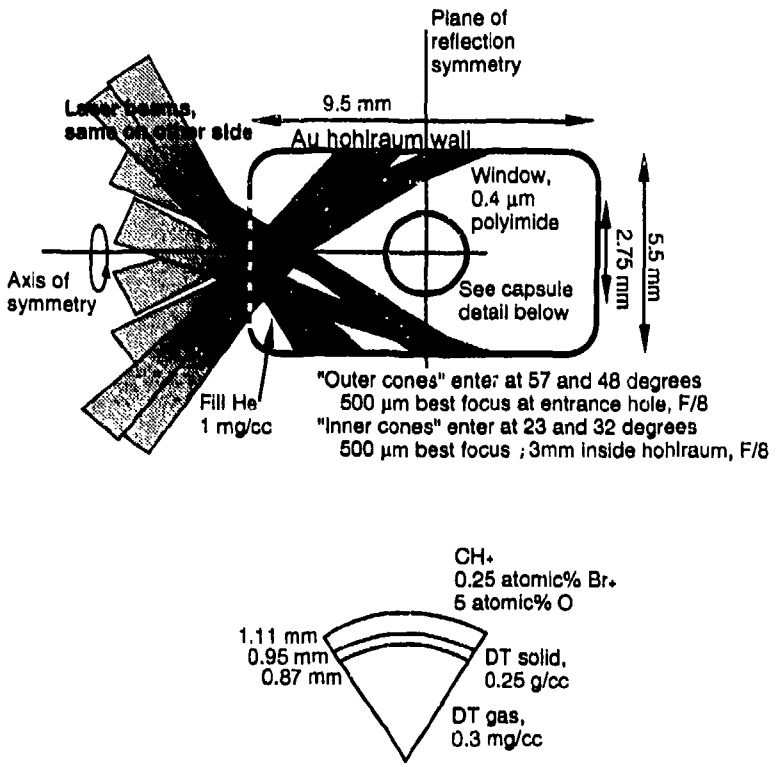


Figure 7-2. This is the point design ignition target for the NIF. The cryogenic capsule is suspended in the gold hohlraum, which is heated by the laser pulse. This design is somewhat smaller than an optimized 1.8 MJ, 4500 TW design, in order to allow margin for uncertainties in the target physics. (From LLNL)

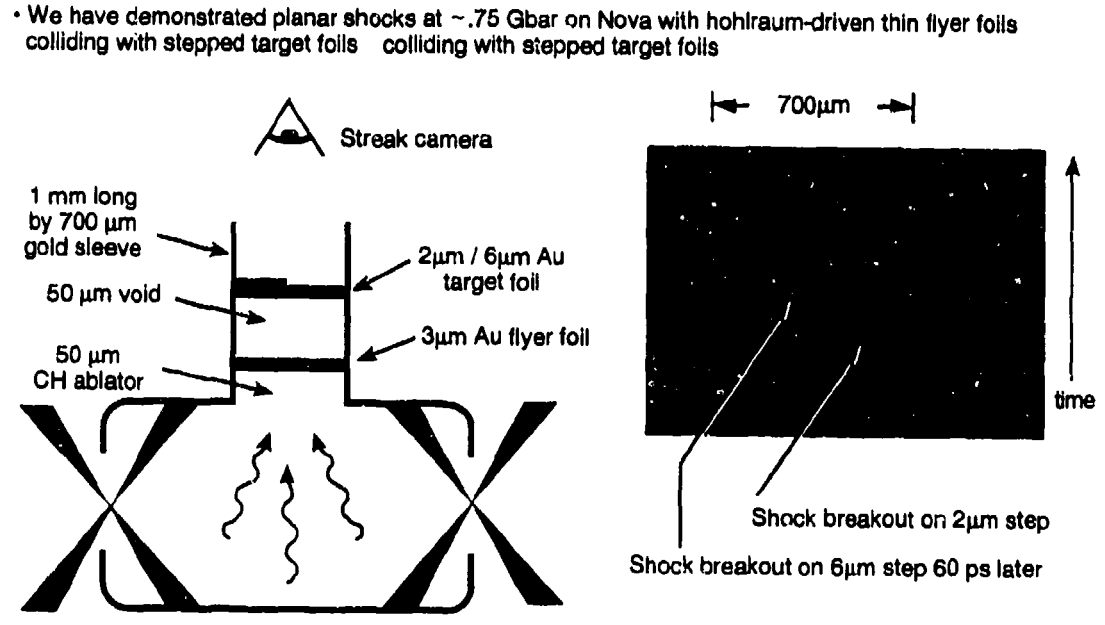


Figure 7-3. High pressure shock experiments on NOVA and NIF hohlraum driven colliding foils. (from LLNL)

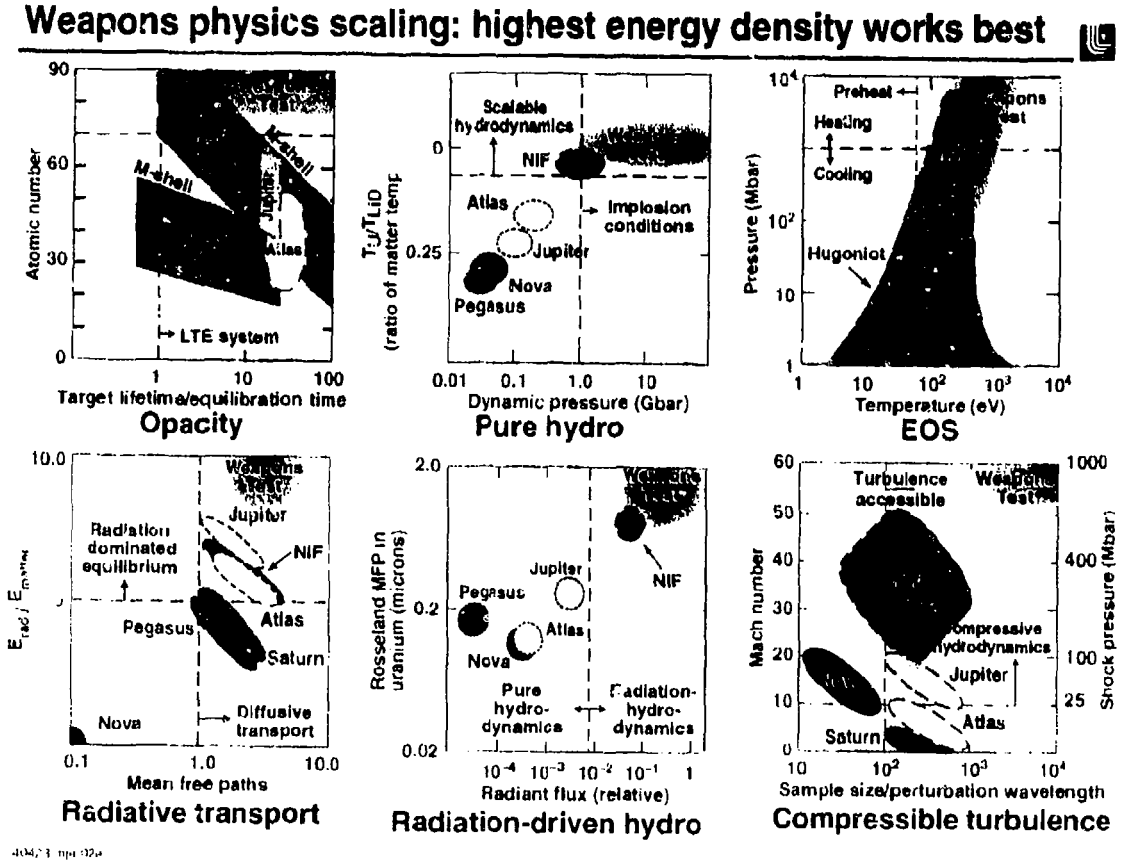


Figure 7-4. Comparison of phase-space coverage by various facilities (from LLNL)

ity with new designs. Such a concern is, of course, not specific to the NIF. It applies to all elements of a SBSS program, since they will all contribute to training and to added interest in nuclear weapons.

The key point to be understood in this connection is that the NIF is a program driven primarily by the goal of understanding inertial confinement fusion and achieving ignition. The "bargain" of the NPT encourages cooperation for peaceful uses of nuclear energy. Advances in understanding fusion as a possible energy source should be shared openly, consistent with this NPT bargain The NIF technology is not a nuclear weapon, cannot be adapted to become a nuclear weapon, and demands a technological sophistication far more advanced and difficult than required for nuclear weapons.

NIF will contribute to strengthening the science based understanding of secondaries of thermonuclear weapons, but without high-yield underground tests ($\approx$ 150 kt as under the current Threshold Test Ban Treaty), it is not practical to envision any significant (if indeed any at all) performance improvements emanating from NIF experiments. Along with other elements of the SBSS, NIF will contribute to training and retaining expertise in weapons science and engineering, thereby permitting responsible stewardship without further underground tests. As such, NIF contributes essentially to the goal of non-proliferation.

