National Nuclear Security Administration

OFFICE OF DEFENSE PROGRAMS

2015 Review of the Inertial Confinement Fusion and High Energy Density Science Portfolio: Volume I

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EXECUTIVE SUMMARY

In September 2012, at the conclusion of the National Ignition Campaign (NIC), the NNSA committed itself to conducting a review of the progress toward ignition three years later. NNSA called upon twenty subject matter experts to independently review and comprehensively assess the progress and program plans for the Inertial Confinement Fusion (ICF) and High Energy Density (HED) science portfolio within the Stockpile Stewardship Program (SSP). A review that included all major program elements of this portfolio had not been conducted in more than 15 years.

This effort covered three main topics:

1. *ICF and Ignition.* An assessment of the scientific hypotheses that guide the ICF Program, the prospects of achieving ignition with existing scientific facilities, and an evaluation of program balance between the three main ICF approaches.
2. *The ICF/HED Portfolio and Long-Term SSP Goals.* An assessment of the alignment of the ICF/HED portfolio with SSP requirements; the contribution to the SSP in the science portfolio in the near, medium, and long term; the scientific and programmatic progress and plans in the ICF Program to meet the goals of the SSP; and, the long-term requirements for "high-yield" capabilities at laboratory scale.
3. *Improving Scientific Foundations in ICF/HED Physics.* The identification of opportunities to improve the underlying physics and the impact of simulations, models, and codes; to increase the integrated rate of progress of ICF/HED programmatic deliverables through new experimental capabilities (including targets and diagnostics); and, to identify areas where partnerships with academia, industry, and other government partners may be strengthened to support these opportunities.

This report summarizes the reviewers' comments and includes a series of conclusions that were assembled by the Office of Inertial Confinement Fusion and the Office of Research and Development, the authors of this report.

An overview of the reviewer comments follows.

ICF and Ignition

- The ICF Program has achieved the milestones set forth in the "Path Forward" report published in 2012. In particular, the program has: (1) identified leading candidates for impediments to ignition on the National Ignition Facility (NIF); (2) nearly doubled the shot rate on the NIF; (3) improved diagnostics capabilities on all its key facilities; (4) made progress in laser-driven direct drive ICF efforts at the University of Rochester's Omega Laser Facility (Omega), and; (5) made progress in magnetically driven fusion at Sandia National Laboratories' Z Facility (Z).

- There are clear SSP drivers to study the properties of robust thermonuclear burning plasmas, to pursue multi-megajoule fusion yields (which requires ignition), and to ultimately pursue high yield.
- Barring an unforeseen technical breakthrough and given today's configuration of the NIF laser, achieving ignition on the NIF in the near term (one to two years) is unlikely and is uncertain over the next five years. Although performance of NIF ignition targets continues to improve and simultaneously making contributions to the SSP, currently there is no known configuration, specific target design, or approach that will guarantee ignition on the NIF.
- The ICF Program has identified and begun exploration of key hypotheses to explain gaps between calculated and measured performance of NIF implosions, however, the present approach is too broad and diverse, and needs better focus. Neither Z nor Omega were designed to achieve ignition, however, they both may be used along with the NIF to understand limitations in NNSA's understanding of physics of ICF implosions, particularly during hot-spot assembly and stagnation. While efforts are improving, there is currently no published "roadmap" to coordinate cross-platform activities.
- Collaboration between researchers and institutions has improved since the conclusion of the NIC. Priorities for further collaborations include: (1) transformative diagnostics, including spatially, spectrally, and temporally-resolved imaging and spectroscopic diagnostics to observe "stagnation" at low, medium, and high convergence; (2) obtaining cross-platform data for fundamental physics validation of models/codes while improving access to codes/models, where appropriate; (3) reviving development efforts for codes to model Laser-Plasma Interactions (LPI); (4) increasing the number of designers and experimentalists working on magnetically-driven implosions and laser-driven direct drive programs; and, (5) enhancing peer review by academia and other institutions.
- There are areas where program direction should be reassessed, including: (1) pursuing the study of long length-scale LPI using partial Polar Direct-Drive (PDD) configuration on the NIF versus pursuing PDD ignition; (2) revising the charter for the laboratory-staffed ICF Council, which is composed of laboratory researchers; and, (3) reviewing the balance of focused versus integrated experiments.

ICF/HED Portfolio and Long-Term SSP Goals

- The ICF Program is well aligned with the weapons program. The HED science portfolio has delivered important data to the SSP, demonstrated the validity of theoretical, computational, and experimental methods important to evaluating the safety, security, and reliability of the stockpile in HED regimes, and demonstrated the competence and credibility of technical staff to work in these regimes without additional nuclear weapons testing.

- There is a strategic plan that describes how research efforts will evolve over the next decade from radiation transport, to boost science, and eventually, to outputs and effects.
- Applications for fusion yields produced on existing platforms are under development. Higher yields (those approaching ~100 kilojoules) are needed for burn physics relevant experiments.
- The long-term requirements case for "high yield" has not been revisited in nearly twenty years. It is not clear how this case has evolved for enhancing predictive capabilities for nuclear weapons performance or for nuclear survivability qualification of components.
- While there are presently no clear drivers for new major (>$100 million) facility investments, support is needed for diagnostics and facility improvements over the next five years.

Scientific Foundations in ICF/HED Physics

- The United States (U.S.) leads the world in HED science. Internationally, a number of facilities are being developed that exceed some U.S. capabilities, such as the high intensity ultrashort pulse laser being developed at the ELI Facility in the Czech Republic, the existing FLASH X-ray Free Electron Laser (XFEL) at the DESY Facility in Hamburg, Germany, and the follow-on European XFEL, scheduled to come online in 2017, also at DESY.
- All HED capabilities (domestic and international) must be considered as NNSA defines the means by which it will execute SSP-related experiments.
- Cross-platform validation experiments (experiments to elucidate similar physics executed on different platforms such as Z and NIF, for example), are instrumental to advances in HED physics. These efforts should take priority.
- Special attention over the next five years should be given to developing a robust cadre of top researchers in key areas of atomic physics, spectroscopy, laser plasma instabilities, and low-energy nuclear physics. NNSA must shape its' academic programs to ensure resources are optimally deployed.

Reviewers were not asked to consider resource constraints when providing comments or recommendations. To affect all recommendations contained herein would exceed current budget profiles. The principal next step is for NNSA to identify specific resource requirements to prioritize these recommendations within existing budgets. This prioritization process will begin in FY 2016.

CONTENTS

Inertial Confinement Fusion (ICF) and High Energy Density (HED) science are core technical competencies within NNSA's Stockpile Stewardship Program (SSP). The overwhelming majority of the yield from a nuclear weapon is produced in the high energy density state with temperatures and pressures rivaling that of the sun. Understanding these fields is critical to ensuring current and future stockpiles are safe and reliable.

The ICF effort has the unique challenge of achieving fusion "ignition" and developing corresponding HED experimental platforms while, at the same time, regularly delivering short-term contributions to support stockpile Annual Assessments, Significant Finding Investigations, and stockpile modernization. In 2012, the ICF Program outlined its three-year path forward toward the development of an ignition capability and committed itself to conduct a review of the Program at its conclusion. What follows in the report fulfills that commitment and, in fact, expands its scope to encompass the full ICF/HED portfolio.

This review process led to the identification of nearly 40 recommendations that cover management, technical, and programmatic issues. These recommendations vary in scope and urgency. One area already identified as being of immediate importance for the ICF effort is the pursuit of advanced diagnostics that will enable the exploration of ICF implosions at higher levels of fidelity required to uncover and quantify important phenomena that lie beyond our present understanding. In the non-ICF HED portfolio, the immediate priority is the study of the boost process, which reaches temperatures and pressures that we are only now able to explore with recent advances at the National Ignition Facility, the Z Facility, and the Omega Laser.

While ignition remains a significant technical challenge, its pursuit and achievement remains important to the SSP into the foreseeable future. Accordingly, I have directed the Office of Inertial Confinement Fusion and the Office of Research and Development to review and implement the findings and recommendations as appropriate.

STEPHEN L. DAVIS, BRIG GENERAL, USAF
Acting Deputy Administrator for Defense Programs
National Nuclear Security Administration

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1 Motivation, Objectives, and Structure for the 2015 ICF/HED Portfolio Review

At the conclusion of the National Ignition Campaign (NIC) in September 2012, the Department of Energy's (DOE) National Nuclear Security Administration (NNSA) committed to conducting a comprehensive review in three years to assess the progress toward ignition – stating in the 2012 Path Forward report that *a program assessment will occur at the end of FY 2015*. As 2015 approached, it was recognized that the Inertial Confinement Fusion / High Energy Density (ICF/HED) physics portfolio would need to be more fully assessed. The NNSA assembled a group of 20 diverse technical subject matter experts and conducted the review between May and September of 2015. The review assessed past and current efforts, but particularly emphasized future plans and opportunities to strengthen the long-term health of the Stockpile Stewardship Program (SSP). Reviewers individually submitted observations, findings, and recommendations to NNSA. This report was written by the Office of Inertial Confinement Fusion (ICF) and Office of Research and Development (R&D) and was reviewed by the Office of Advanced Simulation and Computing (ASC). These three offices are within NNSA's Office of Research, Development, Test, and Evaluation (RDT&D). This review was not a Federal Advisory Committee Act activity.

Section 1 presents the motivation, objectives, and structure for the review. *Section 2* summarizes the evolution of the program since the conclusion of the NIC, as well as achievements and challenges that emerged during the NIC. *Section 3* addresses the major observations of the individual reviewers. The report concludes with *Section 4*, Next Steps. The Appendices can be found in Volume 2. *Appendix A* includes documentation associated with the review process. *Appendix B* contains the reviewers' reports as submitted to NNSA. *Appendix C* contains additional reference documents.

1.1 Report Authorization and Recipients

The audience for this report are federal and laboratory/site leadership and management within NNSA's Defense Programs, particularly those with equities in the ICF/HED portfolio. This includes the Office of Research, Development, Test, and Evaluation (NA-11), the Office of Stockpile Management (NA-12), and the Office of Major Modernization Programs (NA-19). Consideration was given to the interest of external stakeholders in the overarching conclusions of the review and the subsequent direction of the program.

1.2 Primary Objectives of the Review

Individual aspects of the ICF/HED portfolio have been extensively reviewed since the 1980s (see Appendix C.2). These past reviews have primarily focused on the ICF Program or on the NIF. Two unique features set this review apart:

1. The major facilities that achieve high energy density conditions are multi-mission. Therefore, any observation that may impact a facility must be evaluated in its full

mission context. This necessitated simultaneous evaluation of the ICF Program and the HED aspects of the R&D portfolio[1]. This type of all-encompassing review had not been conducted in 15 years[2].

2. The majority of the previous reviews of the ICF Program focused on achieving ignition on the NIF. This review included each ICF approach. The result was a comprehensive review that included all major program elements that comprise the ICF Program.

The review was an independent technical assessment of the program of record as of May 2015. NNSA asked reviewers to provide their individual recommendations to improve current efforts, strengthen the three- to six-year program plans, and perhaps most importantly, identify areas for sound strategic investments over the next 10 to 20 years. The charge to the reviewers is provided in Appendix A.3.

1.3 Structure of the Review

The review was initiated on May 18, 2015 with reviewers attending a three-day overview of the program at which the ICF/HED leadership from the laboratories presented their respective programs. Reviewers were divided into three groups along the elements of the charge. In July 2015, the reviewers had briefings at Lawrence Livermore National Laboratory (LLNL), Sandia National Laboratories (SNL), Los Alamos National Laboratory (LANL), the University of Rochester's Laboratory for Laser Energetics (LLE), the SLAC National Accelerator Laboratory (SLAC), and at DOE in Washington D.C. The laboratories which participate in the ICF Program generated fifteen white papers to prepare reviewers for their visits. Reviewers held discussions with laboratory leadership and management, attended presentations by laboratory staff scientists, and met with established laboratory scientists and early-career scientists for further discussions. Additional details of the review process can be found in Appendix A.

Group 1 – Progress Toward Ignition

Group 1 assessed the potential for achieving ignition through existing scientific capabilities and facilities. The scientific hypotheses that guide today's ICF Program were evaluated across the three established ICF approaches: Laser-driven Indirect Drive (LID), Laser-driven Direct Drive (LDD), and Magnetically-driven Direct Drive (MDD). This group assessed the effectiveness of the ICF Program's cross-platform and cross-laboratory collaborations.

Federal Lead: Lois Buitano, NNSA
Meeting locations: LLNL, LLE, SNL

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[1] The HED aspects of the R&D portfolio are categorized into four areas: Nuclear (materials properties, hydrodynamics), Thermonuclear (mix, burn), Radiation (radiation transport and opacities), and Output and Effects (weapons effects, generating hostile/survivability environments). Over the last five years, significant progress has been made to improve NNSA's understanding of energy balance, boost initial conditions, and secondary performance.