Specifically with reference to the NIF, most of the work can now be done openly and cooperatively as a result of the recent guidelines declassifying much of the ICF research program. High energy density physics (and astrophysics) studied world wide overlaps many parameters anticipated for

NIF and there is little reason to keep the dividing line between unclassified and classified work where it now sits at 350eV hohlraum temperatures. There should be a careful, detailed study taking into account what is understood outside the weapons program to further reduce the need for classification. Some suggest this would lead to allowing the physics for the higher hohlraum temperatures anticipated for NIF (up to 600eV) to be declassified. Any restraint on making portions of codes such as LASNEX available should be justified on grounds of protecting against proliferation. The more open the research program of NIF, the better the U.S. will be able to blunt the concerns about its contribution to proliferation. The program can and should be explained as a necessary component of a responsible SBSS program.

To summarize, the NIF is an extremely sophisticated challenge, not one which could conceivably be undertaken by, or be useful to, a potential proliferator. The necessary physics for simple weapons design of a type useful to third-country proliferators is already declassified. While detailed design codes should not be [illegible]d, openness on NIF could dispel fears of a secret U.S. program for new weapon development. Given the desirability of "scientific stewardship" we believe NIF to be fully compatible with U.S. goals for both a NPT and CTBT.

8 LANSCE, STOCKPILE SURVEILLANCE, AND MATERIALS SCIENCE

8.1 Introduction

A core stewardship activity is to maintain the diminishing stockpile consisting of current weapons designs, with no new designs or manufacture in the pipeline. Here the problem is to fight all the effects of age on weapons remaining in the stockpile far beyond the lifetime, which had been anticipated when they were first built. These effects include formation of heavy-metal hydrides; the effects of He from Pu α-decay, such as swelling and embrittlement; cracks, voids, porosity, and gaps in both heavy metals (from the above effects and others) and in high explosives; stress and failure modes in welded parts; surface bonding and texture formation; and many others. All these are materials-science issues, and hence materials science assumes a particular importance for stockpile surveillance.

There is, perhaps, a golden opportunity for studying these effects en masse, since the U.S. (and, by its own statements Russia) is currently dismantling close to 2000 weapons per year. A high-statistics study of those dismantled weapons, perhaps 50-100 per year, might reveal much more information than is currently being gleaned. Currently dismantled weapons are subject to a cursory inspection by conventional x-ray radiography, and a few weapons each year are taken out of the stockpile and completely disassembled, except for the pits which remain sealed with the Pu not physically

accessible. (A dismantled weapon is not completely disassembled; for example, the secondary and pit are removed intact and stored in separate facilities.) It is too expensive and time-consuming to disassemble 50-100 weapons per year, so non-invasive inspections, that could in principle be done in large numbers and which address the relevant materials issues, are of great interest. This is the context in which we comment on the contribution of LANSCE to stockpile surveillance by means of neutron radiography.

The LAMPF complex at the Los Alamos National Laboratory has been converted to the Los Alamos Neutron Science Center (LANSCE, see Figure 8-1 from LANL). The Defense Programs (DP) division of DOE has taken responsibility for the continued support of the complex with the understanding that the principal thrust of the LANSCE program will support DP objectives. This commitment includes operating the facility as well as managing an upgrade program, since the aging LAMPF complex will require major replacements. Operations, not including the scientific program, are estimated at $30 M a year and the upgrade program is estimated at $35 M.

The potential utility of LANSCE extends beyond applied neutron science. There continues to be the projected use of the facility as a test-bed for Accelerator Production of Tritium (APT) and for Accelerator Based Conversion (ABC) for destroying actinides, including plutonium. At this time the future of both of those programs is uncertain. We shall include them in our considerations of the potential role for LANSCE in stockpile stewardship.

The LAMPF accelerator is the highest average power proton accelerator in the world, and so is its potential neutron flux, among current spallation

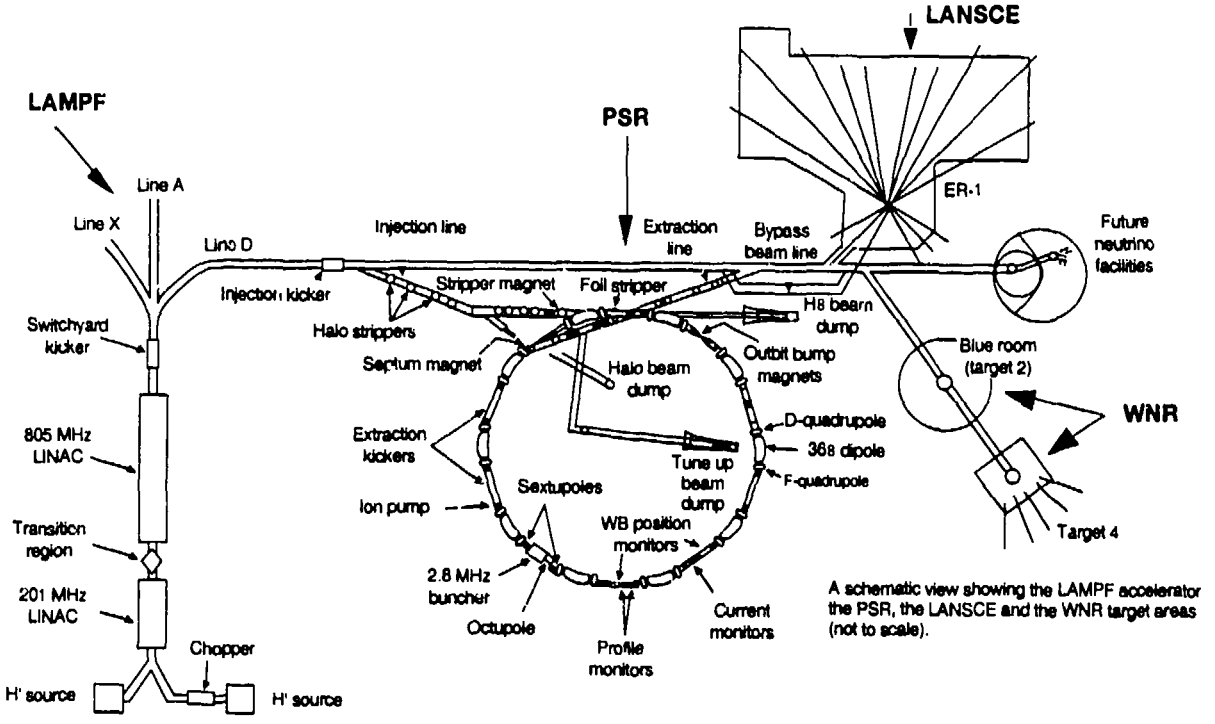


Figure 8-1. The LANSCE complex at LANL.

sources. However, there are many other worldwide facilities approximating the neutron flux performance of LAMPF and the energy of the accelerator is larger than that needed for an optimal neutron science program. Also, since the transition of the complex to its new LANSCE function is relatively recent, there is at this time a lack of adequate involvement of highly competent materials scientists in the program so that its long range potential in applied and basic neutron science is difficult to extrapolate from the current activities.

The specific materials science and surveillance issues which can be addressed by LANSCE are discussed below.

8.2 LANSCE and Stockpile Surveillance

One fundamental advantage of LANSCE for weapons surveillance (as in materials science) is that, unlike x-rays, neutrons are sensitive to the presence of low-Z materials, the state of which in nuclear weapons is of great importance. There are two broad areas where LANSCE is important. One is neutron radiography, which makes use of high-energy neutrons (but not necessarily as energetic as those produced by LAMPF) to make non-invasive radiographs of entire weapons. The other, related to conventional materials sciences, makes use of low-energy neutrons to study weapons components.

In neutron radiography, energetic (10's of MeV) neutrons are transmitted through an intact weapon, and detected on an image plane. It should be

possible to achieve a resolution of about 1 mm, substantially worse than conventional x-ray resolution, but still perhaps enough to detect cracks, chips, voids, and the formation and migration of hydrided heavy metals. It takes a fair amount of time to produce a good neutron radiograph, at source strengths compatible with long spallation-target lifetimes. For example, a dedicated LANSCE-like neutron spallation source driven by a 100μA, 200 MeV proton synchrotron might need an hour to make a radiograph at 1 mm resolution with a signal 5σ above background.

An alternative to simple neutron radiography is to use the neutrons to induce (N,γ) reactions in the low-Z elements, to detect the resulting $\sim$ MeV γ-ray with a γ-ray camera, and to do tomography from the data. This is a standard medical imaging procedure (SPECT: Single-photon emission computed tomography). We do not know precisely what resolution and time scales would be involved, but they are likely to be acceptable for the surveillance task.

In more conventional materials science, there are several surveillance issues related to weapons components: aging and performance of high explosive (burn, shock); aging and hydriding of Pu and U; stress and texture in welds, pit surfaces, etc; neutron cross-sections; and equations of state of high explosives.