[2] High-Energy-Density Physics Study Report, 2001, National Nuclear Security Administration

Reviewers: Jerry Chittenden, Imperial College
Siegfried Glenzer, SLAC
Jim Hammer, LLNL
Nelson Hoffman, LANL
Warren Mori, University of California, Los Angeles
Andrew Randewich, Atomic Weapons Establishment
Sean Regan, LLE
Bob Rosner, University of Chicago
Susan Seestrom, LANL, Retired
Steve Slutz, SNL

Group 2 – Non-Ignition HED Science and Long-Term Planning

Group 2 assessed current and future HED contributions to the SSP, and evaluated the long-term requirements for the ICF Program including the requirements for a "high-yield" fusion platform.

Federal Lead: Njema Frazier, NNSA
Meeting locations: LLNL, SNL, LANL, DOE-HQ

Reviewers: David Crandall, NNSA, Retired
Jill Dahlburg, Naval Research Laboratory (NRL)
John Harvey, U.S. Department of Defense (DoD), Retired
Jeffrey Quintenz, NNSA, Retired

Group 3 – Scientific Foundations

Group 3 examined the fundamental science of the ICF Program and progress made in understanding the physics relevant to ICF/HED sciences: material equations of state, hydrodynamics, thermonuclear burn, opacity, and radiation transport. Group 3 focused on the ICF Program's partnerships with external organizations in these areas. The group assessed the fundamental science experiments currently being executed and the status and contributions of university programs. Lastly, Group 3 assessed current diagnostics and computational modeling capabilities.

Federal Lead: Kirk Levedahl, NNSA
Meeting locations: LLNL, SLAC

Reviewers: Sean Finnegan, Office of Fusion Energy Sciences, DOE
Yogi Gupta, Washington State University
Stephanie Hansen, SNL
Richard (Dick) Lee, University of California, Berkeley
John Sarrao, LANL
George Zimmerman, LLNL

2 Evolution of the National ICF/HED Program since the National Ignition Campaign

The NIC was an integrated national effort consisting of partnerships between national and international labs, academia, and industrial partners to achieve ignition and robust thermonuclear burn on the NIF by the end of FY 2012. During the NIC, 84 of its 86 level one and level two milestones were completed. The two milestones not achieved were the demonstration of limited "alpha heating" and demonstration of ignition. Although the world's most powerful laser, NIF, was constructed and successfully transitioned to routine operations, ignition on the NIF was not achieved by the end of the NIC.

As the NIC was concluding, a workshop was held in May 2012, to "discuss science that had been learned during the NIC, identify new science questions that had arisen, and begin to lay the lines of experimental and theoretical inquiry that could address these over a multi-year time frame."[3] The workshop identified six Priority Research Directions (PRDs) that address key physics issues preventing the attainment of ignition on the NIF.

In December 2012, the NNSA and ICF Program scientific and technical community partners submitted to Congress the, "Path Forward to Achieving Ignition" Report. It proposed a path forward and ICF Program goals for achieving ignition on the NIF and improving understanding of relevant physics to be explored at the other major ICF/HED facilities (Z at Sandia National Laboratories, Omega at University of Rochester) over the three years following the conclusion of NIC. The report presented specific programmatic and technical goals to be pursued at each of the facilities: the LID Program predominantly conducted at the NIF; the LDD Program predominantly conducted at Omega, but with elements on the NIF; and, the MDD Program predominately conducted at Z. These goals became level two milestones for the ICF Program and were accomplished in the 2012-2015 timeframe. A summary of the milestones is provided in Appendix C.3. In addition, a summary of major accomplishments over that timeframe in the ICF/HED portfolio that were not specifically part of the "Path Forward" is provided in Appendix C.4.

[3] "Science of Fusion Ignition on NIF Workshop," May 22-24, 2012, LLNL-TR-570412

3 Review Topics

Each of the 20 reviewers submitted individual written reports that are available in Appendix B. While many reviewers addressed the charge given to their assigned group, NNSA encouraged reviewers to provide comments on all aspects of the program. Upon review of the individual reviewer inputs, the authors of this report sorted the reviewer's comments into the following topics:

- ICF/HED Contributions to the SSP
- The Prospects for Achieving Ignition
- Technical Challenges in Inertial Confinement Fusion
- Experimental Diagnostics and Computational Resources
- Improving Scientific Foundations in HED
- Academic Programs and External Partners
- Program Direction

The sections of the report are organized using this structure, with each topic beginning with background, followed by a segment that captures the major themes contained in the reviewer comments, and closing with a summary of the NNSA program office perspective and next steps.

3.1 ICF/HED Contributions to the Stockpile Stewardship Program

3.1.1 Summary of Reviewer Comments

With the cessation of underground testing in 1992, the U.S. nuclear weapons program could no longer directly develop and exercise the expertise of nuclear weapon scientists and the broader nuclear security enterprise (full scale manufacturing, engineering, production, etc.) through nuclear explosive tests. Established in 1994, the Stockpile Stewardship Program (SSP) was created to maintain confidence in the stockpile and sustain the nuclear deterrent in the absence of nuclear explosive testing. The SSP relies heavily on the NNSA laboratories to maintain expertise in technical areas relevant to nuclear weapons design and performance through leading-edge, science-based programs, thereby providing confidence that the United States has a safe, secure, and effective nuclear weapons stockpile. As captured in the January 20, 2015 laboratory directors' letter to the NNSA Administrator, found in Appendix C.1, "HED science remains a core technical competency for the Nation's Stockpile Stewardship Program for the foreseeable future." In particular, the "pursuit of fusion yield in the laboratory is critical for the long-term health of the Stockpile Stewardship Program." The scientific grand challenge of achieving ignition at laboratory scale attracts top scientists from around the world to the weapons laboratories. It is also recognized that the study of thermonuclear burning plasmas is important to develop and validate computational models that are used for the annual assessment of the stockpile and to resolve issues encountered during weapon surveillance.

Experimental platforms specifically developed for ICF applications have been adapted and applied to mature predictive capabilities for studying material properties, opacity and transport, hydrodynamics and burn, and outputs and effects. There is significant overlap in the skills associated with conducting complex and highly-integrated experiments in the ICF Program and those needed to conduct a nuclear explosive test. Specific skills include the ability to conduct diagnostic development and manage many different interfaces through design, fielding, and analysis. This expertise is important because one goal of the SSP is to maintain the intellectual acuity of the designers, scientists, and engineers who must remain cognizant of nuclear weapons design, development, and operation. SSP scientists, engineers, and designers rely heavily on modeling and simulation. Models and the overall simulation approach must be validated through experimentation. Predicting the results of an experiment, then conducting that work and analyzing the results and confirming or rejecting the related hypotheses and assumptions is an important learning experience: it is key to establishing experimentally validated confidence in those models and simulations, and understanding their limitations. Now, 20 years later, the ICF/HED facilities are the critical tool for providing confidence in the codes and their limitations in the high-energy regime.