While some of the materials-science applications listed above are straightforwardly applied to stockpile surveillance problems, neutron radiography of intact weapons is not yet reduced to proven practice. And for two reasons it is not clear whether the present LANSCE facility would be chosen for

production radiographic surveillance. The first reason is that the LAMPF accelerator is really too powerful; one does not need neutrons of hundreds of MeV, and 1 MW of power focussed to $\sim$ 1 cm spot size on a target leads to target lifetime and cooling problems. The second reason is that it may not be feasible or desirable to transport 50-100 weapons per year to Los Alamos for study there. Both drawbacks can be overcome by using a smaller dedicated source (as mentioned above, possibly a 200 MeV, 100 μA proton cyclotron) at Pantex. Of course, LANSCE in any event would be useful for weapons components testing.

8.3 LANSCE and Materials Science

There is no question that neutron scattering is a very valuable tool for studying materials. Much of the world's work in this area is done with reactor neutrons, and done quite successfully. However, accelerator-driven pulsed spallation neutron sources, such as LANSCE, have certain advantages which are becoming more important as data processors become faster, since, in general, pulsed sources allow the acquisition of many data in a short time.

The LANSCE accelerator, that is, LAMPF, fills a proton storage ring (see Figure 8-1) with numerous accelerator pulses; the ring is then emptied onto a tungsten target in $\sim$ 270 nsec. The neutrons are then slowed to energies appropriate for materials science ($\sim$ 5-500 MeV) and directed to the neutron target. The epithermal neutrons are still tightly bunched in time (some tens of μ sec), so that the neutrons' energies are accurately measured

with time-of-flight (TOF) instruments (a 50 MeV neutron takes about 5 msec to go from scattering to detection). Neutrons of various energies are measured in a single pulse, unlike reactor neutrons, in which case Bragg spectrometers must be readjusted for all desired initial and final neutron energies. This takes much time, which used to be acceptable when processing of data was equally slow. However present fast processors fit very well with the rate at which data can be taken at LANSCE.

A common use of neutron scattering is elastic coherent scattering to measure atomic (e.g., crystalline) distributions in solids; the principle is the same as for x-ray scattering, and wavelength scales are similar (for a 50 MeV neutron, $\lambda = 1$ angstrom). Actually, x-rays and neutrons are complementary: x-rays see electrons, and neutrons usually see nuclei; neutrons scatter well off low-Z nuclei, while x-rays have trouble seeing such materials. For example, the structure of the high-temperature superconductor $YBa_2Cu_3O_{7-x}$ cannot be elucidated well by x-rays alone (which have trouble with the O) or by neutrons alone (which do not distinguish Y and Cu), but together they led to a complete solution of the structure of this material. One can also study inelastic neutron scattering, especially with the $\sim$ 1eV neutrons available in a moderated spallation source, to look at low-Z elements and their reactions in catalysts and hydrogenation. Neutrons can scatter off unpaired electrons by virtue of the magnetic-moment interaction, and they can be polarized to measure certain important details of this electron-neutron interaction (e.g., in anti-ferromagnets).

So far we have discussed applications which could also be done with

reactor neutrons. However, sometimes the short pulses are essential, when the process being studied itself has a short duration in time. An example is the study of materials in very high (30-40T) magnetic fields, where the magnetic fields are generated by pulsed methods.

LANSCE has not had the impact it should have had on U.S. materials science. One reason is the large amount of downtime suffered in recent years. Los Alamos has promised to bring operating time up to 9 months/year, and we recommend that Los Alamos and DOE exert every effort to fulfill this promise. We also recommend that Los Alamos strengthen the materials—scientist presence at the laboratory: LANSCE cannot perform its stewardship function unless there is a strong impetus from world-class science being done there.

8.4 Other Uses of the LANSCE Complex

Recognizing that the LAMPF based LANSCE complex is a major facility on a world scale, DP has taken responsibility for the evaluation of its broader utilization, beyond this core stewardship minimum. We give here brief comments about that utility.

1. Accelerator production of Tritium (APT). As planned goals for the production of tritium shrink, the competitiveness of APT relative to a New Production Reactor (NPR) or to utilization of an existing or partially completed Light Water Reactor (LWR) increases. An APT

facility addressing recently reduced goals can be built at a cost near one billion dollars and with a power consumption matching that of existing frontier high energy physics facilities. The critical path item in developing and continuing an APT is its target complex, not the accelerator [4]. The LAMPF component of LANSCE is useful as a test-bed to address:

- beam intercept and other orbit dynamics issues for an APT
- more precise measurements of neutron yield and economy in a target complex
- target complex development at low power
- tritium handling at a one-percent scale.

2. Accelerator Based Conversion. This application has also been addressed in an earlier JASON report [5] and will be a major topic of the NAS STATS study on nuclear waste conversion.

   LAMPF is potentially the most powerful test-bed for this activity to the extent it will be supported in the U.S. Development of the accelerator itself is not on the critical path for the above mission. As noted in the referenced studies, critical issues include:

   - ABC is not a competitive candidate for WGPu disposition to the "spent fuel standard."
   - ABC remains of interest to long range waste conversion and to Pu disposition "beyond the spent fuel standards"
   - It is not clear at this time whether non-accelerator based options are more cost-effective for the above mission

- ABC has many technical variants as to the choice of fuel cycle in the sub-critical assembly, the use of on-line and off-line reprocessing technology, etc. LANSCE can yield data useful in examining these choices.
- ABC systems in principle can attain higher neutron densities than critical reactor-based systems.

3. Basic Science Application of the LANSCE Complex. In addition to the neutron materials science application discussed above, LAMPF as the world's most powerful medium energy proton source can support continuing highly important goals in elementary particle physics. While this opportunity no longer warrants support of the full LAMPF complex, ER has expressed willingness in principle to provide about $10 M per year for such a program. Of particular interest remain the observations of extremely rare branching ratios of the decays of muons and pions.

4. Accelerator-based Power Sources (APS). The use of a subcritical reactor assembly, stimulated by accelerator produced spallation neutrons, has recently re-entered consideration as a source of nuclear electricity. The basic idea is several decades old, and the basic pros and cons for such an arrangement have long been recognized:
   - APS systems can be controlled on a sub microsecond time scale.
   - APS systems are basically more expensive both in terms of capital investment (the extra capital cost of the accelerator) and operating cost (the electric power consumption of the accelerator).

- APS systems can compensate for an inadequate neutron economy in the fuel cycle during burn-up.
- APS systems must address safety problems akin to those of ordinary reactors such as residual radioactive decay heat, reactivity excursions beyond the subcriticality margin etc.

A specific APS system based on the thorium fuel cycle has recently been promoted in Europe. A major advantage claimed is that:

- no plutonium is produced
- A breeding cycle can be sustained which would be marginal in a non-APS system since thorium has fewer neutrons per fission.

While these claims are correct, it must be noted that:

- The uranium isotope U-233 is as suitable a bomb material as plutonium
- If U-233 is "denatured" with U-238 to make it non bomb-usable, and is then used further as reactor fuel in the breeding cycle, plutonium is produced. Thus the proliferation problem of a breeder cycle is not basically altered.
- Because the growth in time of certain isotopes, notably of Pa, is critical to design, the attainable neutron flux density is a critical design issue.

There does not even exist a "pre-conceptual" design of an APS system and the "devil is in the details." Thus the competitiveness in time of this scheme as an energy source cannot be evaluated now. Should pursuit of this

approach be decided on, the LANCE complex could be a useful test-bed for experimental studies.

8.5 Summary

In the area of stockpile surveillance, LANSCE offers the promise, which remains to be proven, of neutron radiography of dismantled but undisassembled weapons, with the specific possibility of seeing low-Z materials. Given a strong collaborative effort between LANL and outside materials scientists, plus the promised nine months per year of LANSCE operation, LANSCE is clearly a valuable international scientific resource.

It remains to be seen whether LANSCE is the optimal facility for inspection of a fairly large number of nuclear weapons. For various reasons it might be that this could only be done at Los Alamos for a very small number of devices and therefore, should success materialize, a dedicated facility for inspecting nuclear weapons with neutrons would have to be acquired elsewhere at substantial cost. Neither the optimum parameters nor cost of such a dedicated facility has yet been examined, but one can guess that a 200 MeV, 100μA cyclotron would suffice.

The accelerator complex at LANL also has other potential uses, some of these related to nuclear weapons (e.g., accelerator production of tritium). Continued near-term support of LANSCE to evaluate these uses, to evaluate its uses in stockpile surveillance, and to build a strong materials-science

center, is warranted. Longer term support would be based on the progress made toward successfully achieving these near term goals.

9 PULSED POWER

We have considered Laser Pulsed Power in connection with the National Ignition Facility (NIF) and Inertial Confinement Fusion (ICF). In this chapter we wish to discuss electrical pulsed power technology and how it can contribute to the SBSS program. The electrical machines we will talk about generate very high energy density volumes, ranging from fractions of a cubic centimeter to several cubic centimeters, through the use of large capacitor banks (Marx generators), pulse forming networks, inductive energy storage with fast switching voltage addition networks, and even high explosive pulsed power generators. We will discuss this technology in the following. We will also comment on how these facilities can contribute to science, maintain a skill-based competence and relate to important aspects of weapons physics and stockpile stewardship. There are a number of existing pulsed power machines that fall broadly into two categories: fast pulsed ones ($\leq$ 300 nsec) i.e., PBFA II and SATURN at Sandia National Laboratory (SNL); and PROCYON at LANL and slow pulsed ($\sim 1\mu$ sec) ones at LANL; i.e., PEGASUS II; plus several others.