NNSA and its laboratories value the ability to conduct cutting-edge research to attract and retain new employees while also advancing HED science that is critical to the nuclear weapons program. The NNSA laboratories embrace ICF/HED capabilities to test and train the next generation of stockpile stewards. LLNL and SNL are more pro-active in using the ICF/HED facilities in training their stockpile stewards. Since LANL lacks its own major HED facility, young designers at LANL should be incentivized to carry out experiments at the NIF, Omega, and Z facilities as part of their training in nuclear design.

By designing and executing experiments, scientists can experience elements of the design process from hypothesis, to experiment, through complex data interpretation and analysis. This enables development of validated understanding and design in the HED regime that is applicable to many NNSA mission areas.

Recent advances in HED science testify to the scientific and technology value that the ICF/HED portfolio is providing to the SSP. Contributions to SPP include providing equation of state (EOS) materials data and observing the lattice structure of plutonium under dynamic conditions at Z and NIF; resolving “energy balance” through experiments at the HED facilities; improving opacity models and equations of state of other materials of relevance to nuclear weapons; developing x-ray and neutron sources to test electronic components and shock reentry body/vehicle materials; and, significantly improving the understanding of radiation/hydrodynamic instabilities, an area that is very difficult to probe experimentally.

Experimental platforms have been developed for the NIF that achieve fusion yields greater than 10 kilojoules (10^4 joules). Studies of thermonuclear burn physics become possible as yields increase to the ~100 kilojoule regime. Exploring burn physics in support of the boost science effort is the primary focus for applications of yield over the next decade.[4] A multi-megajoule capability (>> 1 megajoule) would be used to extensively study burn physics and to develop an intense radiation source with an appropriate spectrum to support precise assessments of nuclear survivability and vulnerability and to validate nuclear weapons effects codes.

While the pursuit of ignition is valuable on many levels, significant challenges remain for the attainment of "high-yield" laboratory fusion. The pursuit of high yield will test the innovation of designers in ways that few other technical pursuits can. Higher yields enable experiments to test the validity of current nuclear weapon codes in temperature, pressure, and density regimes closer to nuclear weapons operating conditions, serving as a key means[5] to train the new generation of nuclear weapons scientists and engineers who have no experience preparing, fielding, or observing an actual nuclear explosive test. Although there is an inadequate technical basis today for thinking that high yield from laboratory inertial fusion can be obtained on existing facilities, the ultimate goal of high yield provides direction and shapes program decisions many years in advance of the perceived need. Assessing the need for high-yield capabilities at laboratory scale should be a long-term goal of the ICF/HED Program.

Guarding against technological surprise is another significant driver for the ICF/HED Program. Given the unique capabilities and the role of the ICF/HED Program in the SSP, continuing and broadening DOD and Congressional support for the program through improved communication is vital to the strength of the SSP and to ensuring national security.

3.1.2 NNSA Program Office Perspective and Items for Future Prioritization & Action

- NNSA will conduct a gap analysis to determine the ways the ICF/HED experimental program may be better developed to test weapon's designer skills and judgement. Additionally, reviewers commented that better intra-laboratory integration may be a welcomed step in this direction. For example, LANL should build upon recent successes to improving integration between the HED physics team and the Theoretical Design Division (XTD).
- The three laboratories must strengthen the integration of ICF/HED capabilities (particularly NIF and Z) with the weapons effects and hostile environment communities. Future Live Extension Programs for stockpiled weapons will inevitably have components that will need to be certified for evolving Stockpile-to-Target-Sequence (STS)

[4] Ten-year National HED Strategic Plan, January 30, 2015, COPD-2015-0003, LA-CP-15-00064

[5] In addition, for example, to sub-critical experiments executed at the Nevada National Security Site (NNSS) and hydrodynamic experiments executed at the Dual-Axis Radiographic Hydrodynamic Test (DARHT) Facility at LANL.

requirements. NNSA will pursue an effort to assess the near term and long term requirements for ICF/HED capabilities in the areas of outputs, environments, and effects.

- Working with laboratory leadership, NNSA will explore how the ICF/HED portfolio can be better balanced to avoid technological and geopolitical surprise over the long term.

3.2 The Prospects for Achieving Ignition

3.2.1 Summary of Reviewer Comments

Barring an unforeseen technical breakthrough and given today's configuration of the NIF laser, achieving ignition on the NIF in the near term (one to two years) is unlikely and uncertain in the mid-term (five years). The focus of the LID Program over the next five years should be on the efficacy of NIF for ignition. The question is *if* the NIF will be able to reach ignition in its current configuration and not *when* it will occur. The focus of integrated experiments in the LID Program should not be on high-gain capsules simply because codes and models predict they will perform well. The codes and models themselves are not capturing the necessary physics to make such predictions with confidence. A lack of appreciation for this combined with a failed approach to scientific program management, led to the failures in the NIC.

There are areas of physics that are not well understood or not properly captured in models, codes, and current simulation approaches. Therefore, it is important to probe high energy density states and the systems that create HED conditions as a function of energy density and changes in that energy density. This requires a systematic experimental program to explore factors that impact ICF implosions, and novel ways to measure the physics of the drive conditions (laser-target interactions) and the implosion characteristics throughout the time of the implosion.

Despite the failure to achieve ignition during the NIC, there are clear SSP drivers to study the properties of robust thermonuclear burning plasmas, to pursue multi-megajoule fusion yields, and to ultimately pursue high yield. This places significant onus on the NNSA, the laboratories, and the sites to take a different approach to ensure that the significant technical challenges to laboratory ICF will be met with the best possible science, within fiscal constraints.

Recent program management changes discussed in section 3.7, and sound scientific and structural groundwork, increase the odds for achieving ignition at the NIF and multi-megajoule fusion yield on a potential future laboratory driver. Nationally, a reorganization of the ICF Program has been implemented and is highly effective; providing capable leadership, greater functionality, and better alignment of the ICF Program with the broader weapons program. The new research paradigm of the ICF Program is not an open-ended scientific program or an exercise in systems engineering; but is a balance of integrated experiments, ignition science that pursues focused experiments, and physics integration.

Although buoyed by the contributions of ignition-driven and high-energy density experiments, the SSP must be prepared for the possibility that there is no existing experimental driver that will achieve ignition. Due to this uncertainty and the challenge in achieving ignition at laboratory scale, approaches to ignition in LDD and in MDD systems must be strengthened. The Z and Omega Facilities (which are not designed to achieve ignition), along with the NIF, can study the physics of the assembly and "stagnation" of thermonuclear burning plasmas, which benefits the development of all ICF approaches. The National Implosion Stagnation Physics Working Group (NISP) is in the process of identifying specific ways to do this. A summary from this Working Group's first meeting is located in Appendix C.5.