We do not intend to comment on all of these facilities. Our intent is to try to set two proposed new facilities, ATLAS and JUPITER, into perspective and discuss how they may fit into the SBSS program, particularly relative to the ICF approach to high energy density (NOVA and NIF). However, to set the stage for comparisons, and the parameter reach of the proposed new facilities, we first give a brief description of each one and illustrate the

physical phase space region it can probe.

PBFAII is a fast pulsed accelerator ($\sim$ 50 ns) at SNL. Its purpose is to create an intense ion beam for hohlraum physics. The most critical contribution is to create a 140 eV hohlraum for 10–20 ns in a substantial fraction of a cm^3 volume. Obvious uses include the study of nuclear weapons effects (bremsstrahlung spectrum) and light-ion drive for ICF. Using the long drive time relative to lasers, it also provides radiation flow over large enough volumes to look at aging effects on materials (flaking, corrosion, etc.) which can be studied on an interesting scale. When modified to drive magnetic implosions, its x-ray yield is greater than 2 MJ.

SATURN at SNL is a fast-pulsed accelerator driven by a Marx capacitor bank that can produce a 600 kJ radiation source from a 4 MJ store. This source can be used for studies of nuclear weapons effects or to create hohlraums up to 100 eV. SATURN is used in international collaborations with the UK and Russia. The hohlraum (of volume .25 cm^3 at 100 eV) is loaded with foam (not vacuum as with NOVA and NIF). The foam can be graded in density so as to study radiation pulse shaping. SATURN drives a peak current of 10 MA.

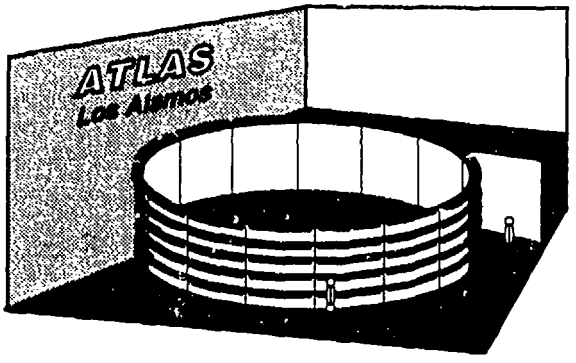
PROCYON at LANL is a 15 MJ high explosive pulsed-power system with an explosive fuse opening switch providing a 2 to 6 microsecond drive. Using a plasma flow switch, the pulse duration can be reduced to less than a microsecond. PROCYON has been used for direct-drive plasma implosions to produce soft x-rays for weapons physics experiments. The measured implosion parameters are an initial radius of 5 cm for a 68 mg aluminum

plasma, a final radius less than 0.5 cm, an implosion kinetic energy of 1 MJ, a temperature of 50 eV and a soft x-ray output of 1 MJ.

PEGASUS II at LANL has a 4.3 MJ capacitor bank with a slow (up to $\sim 6\mu$ sec.) direct drive for hydrodynamic studies with an experimental volume of 1 cm^3. A precision cylindrical liner drives ejecta experiments, with an axial hologram diagnostic that characterizes particle size and average velocity. The smoothness of the drive matches the resolution limit of the recording film. It also has a plasma flow switch for pulse compression as in PROCYON. Experiments with the existing PEGASUS facility with its excellent diagnostics are relevant to the SBSS. Pegasus can study such important phenomena as melting and hydro in primaries, early and late time spall in converging geometries, distortion in implosion systems, and effects of gaps, among many others. There have also been many proposals for collaborative studies of the physics properties of matter in mega-gauss fields.

ATLAS is a proposed new pulsed power facility with a 36 MJ capacitor bank that will offer an order of magnitude increase in dynamical pressure over PEGASUS, bringing it to within a factor of two to three of that created in a weapon test. Its main features are listed in Figure 9-1.

Referring back to Figure 7-4, one sees that ATLAS offers the unique and important new possibility of doing hydro experiments on macro-sized targets ($\sim$ cm in dimension) at high enough pressures and temperatures to achieve material ionization. This is an important regime typical of the late stages of primary implosion and of the early hydro stage of the secondary, and is important, in particular, for the study of the effects due to cracks and other



Current to target:	25 MA
Pulse length:	0.3 - 2.5 μs
Stored energy:	36 MJ
Capacitance:	200 microfarads
Output voltage:	540 kV
Inductance:	30 nH

- Pressure: >10 Mbar in 1-cm^3 volume, nearly gradient free
- Temperature: ~200 eV for radiation, atomic physics, plasma physics
- Implosion energy: 2-3 MJ in plasma or solids
- Large-scale experiments wtih high-resolution diagnostics
  - — Optical and x-ray imaging
  - — Spectroscopy
  - — Triple-axis radiography
  - — Laser holography
- Applications to both primary and secondary physics
- FY96 start — FY98 operation
- And beyond . . . Hercules or 100-MJ high-explosive pulsed power

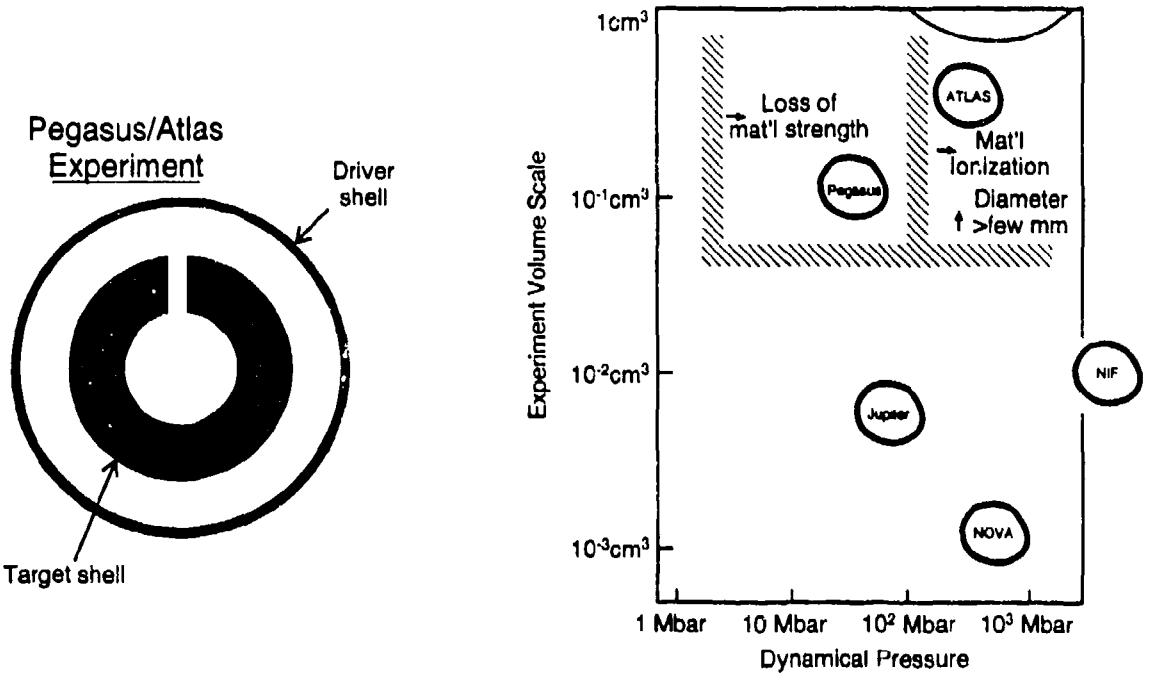
Figure 9-1. Atlas is the next step in high-energy pulsed-power for weapons physics applications. It permits hydro studies in ionized media.

material degradations, particularly those with high-aspect ratios, that may appear. This capability plus the study of melting and hydro in primaries, defines the real case for ATLAS and its contribution to weapons science in the SBSS. Figures 9-2, 9-3, and 9-4 illustrate important characteristics and new capabilities of ATLAS, including its potential as a radiation source for study of nuclear weapons effects. ATLAS also offers great scientific potential to a broad community with its reach to very high $\vec{B}$ fields.

There appears to be an important mission in the SBSS for ATLAS. At a construction cost of about $43 M, making use of existing buildings, extensive use of existing commercially available switches and also of the diagnostic equipment at Pegasus II, it is a real bargain. If given a timely positive KD1 it could be operational in FY 98.

The use of ATLAS, and pulsed power machines in general, in fundamental science has not yet been thought through very thoroughly. The conditions achievable by ATLAS—compression of materials to greater than 10 Mbar, producing magnetic fields of 10 MG in relatively large volumes ($\sim$ 1 cm^3) for reasonably long times (up to $\sim 5\mu$ sec)—are very unusual and should provide opportunities for interesting research in many-body physics, astrophysics and atomic physics. Clearly people in the program need to be much more effective in soliciting collaboration with outside groups to exploit these opportunities.