Ignition is an important step toward multi-megajoule fusion yield, not an end in itself. ICF Programs in Russia and China are pursuing platforms that may surpass current U.S. capabilities. High yield must remain a long-term goal for the ICF Program, even if ignition is not reached on the NIF. In an extended era without nuclear explosive testing, driving towards a fusion source of 500 megajoules or greater will be essential for the health of the program.

Scientific exploration the efficacy of the NIF for ignition is an important endeavor for the SSP and for broader scientific community in the United States. If the NIF achieves ignition, applications of fusion yield would be of immediate relevance to the SSP.[6] If it does not achieve ignition, the reasons for this must be understood and each major ICF facility play a could role in developing this understanding. The following sections detail the technical challenges facing each approach to ignition and the technical challenges they share.

3.3 Technical Challenges in Inertial Confinement Fusion

This section summarizes technical observations in the following areas:

- Laser-driven Indirect-Drive (LID), predominantly executed at the NIF
- Laser-driven Direct-Drive (LDD), predominantly executed at Omega
- Magnetically-driven Direct-Drive (MDD), predominantly executed at Z
- Shared Technical Challenges between LID, LDD, and MDD

3.3.1 Laser-Driven Indirect-Drive (LID)

3.3.1.1 Summary of Reviewer Comments

During the last three years on NIF, LID achieved hotspot densities and temperatures with lower convergence and higher adiabat capsule implosions, sufficient for about half of the total fusion yield to come from alpha particle plasma heating. Trends can be observed in these results, as the implosions have demonstrated better reproducibility than past implosions. Although the fusion yield is improved, it remains significantly lower than predicted by unperturbed (1-D)

[6] "Applications of Ignition 90-Day Study," February 29, 2012

calculations and a significant fraction of the laser energy (up to 200 kJ) remains unaccounted for in gas-filled hohlraums.

The volume of high quality published research resulting from experiments during the last three years is impressive. These articles have concentrated on the 'high-foot' platform, where scalar yield performance has more closely matched predictions. This platform – which uses the same capsule as the NIC point design with a larger laser prepulse (the "foot") – has achieved close to 10^{16} DT fusion neutrons (~26 kJ). This result is important and encouraging, because significant alpha heating is a critical first step toward ignition launching a nuclear burn wave followed by a rapid 10-fold increase of temperatures and thermonuclear burning of the surrounding dense fuel. The fusion community has recognized this as a significant achievement.

New NIF diagnostic capabilities, focused experiments, and the ability to simulate the multi-dimensional effects of perturbations have improved the ability to discern which factors are making the most significant contributions to performance degradation. Principal degradation sources are thought to be time-dependent drive asymmetry due to laser-plasma interactions and shell perturbations caused by capsule mounting features (commonly known as the "tent"). In addition, high convergence implosions suffer from mix, non-uniform fuel areal densities, and shell-break up.

Despite the success of the 'high-foot' design, the fusion yield remains significantly lower than predicted by unperturbed (1-D) calculations. Producing adequately symmetric implosions of indirect-drive ignition capsules has proven to be much more difficult than expected on the NIF. Laser Plasma Instabilities (LPI), such as Stimulated Raman Scattering (SRS) and Cross Beam Energy Transfer (CBET), are obstacles to creating the necessary time-dependent drive symmetry. Time-dependent drive multipliers are applied in simulations to the x-ray drive to match the trajectory of the imploding shell. There appears to be a correlation between the shape of the tent's contact with the capsule and the structure of the capsule observed in radiography images. Other contributing factors to reduced performance of the high-foot design include the fill tube, hot electron preheat, and inaccuracies in the equation of state of deuterium which impacts target design.

These are also major issues for the 'low-foot' design with a higher convergence ratio, wherein hydro instabilities and mix are known to be larger than in the 'high-foot' design. Low-adiabat implosions, known as low-foot implosions, show areal densities close to simulations and to those needed for high-fusion gain implosions. The experiments have shown low fusion yields, however, suggesting that the hot spot of the implosions is not forming adequately. Importantly, x-ray radiographs have shown evidence for shell perturbations caused by the capsule "tent" that holds the capsule in place inside the radiation cavity, i.e., the hohlraum.

The LID Program has stepped back from a singular focus on a monotonic increase of yield, but it has also become diverse. This has led to reviewers' concern that there is a slowing and dilution of progress due to pursuit of too many scientific paths at once. The number of experimental fronts pursued at the NIF grew at first but has recently decreased. This was viewed by reviewers as commendable, as focus is required for progress. Activities have been undertaken to ensure that the diversity of ideas is not lost; ingenuity and ideas are desirable even if they are ultimately discarded. Ideas that survive initial analysis may lead to short, targeted experimental campaigns on Omega or Z to determine feasibility before progressing to the NIF.

Predicting the physics of implosions through simulations is extremely challenging. While some aspects of the symmetry of imploded capsules is reproducible under small changes in initial and boundary conditions, computational capabilities for LPI are not yet fully predictive and hydrodynamics calculations have never been validated for the final stages of hot-spot assembly and fuel "stagnation."

It is unclear which path is more likely to eventually lead to ignition of the hot spot and cold fuel, and the odds of success. It is also unclear at this time whether this multi-platform approach is better than one that focuses on fewer options at a time, in greater depth. It can take five to ten experiments or shots to adequately study one concept on the NIF. Currently, there are only ~30 high-energy shots per year. Deciding which matrix of experimental campaigns to pursue is not simple and requires constant planning, technical peer-review, and some degree of flexibility.

3.3.1.1.1 Physics Issues Specific to LID

Incremental improvements in yield in LID have been achieved through an approach that circumvents problems, rather than by understanding and addressing them directly. While this has created a baseline for future design efforts, there are underlying physics issues that consistently emerge and that need to be addressed. Significant limitations to predictive capability remain. This means that the experimental exploration of parameter space is empirically-led or constrained to incremental departures from places of known performance. Investing in diagnostics and other efforts in this area could adequately constrain models, particularly hohlraum models.

Cross beam energy transfer (CBET) was one of the first problems encountered during early experiments on the NIF. There has been little attention given to assessing the time dependence of the radiation symmetry that is responsible for introducing swings in the capsule shape during implosion. It may be possible to use different pulse shaping on the inner and outer beams to provide some time-dependent control of CBET, to design a shimmed capsule with a graded ablator, or to vary dopant thickness to mitigate swings in capsule shape during the implosion. It is also possible that gas-filled hohlraums will provide the only path to ignition on the NIF and need to be understood.

The low- and high-foot campaigns experienced significant SRS from the inner beams. In fact, at least 20 percent of the energy was reflected after including the CBET. Perhaps more importantly, when comparing 15 shots with the same nominal target and laser conditions, there were variations of 15 to 20 percent in the back scatter energy. In addition, there continues to be variation in the amount of light absorbed or rescattered as it reflects back to the laser entrance hole.

The lack of control over the time dependence of the CBET within the gas-filled hohlraums has led to the development of alternative hohlraum designs with lower gas fill pressures. The reduction of the tamping effect of the gas introduces a new set of challenges and requires accurate modeling of the plasma expanding from the hohlraum wall, and modeling of the collision of plasma expansion with the blow off from the capsule. These issues can be mitigated by the use of denser ablator materials such as high density carbon or beryllium. These would require a shorter radiation drive pulse and allow the laser energy to couple to the hohlraum before it is filled by high density blow-off plasma.

A significant number of limitations remain that hinder predictive capability and inevitably mean that the experimental exploration of parameter space is constrained to incremental departures from a place of known performance. With the perturbation amplitudes apparent in current experiments, the stagnation process is intrinsically three-dimensional. In places where discrepancies lie between experimental observation and 3-D simulation, it is unclear if these are due to deficiencies in the way in which the hotspot is modeled or if the discrepancies arise before the start of the deceleration phase. Simulations of the emitted neutron spectra are an important predictor for whether or not key indicators of the hotspot temperature and velocity are observable. Anisotropy of the neutron spectrum is a clear indication of a net center of mass velocity in the hotspot. This is indicative of a low mode asymmetric implosion. Differences between the DD and DT ion temperatures inferred from neutron spectra indicate that the calculated spatial temperature distribution may be incorrect.

3.3.1.1.2 The Future LID Program

The LID research program is pursuing integrated experiments, focused experiments to understand the ignition science, and a physics integration effort with codes and models. This approach will explore many different ideas and iterate on multiple platforms such as:

- Pushing 'high-foot' designs toward ignition through different gas fill, ablators, hohlraum sizes and shapes, walls and drive profiles,

- Lowering convergence ratios further and pushing to higher velocities and larger hot spots so that the hot spot itself has enough mass to provide greater than 100 kJ yield, and
- Increasing the laser energy.

Within each focus area, the goal is to find an experimental platform for which there is agreement with 1-D calculations and to use this as a jumping off point and gradually push toward higher yield and ignition. The hohlraum/capsule configuration should be modified to improve symmetry without the need for CBET. This will require larger hohlraums with a reduced gas fill density. Larger hohlraums require more energy to maintain a given radiation drive temperature. Some of this energy may be obtained through reduced LPI and backscatter, but it is probable that adequate symmetry will only be achieved at lower radiation drive temperatures.

The LID Program should emphasize hypothesis-driven focused experimental campaigns that are adjudicated through the interpretation of the data. It is important that experiments test the physics models used in the radiation-hydrodynamic codes. Understanding the target physics of a few focused areas is more important than executing an exhaustive experimental campaign of many permutations of ablator, capsule mount, and hohlraum gas fill.

Ideas for reducing the effect of the capsule support structure should be pursued, with the goal of identifying an improved alternative to the current tent. Many promising concepts for less intrusive support structures have been presented and should be investigated. Since high yield can be degraded by many effects, it is necessary to conduct these experiments under stringently optimized and reproducible conditions (e.g., with good ice surfaces and well controlled laser pulses). Engineering solutions designed to reduce perturbation levels can be directly evaluated through inflight radiographic diagnostics. The relative stability of the current best performing capsules means that the perturbation induced by the capsule mount will be at the limit of diagnostic resolution when the implosion is approaching the axis.

The pursuit of reduced convergence implosions is an important new feature of the program and should be given a high priority. The so-called ‘big-foot’ design increases hot spot rho-R at the expense of the cold fuel. The results will provide an important test of the new figure of merit replacing implosion velocity with capsule convergence. If validated, this result will have important consequences for future planning and will motivate fielding designs on the NIF to deliver yields approaching 100 kilojoules. One risk with this thinner ice-layer design is that mix at the fuel-ablator interface, previously undetected in earlier experiments, could expose higher Z material to the hot spot.

Beryllium (Be) and other alternate ablators must be tested with hohlraums and laser pulses optimized for them. It will be necessary to develop beryllium target designs in hohlraums that demonstrate the expected desirable features, in order to fully evaluate and benefit from the properties of beryllium that make it a potentially appealing ablator. This will require intensive computational design and experimental efforts. Possible directions include large low-temperature hohlraums optimized for capsule absorbed energy or drive symmetry, or higher temperature hohlraums with the capsule optimized for hydrodynamic stability. It must be ensured that these designs are optimized before ranking the ablator's performance relative to other optimized target designs using other ablators.

LANL is pursuing alternate designs including double-shells, wetted foams, and Be ablators. Double-shell capsules have two advantages over the single shell designs. The required radiation drive temperature in double-shell capsules is lower and the wall motion will be easier to control due to the short pulse length requirement. It is not clear if double shells will be less susceptible to drive asymmetries due to the overall high convergence and the fabrication of double shell targets is more complex. Target fabrication issues are presently impeding progress on wetted foam designs.

LANL's innovative designs are worth exploring, but are in need of a strategy. LLNL must work more closely together to define the roadmap and decision processes for these designs.

3.3.1.2 NNSA Program Office Perspective and Items for Future Prioritization & Action

- A specific effort to better understand all aspects of LPI, including CBET and SRS independent of each other and in combination, is needed to measure, model and predict the time-dependent drive symmetry in gas-filled hohlraums. NNSA will assess ways in which this may be accomplished with a special focus on engaging the broader scientific community.
- Mitigation of the effects of the tent is one obstacle to improved performance in LID implosions. LLNL should identify a tractable number of alternate capsule support structures, and the plans to experimentally assess those should be externally peer-reviewed. For planning purposes, the program of experiments to investigate capsule support features should conclude on approximately a twelve month horizon.
- A focused campaign with precision measurement of 1-D implosions, especially with large case-to-capsule ratio experiments, is a priority. This campaign should be integrated into fiscal year 2016 planning.
- Beryllium and other ablators or concepts must be evaluated using a hohlraum and laser pulse combination optimized for the ablator under investigation. A strategy, roadmap, and decision process for alternate ablators and designs should be developed.

- More resources should be dedicated to 3-D simulations using codes that are capable of resolving the physics they are meant to simulate. This would provide insights into the residual kinetic energy in the compressed shell, hot spot assembly, and stagnation phase of the implosion.
- An evaluation should be made to determine the optimum balance between high-energy, highly-integrated ICF experimental campaigns, and lower-energy, discovery experiments on the NIF.