We recommend that a KD1 on ATLAS be approved to allow the program to proceed in an expeditious manner; our only hesitancy in this recommendation results from our own limited knowledge of whether or not it is possible



- Cylindrical experiments to enable diagnostic analysis
- Experiments must be sufficiently large to allow detailed diagnostics and fabrication of complex perturbations, implies volume $\gtrsim 0.1$ cm^3
- Implosion must be near-perfect (implosion nonuniformities small vs. perturbations)
- Presure must be > material strength and (desirably) into ionization regime

Figure 9-2. Effects of gaps or other perturbations on implosion hydrodynamics.

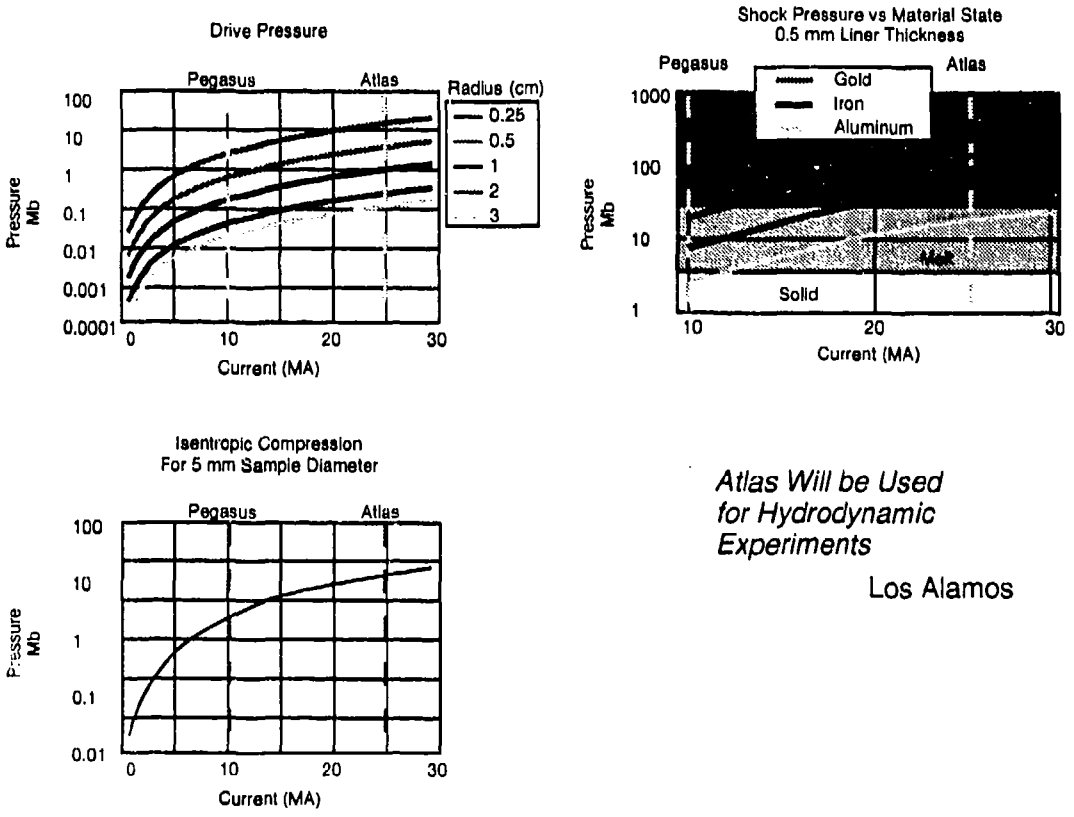


Figure 9-3. Atlas will be used for hydronamic experiments.

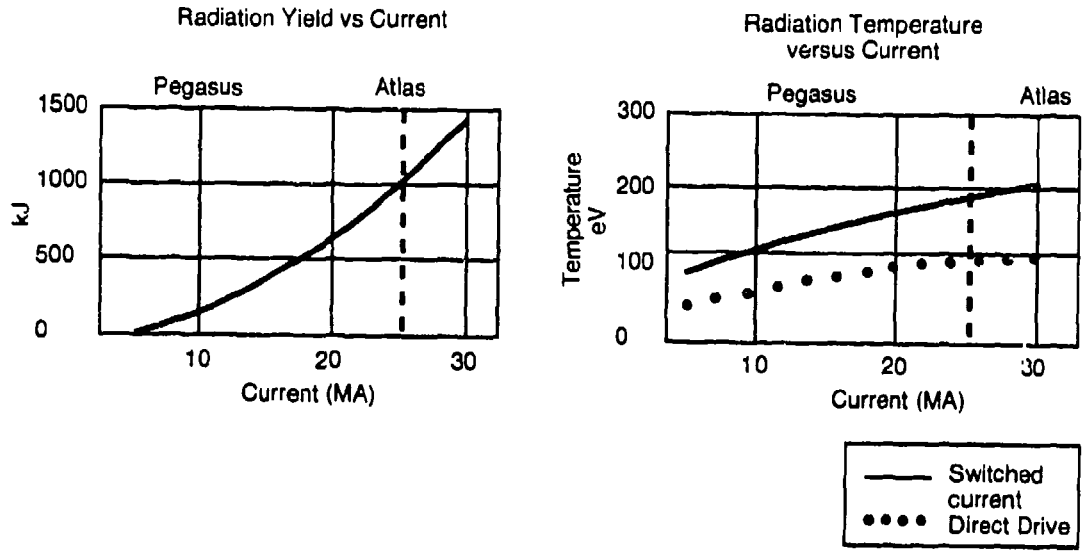


Figure 9-4. Atlas will be used as a radiation source.

to modify, effectively, and economically, existing short pulse (< 300 nsec) devices to imitate ATLAS parameters.

Finally, the JUPITER facility (15 MJ radiation output; 250 eV hohlraum temperatures; construction cost of ~ $240 M) was conceived as a joint program by DNA and Sandia to provide the most powerful above-ground nuclear weapons effects test machine for x-rays. This in itself is of interest but at present is not a major focus of the SBSS. In other aspects of the SBSS, JUPITER would provide only an incremental addition to the current existing suite of pulse power machines and can not be justified solely on the basis of SBSS. Figure 7-4 illustrates how it compares in parameter space with ATLAS, NIF and other facilities. We believe that ATLAS, as currently planned, together with the existing PEGASUS II, will more than adequately cover the domain of macro-sized targets for hydrodynamics studies. Experiments involving radiation or burn can be addressed over broad regions by the NIF. There is, however, a great science interest in JUPITER because of the very high magnetic fields and the critical fields for high temperature superconductors. Any decision on JUPITER, should be deferred at this time for future consideration.

9.1 Summary

The existing and planned pulsed power facilities included in Figure 7-4 have real merit in providing the nation with important contributions to the SBSS.

No one machine or facility can adequately address all of the problems attendent on the stewardship program. It is evident that the NIF will be dominant in all of the parameter spaces shown when it comes to reproducing bomb conditions. However, one important parameter that is not represented in the figure is that the NIF target volume is only millimeter in size whereas the pulsed power machines have target volumes that approach sizes larger than a cm^3. In any event, ATLAS and JUPITER do overlap the NIF in some of the parameter spaces shown and are complementary in others. In the realm of implosion hydrodynamics, while NIF and NOVA are best suited to study that subject for the miniature capsules used in the ICF research, their small targets have difficulty in faithfully modeling gaps and cracks of high aspect ratio that may show up in aging weapons in the inventory. The effect of such imperfections can more faithfully be modeled and studied with the larger experimental volume offered by the ATLAS concept as shown in Figure 9-2.

10 SPECIAL NUCLEAR MATERIALS AND PROCESSING

An important part of the nuclear stewardship program of the United States will be the element devoted to maintaining expertise and remanufacturing capability for weapon components that are made of special nuclear materials (SNMs). Of highest importance are those composed of highly enriched uranium (HEU), weapons-grade plutonium (WGPu), and tritium (T). Certain other materials, including lithium-deuteride (^{6}Li-D), depleted uranium and beryllium, are also important to nuclear weapons, but the technologies associated with these latter materials present less critical questions for stewardship.

As discussed elsewhere in this report, the primary—if not the sole—nuclear weapons manufacturing capacity that must be provided for in an era of no nuclear testing is the remanufacture of copies of existing (tested) stockpile weapons. While in some cases deficiencies may be discovered that require changes in non-critical, non-nuclear components of existing designs, the ultimate goal should be to retain the capability of remanufacturing SNM components that are as identical as possible to those of the original manufacturing process and not to "improve" those components. This is especially important for pits since they are critically involved in the proper functioning of a weapon during implosion and in the stages that follow. Because pit implosion takes plutonium/uranium through a sequence of states that cannot be achieved outside of an actual nuclear explosion, and since these states are

not fully understood from first principles even today, it is highly unlikely that significant design changes will be undertaken for pits in an era of no testing.

In discussing the remanufacture of pits and other nuclear components of tested designs, it is important to distinguish between the manufacturing process itself and the final manufactured object. It is the latter that must be essentially identical in performance to the original item, not every detail of the manufacturing process itself. For example, new environmental and safety regulations and other considerations will likely require departures from some of the original manufacturing procedures. Whatever the reason, all changes from the original manufacturing process must certifiably result in a remanufactured component that is identical to a tested pit. Whenever components from disassembled nuclear weapons, e.g., pits, are available and can be certified as meeting original standards, they should be used first in a weapon remanufacturing process. Only when original manufacture, but certifiably good, pits and other needed nuclear components are unavailable, should component remanufacture be done for weapons that are to go into the active stockpile.