3.3.2 Laser-Driven Direct-Drive (LDD)

3.3.2.1 Summary of Reviewer Comments

The LDD effort has demonstrated a series of precision cryogenic implosion experiments on the Omega laser with inferred hot spot pressures of ~50 gigabar (GB), and initial NIF experiments in the polar direct-drive configuration have begun. Since direct-drive ICF target designs couple more energy to the capsule than LID target designs, the required hot-spot pressure and convergence ratio is lower for LDD target designs (~150 GB hot spot pressure for LDD versus 350-400 GB for LID, and convergence ratios of less than 25 versus ~35 for LID). However, relaxing the plasma pressure requirements in the proposed way makes it harder to meet driver and experiment fielding requirements. Requirements on the laser, such as drive uniformity, laser colors, and power balance, and requirements on fielding experiments, such as a fast shroud retractor for the cryostat, target alignment, and vibration control are more stringent than for LID implosions. In addition, the Two Plasmon Decay (TPD) instability will need to be mitigated.

The LDD Program consists of two major components. The first is a program using a partial Polar Direct Drive (PDD) configuration at the NIF to investigate LPI and other laser-target physics. The second is a scientific study of Symmetric Direct Drive (SDD) implosions at Omega, where the goal is to demonstrate high pressures in the low volume Omega targets. The demonstration of greater than 100 GB pressures on Omega DT implosions would be a significant result; calculations using LLE's in-house codes suggest that performance may be extrapolated to NIF-scale implosions to produce ~100 kilojoule yields.

Simulations of higher convergence and lower adiabat implosions indicate that mix due to so-called target debris or capsule impurities is affecting inferred hot spot pressure. The LDD effort is actively investigating 3-D effects due to low-mode asymmetries induced by, e.g., laser power imbalance, target offsets, and beam miss-pointing effects. LLE uses an in-house code for the calculations of 3-D effects. No benchmark calculations, or comparisons with other hydrodynamic simulations or with experimental data are presently available.

3.3.2.1.1 Physics Issues Specific to LDD

A potential limitation for LDD is LPI at NIF scale-lengths. The LDD Program has made significant progress in understanding the effects of CBET and the TPD instabilities relevant to LDD. Currently, the predicted CBET for NIF-scale coronas in direct drive targets makes ignition impractical even with 1.8 MJ of laser energy using SDD without mitigation.

As the capsule implodes and becomes smaller than the laser spot sizes, there is increased overlap of the beams and an increased level of CBET. To counter this effect for SDD, LLE is developing 'zooming' phase plates on the five-year timescale. The present program of work using the PDD configurations on the NIF should instead concentrate on the use of an increased range of laser wavelengths as the approach to CBET mitigation. The beam zooming option is being explored on Omega and the wavelength detuning option is being explored on NIF.

TPD drives large-amplitude electron-plasma waves that cause hot electron preheat effects on the fuel, affecting compressibility and laser-target coupling. Mitigation of CBET could itself give rise to plasma conditions where the TPD instability generates significant hot electron preheat. Methods of reducing the impact of these effects will be addressed in focused experiments in the SDD configuration on Omega and in the PDD configuration on the NIF by introducing layers of intermediate Z material. It is important to adequately address the threshold and scaling for TPD with laser intensity, plasma-scale length, and for zoomed laser beams. The mid-Z layer is effective in raising the coronal plasma temperature that in turn will lead to increased Landau damping of plasma waves and consequently reduced hot electron preheat. The predicted increase in temperature has been observed with Thomson scattering. A complete assessment of mid-Z layers must analyze the effects on shock timing and possible generation of reverberating shock waves in the ablator and exacerbated hydrodynamic instabilities.

The LDD effort has benefitted from extensive experience and computational capabilities that support the modeling of CBET and TPD preheat, benchmarked against experiments on Omega. The density scale lengths are a factor of four larger in SDD on NIF compared to Omega. Predicting the behavior of LPIs in these plasmas will stretch the capabilities of these models. It is therefore important that data from PDD on NIF is obtained to validate models that may be used for extrapolation to SDD on the NIF.

Naval Research Laboratory (NRL) researchers have shown experimental results from Nike, a 2.5 kJ krypton fluoride laser located at NRL, of laser imprint reduction using thin gold overcoat layers on planar targets, as well as alternative laser beam smoothing schemes. NRL is currently extending their gold overcoat campaign to the Omega Laser System.

3.3.2.1.2 The Future LDD Program

3.3.2.1.2.1 Polar Direct Drive on the NIF

PDD experiments on NIF are an key component of the LDD Program, but are unlikely to lead to ignition. As such, this program should discontinue preparing the NIF for PDD implosions. For example, it is not clear that 48 quads of Smoothing by Spectral Dispersion (SSD) are required if PDD ignition attempts will not be pursued.

The principal aim of PDD experiments on the NIF is to provide a platform to test strategies for CBET mitigation on density scale lengths that are significantly larger than can be obtained on Omega and are within a factor of two of those that will ultimately be encountered in SDD experiments on NIF. The focus is on experiments and diagnostics leading to high fidelity tests of LPI physics (particularly CBET and TPD) at the correct scale lengths and plasma conditions relevant to ignition with SDD on NIF. The bulk of these could be planar and hemispherical experiments and include tests of high-Z overcoats or buried mid-Z layers as described in the program plan. Tests of imprint for ignition SDD conditions should be included. Smoothing on enough quads to enable high fidelity tests would be needed, but the deployment could be paced by experimental progress.

The LDD Program will need to employ and develop simulation tools that have been tested extensively against data. For example, for applications that will use the code HYDRA it will be important to further develop the code and to implement CBET ray tracing to make quantitative predictions. These tools should be tested against NIF experiments.

3.3.2.1.2.2 Symmetric Direct Drive on the NIF

The LDD strategy is based on the concept of demonstrating “hydro-equivalence” or assuming that hydrodynamics that lead to high inferred pressures on Omega at 60 kilojoules will scale to NIF implosions at 1.8 megajoules. The original papers on hydro-equivalence noted that there are many physics phenomena that will not scale. This includes CBET, LPI, and heat transport in the conduction zone, thermal conduction in the hot spot, and the mean free path to hot spot size for the equilibration of the deuterium and tritium ions.

The goal for SDD integrated DT cryogenic shots on Omega is the demonstration of an implosion that is hydrodynamically equivalent to a SDD implosion on the NIF at 1.8 MJ. Fuel pressures of about 120 GB will be needed for a direct drive ignition capsule on NIF to ignite. Similar pressures will have to be demonstrated on Omega. The plan is to increase the fuel pressure by mitigating CBET, using thicker shell capsules, and improving beam pointing (symmetry).