The only SNM for which production capability will be required in the foreseeable future is tritium. This is a consequence of the relatively short half-life (12.3 yr.) of this material. The precise scale of the production capability needed for new tritium has been the subject of considerable discussion and revision for over a decade now as expectations concerning the future size of the U.S. nuclear operational stockpile have been steadily revised downward. The U.S. currently has no major active capacity to manufacture tritium.

Dismantlement of U.S. nuclear weapons under START II and correspondingly large reductions in tactical nuclear weapons will result in a recovered amount of tritium adequate to supply the needs of the remaining operational stockpile until close to the end of the first decade of the twenty-first century. Any further reductions in the stockpile levels below the START II number will allow an even greater delay in the date new tritium production must begin .

The tritium supply problem involves issues that are primarily economic, not technical, in nature and must be addressed whether or not a comprehensive nuclear test ban treaty is negotiated. At present, DOE has not identified the best means for future tritium production. A key decision is whether to build a dedicated high current proton accelerator for tritium manufacture, to construct a replacement for the K reactor at Savannah River, [4] or to utilize an existing or an almost-complete light water reactor that the government can purchase from an electrical utility. Purchasing tritium from a foreign supplier is another option. Since tritium is employed in a gaseous form in nuclear weapons, component manufacturing is not involved for this material.

In contrast to tritium, the existing vast U.S. stockpiles of HEU and weapons-grade plutonium (WGPu), and additions to these stockpiles that will come from the scheduled dismantlement of U.S. nuclear weapons, means there is no need to retain capacity for manufacturing new stocks of these materials.[6] Instead, the key issues concerning HEU and WGPu are the safe and secure management and disposition of excess stockpiles of these materials in the United States and Russia, issues that have already received considerable attention [7], and maintaining a knowledge base in metallurgy

and chemical processing to understand their aging behavior, particularly required for Pu which is an extremely complex material.

The key SNM manufacturing expertise that the U.S. needs to maintain in its stewardship program is the ability and capacity to cast, machine, and finish metallic uranium and plutonium, particularly weapons-grade plutonium and highly enriched uranium (HEU). The technologies of cladding and coating these materials in nuclear weapons must also be preserved.

Preserving a SNM remanufacturing component does not require preserving the machinery used in the past. What is needed is to produce the same microstructures and surfaces that have already been qualified by previous testing and analysis. One (but perhaps not the only) way to realize this is to follow the same casting temperatures and cooling procedures used in the original manufacturing processes as well as using the cutting tool materials and feed rates of the past. It is likely that today's computer controlled metalworking machinery can be employed to replicate faithfully identical conditions to past manufacturing histories. Similarly, modern micromeasurement (gauging) techniques can be employed to compare the final dimensions and surface conditions of remanufactured pits and other SNM components to the original specifications. Consequently, metallurgy and metal machining skills and knowledge of first order are required, but the emphasis should be on quality control, not on innovation or cost savings.

At present the U.S. needs only a very limited SNM remanufacture capability -perhaps of the order of ten or so pits per year. Such a capacity could be expanded quickly by a factor of two or three by going to

multi-shift operations. On the time scale of a few years, capacity could be expanded to hundreds of units per year level by installing additional equipment and training additional workers. It seems best to locate the primary SNM remanufacturing capability at one site in the nuclear weapon complex, although some SNM casting/machining/finishing capability may be retained for special purposes at additional sites.

Unlike other parts of the stewardship program, the SNM manufacturing component does not lend itself to a science-based treatment in which opportunities are created for individuals within the nuclear weapons complex to engage in (unclassified) research in areas that are akin to those that are associated with specific issues on weapons technology. Having an open research program on the physics and metallurgy of uranium and plutonium is highly undesirable from the perspective of nuclear nonproliferation. Consequently, we see the SNM manufacturing component of the stewardship program as a narrowly defined, sharply focused engineering and manufacturing curatorship program.

11 ADVANCED COMPUTING FOR STEWARDSHIP

11.1 Introduction

Computation has always been important to the development and understanding of nuclear weapons. It permits scientists to go beyond the basic physical principles underlying both the fission and fusion processes, which in themselves are understood, to understand how these principles actually express themselves in the complex operation of modern devices. These operations include, for example, the flows of reacting chemical species generally not in thermodynamic equilibrium, turbulent gases, shock waves, neutron fluxes, and various instabilities that need to be analyzed and understood or reliably modeled.

During the more than four decades of nuclear testing—during which the U.S. performed more than 1000 out of a world-wide total of approximately 2000 tests—the U.S. could use nuclear tests to work around inadequacies in physical understanding or computational resources. Empirical factors and phenomenological approximations were introduced that could be adjusted using data obtained from test diagnostics and scaled, or extrapolated as appropriate, from additional shots. As a result, today we have models of weapons behavior but no confident basis for anticipating changes in performance or failure modes over long periods of time due to material aging,

contamination, or imperfections. These models, and the existing computer codes based on them for describing the development of an explosion, generally contain several empirical factors and simplifications (to 2-dimensional or 1-dimensional approximations).

The U.S. now appears to be entering a post-cold-war era with no further underground testing, and with reduced numbers of warheads and fewer warhead types that are expected to remain in the stockpile for at least several decades. Under these circumstances there is a need for improved scientific understanding and better modeling of the nuclear warheads. This generates a requirement for more sophisticated, complex, and demanding computer programs, greater computational speeds, and higher memory capacity. Instead of test shots, our understanding will be based on computer simulations and analyses benchmarked against past data and new diagnostic information obtained from carefully designed above-ground and laboratory experiments.

The original bombs, starting with the Trinity test of the first plutonium implosion fission bomb, were designed successfully using much less computing power than today's bottom-of-the-line laptops. However, accurate modeling of modern two-stage designs that achieve very high (limiting) yield-to-weight ratios in restrictive geometrical configurations pushes the limits of modern computing science. Irrespective of how the stockpile may evolve over the long term—perhaps with the reintroduction of already-tested weapons that are less sophisticated and more robust in design, with lower yield-to-weight performance—for the present the U.S. requirement is for responsible stew-

ardship of what we have already deployed, particularly to retain confidence in the performance and safety of the nuclear weapons as they age.

Several specific examples are useful to show the range of stewardship issues where advanced computation can play important roles. In order to maintain confidence in the performance of an aging stockpile without nuclear testing, models are needed to do full 3-dimensional calculations of how localized cracks, corrosion, or other chemical changes due to gas leakage or radioactive decays can affect performance. There is also a lot to be learned from sophisticated 3-dimensional calculations about the safety of modern devices if, as a result of an accident or unauthorized incursion, the high-explosive is detonated at one or more off-axis points. Due to limitations on computation power and bomb-modeling, the "state of the art" is still relatively primitive in the ability to model such phenomena in three dimensions, as opposed to reducing the analyses to two—or even one—dimension by geometric averaging. For such analyses there is a need for higher spatial resolution then presently achieved—i.e., grid sizes of the order of mils as opposed to millimeters—in order to model effects of interest.

Over time it may become desirable to introduce design changes in some components of the present stockpile—perhaps for safety, by replacing sensitive by insensitive high explosives, or to reduce the amount of tritium required for boosting. Most would agree that, today, we do not have the ability to introduce any such changes without proof-testing. It will require considerable computational analyses of both primaries and secondaries in order to develop even a limited capability for redesign of warheads without proof-

testing—short of returning to very primitive devices as in the first-generation weapons stockpile. Finally, the better the U.S. can reverse engineer and develop detailed understanding of warhead designs of would-be proliferators, the better we will be prepared to face the threat of, and possibly render-safe, any such threats should they occur.

In the sections which follow, we discuss trends in computer hardware, and their implications for the type of computer architectures likely to be available in the long term for Science Based Stockpile Stewardship. We then discuss which kinds of computational problems are likely to work efficiently on specific hardware architectures, and conclude with a brief section on software development and visualization tools.

Advanced computing was a relatively small part of this summer's study on Stockpile Stewardship; the summer study emphasized experimental rather than computational facilities. Thus our comments on computing are not based upon an extensive, in-depth study of specific modelling codes or detailed computer hardware needs. Rather they are intended to give an overview of the subject, and to highlight the main issues for the future.

11.2 Computer Hardware Trends

The following paragraphs discuss some issues in computing for stewardship centered around industry trends. Increased computer power has historically been matched (or even overmatched) by improvements in algorithms

and models. Thus computers 100 times more powerful seem to elicit algorithms another 100 times more powerful, for a gain of 10,000. One vexing issue is balancing resources between improving modeling and algorithms and getting experimental data.

The tenor of this section is that scientific supercomputing will less and less be able to outstrip the technology present in the broad market-based commercial world. The long-term (five to ten years) future of computing for stewardship will be learning to adapt machines built for commercial purposes. Thus if there are important SBSS computing needs which would not be adequately met using commercial off-the-shelf desktop computers, a deliberate effort will be needed to ensure that the supercomputer industry is able to meet these needs.

11.2.1 Computers

The historical trend for increases in core computing power (and chip component counts) has been about a factor of two every eighteen months, which is a factor of 10,000 every twenty years. The raw speed of the CPU is not particularly well measured by the clock rates, which are what are commonly reported. In addition, the useful speed of inexpensive computers is not well estimated by the raw CPU speed. In particular, although memory densities and prices lie nicely on the above exponential curve, memory speeds, for the most common memory chips, have increased relatively slowly. Thus, unless some design effort, and time and money, go into the cache and memory

subsystem, the CPU can spend a depressing amount of time waiting for the memory to deliver code or data. The problem is magnified in shared memory multiprocessors, where the extra cost per CPU over the cost of stand-alone workstations with the same CPUs can mostly be allocated to the memory subsystems. These are complicated by the logic to make sure all the CPUs see a consistent state of the memory despite the local caching. As an independent way of making the same point, approximately half the gates in some of the Cray multiprocessors were dedicated to controlling memory.

Thus, individual computers increase performance at the maximum rate, sustained by the enormous market for desktop machines. Networks of desktop machines, viewed as a computer system, increase their raw power at the same rate, but exploiting them is retarded by algorithmic difficulties. Many modern parallel machines consist of desktop CPU and memory chips connected by special communications networks. These machines will lag behind the power curve by the time required to design, or re-engineer, the communications networks for improved CPUs and memories. The engineering effort available to design these machines is constrained by the size of the market, which seems to be about one billion dollars per year. Intel's Paragon is an example of this effect. As of this writing it is the world's speed record holder, at about 140 Gflops. But the Paragon uses a CPU chip that Intel has deemphasized; follow-ons will have to move to the 486 family. Intel's supercomputer business brings in only about $100,000,000 per year, and they have decided to start trying to sell the machines to commercial customers. There are big customers for scientific computing, but together they are not big enough to support innovation at the rate needed to keep up with the

present rate of improvement in desktop equipment. The situation is just as bad for shared memory multi-processors, which are the easiest of the parallel machines to program. These are limited in the number of CPUs, 32 probably being an upper bound. As mentioned above, the memory systems are complicated, and much more tied to the vagaries of the CPUs than are other architectures.

Barring architectural breakthroughs (see below), the future of computing is with loosely connected desktop-class machines. (Or with whatever class of computers drives CPU development. There is no chance these will be intended for scientific computing.) Thus in the long run, algorithm development should look towards exploiting fast CPUs loosely connected by networks whenever this is possible. We discuss in subsection 11.3 what types of algorithms are most likely to be able to benefit from use of loosely connected desk-top machines.

11.2.2 Networks

The world of networking is about to undergo a revolution. For more than a decade, local area networks have been at ethernet speeds, with some nods towards expensive faster nets that connect to only a subset of the computers. The likely future is ATM networks at SONET speeds (155 megabits/second and up). Furthermore, these local networks ought to mesh seamlessly with campus-sized networks, and eventually with the networks of the long-distance carriers.

Widely available high speed networking will have two effects. First, it will be relatively easy to transfer large amounts of data between machines. Achievable bandwidths ought to be quite comparable with disk bandwidths for desktop machines. (Short messages, however, will likely still require milliseconds. Also, the speed of light determines a harsh lower bound on the latency of long distance communication, even if one can get away from the IP protocol.) High speed networks present an opportunity, even if it is not immediately clear how to exploit it. Second, the fact that network speeds are comparable to internal bus speeds is likely to change the architecture of computers in unforeseeable ways.

Thus, an important element of future computing will be with desktop class machines, loosely coupled by networks that are at least as fast as disks.

11.2.3 Storage

Disk sizes and costs per byte are improving rapidly. Disk reliabilities are increasing. More and more, commercial firms have on-line databases on the order of a terabyte. Much of this technology is immediately applicable to the concerns of stewardship. The parameter space for discussing storage is approximately five dimensional: total size of the data, size of the individual items to be stored, how fast the data have to be moved in or out of storage, how long they have to be kept, and how quickly they have to be retrieved. It is easy to pick performance points that are either unachievable, or that require serious misapplication of resources to achieve. On the other hand,

storage systems for stewardship are likely to be similar to those of large companies in requiring the assembly and tuning of off-the-shelf subsystems.

Effective use of large storage systems will require tradeoffs. Presumably the heaviest demand will be for storing checkpoints of long computations, including end results. The storing of high-resolution movies is more questionable. One need store no more than is appropriate to the use. For movies, the frame could be smoothed and compressed. Highly accurate still frames could be recomputed from the checkpoints. Since much of the change in desktop machines has been in their graphics capabilities, for many cases it might even be best to compute the movies on demand.

The situation with bulk storage (bigger and cheaper than disks and preferably permanent) is not very satisfactory. Either the writing speeds (as with most optical media) or the retrieval speeds (as with tape) tend to be very slow. This is a research area that might not be attracting enough commercial attention.

The future of computing is with desktop class machines, loosely coupled by fast networks, to each other and to an amount of disk about 10 or 100 times as large as the amount of main memory.

11.2.4 Potential for Advanced Architectures

From a technical viewpoint, the kind of machines described above are

unbalanced. The marginal cost of a CPU chip is not much different from the marginal cost of (about) 8 memory chips. That argues that computers ought to have a much larger ratio of CPU to memory than is common today. This might be feasible if one required programs to ensure a consistent view of memory, instead of placing the demand on hardware.

A more radical view of the same economics suggests that the way to overcome the imbalance between CPU and memory is to put computing power on memory chips. Such an architecture would allow fabulous peak rates, but would pose considerable algorithm challenges as well.

11.3 Types of Computations

Of the several types of physical computations relevant to nuclear weapons and Stewardship, most are well suited to some degree of parallelism with fairly obvious adaptations and extensions of present algorithms. However, the requirements for interprocessor communications bandwidth, memory architecture, and long-term storage depend upon the type of computation. Some problems are already suitable for networks of fast workstations. Others are not, unless ingenious new algorithms can be found.

Hydrodynamic problems in general, and the detonation of chemical high explosive in particular, require the solution of local partial differential equations (PDEs). One typically solves such problems by partitioning physical space into a discrete grid of cells or finite elements and approximating

the physical state with a few variables per cell. Most such calculations are presently two-dimensional, exploiting the axisymmetry of most weapons designs. In future, three-dimensional calculations will be required to model nonsymmetric imperfections caused by aging and the consequences of improper/nonsymmetric detonation. Adequate spatial resolution typically requires $\sim$ 100 cells in each dimension (although modelers would probably make good use of more cells if more powerful computers were available). Hence 3D calculations will require about 100 times more computer power than the present 2D ones.

"Explicit" computational schemes, which are most commonly used, advance the state of each cell from one time step to the next according to the state in the immediately neighboring cells. Such calculations can be parallelized by making each of p processors responsible for a spatial region containing n contiguous cells. (Depending on the computer architecture, n may not be the same for all processors, but for the sake of simplicity we shall ignore this.) At least once per time step, the states of the cells on the boundary between two such regions have to be communicated between processors. The number of boundary cells scales as $n^{(d-1)/d}$ in d spatial dimensions. The time T_{step} required per time step with $N = n \times p$ total cells can be estimated in terms of t_{comp}, the time needed for the computations within a single cell, and t_{comm}, the time to communicate the state of one boundary cell between processors, as

$$T_{step} \approx \frac{N}{p} t_{comp} + 4dp \left(\frac{N}{p}\right)^{(d-1)/d} t_{comm}. \qquad (11\text{-}1)$$

The first term on the right represents computations, and the second, communications; the numerical coefficient assumes a rectangular grid and accounts

for communication in both directions. To the extent that computation and communication can be carried out simultaneously, these terms are not strictly additive. Nevertheless, T_{step} will be dominated by the larger of the two terms and is therefore minimized when the two terms are about equal. Hence the optimal number of processors is

$$p \approx \begin{cases} \left(\frac{t_{comp}}{4dt_{comm}}\right)^{d/(d+1)} N^{1/(d+1)} & \text{in general,} \\ \left(\frac{t_{comp}}{12t_{comm}}\right)^{3/4} N^{1/4} & d = 3. \end{cases} \qquad (11\text{-}2)$$

Most present calculations assume axisymmetry, so that the computations are effectively two-dimensional ($d \to 2$). In the future, fully three-dimensional calculations will be required. The optimal number of processors then increases very slowly with the number of grid cells. The RAM required per workstation scales as $N/p \propto N^{3/4}$ for $p \gg 1$.

If present trends continue, high-end workstations with sustainable speeds ~ 1 Gflops (10^9 floating point operations per second) will be available in perhaps five years. These workstations will probably achieve these speeds by closely coupling several internal processors. A typical detonation code performs about 1000 floating-point operations per cell per time step, so $t_{comp} \sim 10^{-6}$sec. Assuming 10 double-precision state variables per cell and a workstation-to-workstation bandwidth of 155 Mbits per second (using an ATM network as described above), $t_{comm} \sim 4 \times 10^{-6}$sec. According to the formula above, therefore, a 3D detonation calculation with $N = 256^3 = 1.7 \times 10^7$ grid cells would then be most quickly performed with $p \approx 3.5$ workstations. Large three-dimensional hydrodynamics computations probably will not parallelize efficiently across networks of workstations. The effective computation rate of such a network will be limited by communications rather than the

speed of the individual machines.

Furthermore, the communications bottleneck will be much more severe for problems whose physics is iess local, as is neutron and high-energy photon transport. When the neutron mean-free-path is comparable to the size of the system, or to any scale over which the scattering medium varies significantly, diffusion equations do not describe the transport accurately One can approach the transport problem "deterministically" by solving a Boltzmann equation. In three dimensional problems without symmetry, this becomes an integro-differential equation in six independent variables, plus the time, and requires a prohibitive number of computational cells to resolve all dimensions adequately. Nondeterministic (Monte-Carlo) methods avoid the Boltzmann equation by directly simulating the random walks of individual particles. Momentum and position become dependent rather than independent variables, so memory requirements are much reduced. It is probably inefficient to parallelize by dividing the scattering medium into spatial subregions smaller than a mean-free-path. One can parallelize over particles instead, so that different processors follow the paths of distinct groups of particles. In this approach, each processor must have rapid access to the state of the scattering medium over the entire computation region, which is likely to be tabulated on a grid. Hence the amount of memory per processor must be large, or one must use a shared-memory architecture. Workstations of the near future may well have sufficiently large RAM. Provided that the particles have neglegible effect on the background, such Monte-Carlo algorithms should be very efficient on a workstation network, since very little communication between processors will be required. If the particles modify the background, however, then changes

over the entire grid must be communicated frequently across the network. In fact the amount of data to be communicated will be much larger than in purely hydrodynamic problems, so that it will probably be much more efficient to do the calculation on a single machine.

A third example of the kind of computations that will need to be done in support of the SBSS is the calculation of atomic structure and spectra of high-Z atoms. In the Configuration Interaction approach, one constructs and diagonalizes a large-matrix approximation to a multi-electron Hamiltonian with relativistic and quantum electrodynamics corrections. Most of the time is spent calculating the individual matrix elements, by performing quadratures on products of single-particle wavefunctions. These elements can be calculated entirely independently of one another. So this problem is well suited to workstation networks.

The three kinds of calculations we have just considered do not make an exhaustive list, but they are representative of the problems that SBSS theorists will want to solve. Some are "embarrassingly parallel:" calculations performed on a network of p workstations will be carried out in $1/p$ as much time as would be required on a single workstation. In most cases, unfortunately, the speedup obtained from applying several workstations to the problem instead of one will be much more modest, because most of the time will be spent communicating data across the network. Individual calculations of the latter sort, which probably include large 3D hydrodynamical problems, could be more efficiently and quickly performed on a large multiprocessor designed for high interprocessor bandwidth.

It is not reasonable, however, to assess the computational needs of SBSS by considering the time required by individual calculations of any type. Once his or her code is written and debugged, the user cares only about turnaround time: the time elapsed between submission of her calculation and the availability of the results. This time can be infinite if she has no machine powerful enough to do her calculation at all, but that depends more on memory and storage than on cpu speed per se. A fast supercomputer that must be shared with many other users may be less useful to the SBSS theorist than a much slower but adequate workstation of her own. Furthermore, even very large calculations are not done just once. They must be repeated for different input parameters, and these calculations may be done simultaneously on several workstations as quickly as they can be done serially on a single supercomputer.

The capability of high-end scientific workstations increases exponentially with time. Speeds of order a gigaflop and memory (RAM) measured in gigabytes will probably be available for less than $50K in constant dollars in perhaps five years. Such workstations, connected by the fastest affordable networks and supported by generous amounts of mass storage (disks etc.), may be able to perform most of the calculations required by SBSS, though probably not as quickly as one might like. We recommend that funds and human effort be put into fast networks for such machines, and not only into the hardware, but also into algorithms that minimize the communication required between workstations. Successful efforts in these directions will allow workstations to be used more efficiently in parallel for the solution of large problems.

11.4 Software Development and Visualization Tools

Computer software and its usage account for the lion's share of the expense related to advanced computing for nuclear weapons. For example, in the course of our summer study we were told of a specific recent weapons-related exercise in which less than a year of elapsed computer time was accumulated, as contrasted with more than 45 man-years of manpower devoted to using the software and analyzing the results. The bulk of the actual expense related to nuclear weapons computing goes into people and software. Thus it is worthwhile to consider how to use the latter most effectively for Stockpile Stewardship.

Large-scale computations of nuclear weapons design and performance have been in progress for many decades. There is available to today's weapons physicist an extensive library of design codes and related software, some developed in the recent past with the latest software engineering standards and tools, and some dating from many years ago when such standards were nonexistent and when programming languages were quite a bit more primitive than they are today.

It is clear from the discussion in the previous subsections that computer hardware architectures will continue to change, probably in the direction of more parallelism (either within one massively parallel supercomputer, or distributed among many networked workstations). A range of policy issues arises from the need for both old and new software to adapt to the evolving new hardware environment:

1. "Old Codes": An immense number of man-years are represented in the accumulated programming effort for existing nuclear design software. In the immediate future it will be neither possible nor desirable to re-program a majority of these codes into forms which are easily parallelized, or which conform to modern standards for self-documenting and modular software design. As a result, the SBSS program should prioritize which of its existing codes would benefit the most from being upgraded, and should develop a long-range plan for how to evolve its extensive existing software base toward the computer environment of the future. This should include plans for how to more fully document the contents and the functioning of the most important existing computer codes, so that future generations will be able to use them intelligently.

2. "New Codes": New and actively used computer codes should be written in a scalable manner, so that they can evolve gracefully to new computer architectures (such as massively parallel computers or networks of workstations).

3. Visualization and other tools for software interpretation: As noted above, the majority of the time and expense related to nuclear weapons computations lies in developing software, and in understanding the results of a given computation once it has been completed. It is important to make the latter more efficient. In the past the nuclear weapons laboratories have not led the way in the development and use of advanced visualization tools for large computations. Today the laboratories are realizing the importance of these tools, and are rapidly developing ex-

pertise in this area. However with the trend towards use of three dimensional computations in the future, advanced tools for visualization will become even more essential to understanding of the results of nuclear weapons-related computations. The SBSS program will need to become a leader in this rapidly developing area.

4. "Archive" of nuclear weapons knowledge: With the cessation of nuclear testing, there are several proposals for making a national archive of information from all the past nuclear tests. The need to preserve the historical record of accumulated wisdom as the practitioners of nuclear weapons design and engineering begin to retire is clearly a real one. But careful thought and analysis needs to be given to how to accomplish this. Very large archives and data bases have a tendency to become extremely expensive and unwieldy (see EOSDIS for an example of the latter). On the other hand, commercial data bases are becoming more and more capable and flexible. This area of the archiving of nuclear weapons knowledge needs careful thought, priortization, and review by external experts in the field, before DOE embarks on a large and expensive software effort.

11.5 Conclusions

The future of multimillion-dollar supercomputers is in doubt because of competition from fast workstations and because of weak commercial demand. Extremely powerful massively parallel supercomputers are technically

feasible, and they would be more efficient for some SBSS calculations than networked workstations, but the commercial market may not continue to produce such supercomputers without substantial government support. If massively parallel supercomputers are essential to SBSS, the Stewardship program and the National Labs should develop a plan to support and encourage the supercomputer market. We understand that the Labs have a program, the Accelerated Strategic Computing Initiative, to do just this. It would be useful to make a common front with supercomputer users in the commercial sector (e.g., aircraft manufacturers), intelligence agencies, and non-defense government agencies (e.g., the National Weather Service), to agree upon desired capabilities and perhaps architectures. A would-be supercomputer manufacturer is more likely to succeed if a single machine can be designed that meets the needs of many potential customers. As a small-scale research project, the Labs might investigate whether classified computing can be done securely on unclassified machines using sophisticated encryption. If this were possible, very powerful machines might be shared among agencies or companies with classified and unclassified missions.

Despite our doubts about the future of the supercomputer market, advanced computing will certainly be essential to the success of the SBSS, whatever the machines that are used to carry it out. Computing costs should be a significant part of the SBSS budget, whether the computing is done on a few massively parallel processors or on large numbers of networked stations. If there is to be science in Stewardship, then there must be a strong theory program, and given the complexity of the physics involved in nuclear weapons and inertial-confinement fusion, the theorist needs a powerful computer to

extract meaningful predictions from fundamental equations. Without theoretical interpretation, the data from exciting experimental programs such as the NIF will be of little use in understanding nuclear weapons and of little use to the larger scientific community. Computer resources should be planned and acquired not as ends in themselves, but as tools in a strong theory program. No amount of computer power will make up for a shortfall in human expertise and insight. Nevertheless, the availability of generous computer resources will help the Stewardship program to attract the best theoretical minds. Also important in this regard will be a continuing effort to assure that the open scientific community has access to all the advanced code work that is appropriate, consistent with the country's non-proliferation concerns.

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