Proving the scientific case for investing in SDD on the NIF, and in particular, proving that the known issues such as CBET can be mitigated, represents a significant scientific challenge. This is particularly challenging in cases where not all of the physical conditions necessary for such a test can be accessed with existing facilities. It is inevitable that when scaling up a design to a larger platform, not all of the parameters ranges that will be encountered can be fully explored beforehand. It is therefore important that the data obtained in both PDD on NIF and SDD on Omega are utilized to inform and constrain theoretical and computational models that will be essential for underwriting the scientific case for SDD on the NIF.

3.3.2.2 NNSA Program Office Perspective and Items for Future Prioritization & Action

- NNSA will consider establishing a working group on hydro-equivalence with researchers from across the LDD, LID, and MDD efforts. The group should rank the areas of scientific concern with the hydro-equivalence argument, and decide what physics needs to be explored/added to the design codes.
- The LDD Program, with representatives from both LLE and LLNL, should develop a multi-year plan that describes the deliverables and milestones that would be required to technically justify a decision to convert the NIF to SDD illumination; in essence, develop a decision tree, including a time-scale for determining the cost and impact of this conversion.
- The NIF PDD experimental plan should focus on understanding the physics that does not scale hydrodynamically from Omega SDD experiments, primarily LPI.
- SDD implosions on Omega should be simulated using validated 3-D codes. Better integration is needed between LLE and LLNL in this area, which is discussed in section 3.4.2.
- Diagnostics to better quantify "mix" should be developed for Omega and experiments should be conducted to constrain simulations.
- Beam smoothing for LDD should be limited to a subset of NIF quads until/unless a decision is made to convert NIF to SDD. An assessment of the minimum quantity of beam smoothing to study LPI-related physics on the NIF is needed, to support decisions for potential future investments in SDD.

3.3.3 Magnetically-Driven Direct-Drive (MDD)

3.3.3.1 Summary of Reviewer Comments

The MDD approach provides an intriguing alternative to LID and LDD. Considerable progress has been made in the development of the Magnetized Liner Inertial Fusion (MagLIF) concept in the last few years. The achievement of fully integrated shots incorporating liner implosion, magnetization, and laser preheat represents a significant milestone. The MDD approach has lower implosion velocity, thick imploding shells, and lower required peak fuel pressure than the laser-driven approaches. There is a much smaller experimental and computational database and less is known about the potential issues. Similar to other inertial fusion concepts, the first fully integrated MagLIF experiments produced fusion yields significantly lower than those predicted by 2-D Magnetohydrodynamics (MHD) simulations.

The first MagLIF experiments at the Z Facility have reached DD fusion yields of ~4 x 10^{12} neutrons at temperatures of ~2.5 keV. It is thought that conditions suitable for 100 kilojoules of DT fusion yield with a pressure-time product ($P{\cdot}\tau$) of greater than 5 GB-ns and a magnetic field-radius product ($B{\cdot}r$) of greater than 0.5 MG-cm can be achieved on Z. DT fusion yield estimates are based on experimental demonstrations of DD equivalent yield; use of tritium on Z is not expected in the foreseeable future.

3.3.3.1.1 Physics Issues Specific to MDD

A number of mechanisms are thought to be inhibiting the fusion performance, based on experimental observations and 3-D MHD simulations. These include the non-uniformity and reduced efficiency of the laser energy absorption, hydrodynamic mix of the liner and fuel, mass loss through the Laser Entrance Hole (LEH), enhanced radial heat flow due to extended Ohm's law effects, and reduced convergence due to 3-D asymmetry at stagnation.

Much of the unpredictability of past experiments is explained by insufficient laser beam propagation in the target. In current experiments, the Z Beamlet (~2 kilojoule laser) with a target filled with D_2 fuel produce laser heating temperatures of 200 eV. Initial simulations of this process using LASNEX and HYDRA significantly over-predicted the fraction of laser energy that would penetrate the LEH foil and be deposited in the target. In addition to reducing the fraction of the beam that penetrates the LEH foil, LPI potentially causes the beam to filament and spray. Filaments that heat the electrodes or the liner could mix this material into the fuel and degrade the yield. This is supported by a recent experiment with beryllium electrodes that performed significantly better than a number of previous experiments that had used aluminum electrodes. An additional factor that complicates the modeling process is the presence of embedded magnetic fields. Collaborations have been formed between SNL, LLE, and LLNL to perform dedicated studies of the laser heating process at Omega and, soon, at the NIF.

A significant risk to the MagLIF concept is the mix of material, either liner, window, or dense DT fuel, into the hot fuel. The conventional wisdom is that at stagnation MagLIF is more prone to mix than laser-driven ICF because MagLIF designs have lower hot spot ρR than laser-driven ICF. This translates into a longer burn duration to generate enough fusion heating to ignite. Additionally, other poorly understood phenomena play crucial roles in the operation of a MagLIF target, including the implosion of a magnetized liner/plasma assembly undergoing magnetic flux loss, and magneto-hydrodynamic instabilities such as magnetic Rayleigh-Taylor and electro-thermal instability. The limited existing capability for experimental diagnostics and predictive simulations prevents sufficient understanding of target performance in these areas.

3.3.3.1.2 The Future MDD Program

The MDD Program is largely concentrated on evaluating a single computational design, primarily because experiments on Z occur at a lower repetition rate than laser experiments and the program has a limited number of shots. While many of the design aspects for MagLIF are constrained by the generator and available laser parameters, the main constraint appears to be operational as the present program has insufficient experimental opportunities and lacks availability to a sufficient number of designers and experimentalists to thoroughly evaluate more than one design. This is a cause for concern as there would be a limited selection of mature alternatives if current performance limitations ultimately prove insurmountable. Given the current constraints, it is not immediately clear how alternative designs that go beyond simple variations on a theme could grow from a nascent idea to a viable alternative.

As with the other inertial fusion approaches, it is extremely difficult to directly diagnose hotspot conditions. This is made even more difficult due to the large ρR of the liner surrounding the fuel at stagnation. Results from the NIF have shown that there is a wealth of information embedded within the neutron spectra. Progress on this has been made at SNL with measurements of primary DD spectra and secondary Triton reactions. However, the introduction of tritium handling capabilities at Sandia would mark a considerable improvement through increased yield and by introducing a range of new diagnostic options for assessing hotspot ion temperature, plasma motion, and beam-target contributions. The ability to add tritium or ^{3}He to the fusion fuel and measure the fusion gamma rays produced in DT or D^{3}He reactions would allow observation of the fusion reaction history in the implosion, placing constraints on model development.

The program could use more 3-D modeling to develop mitigations of instability features in the implosion. This would complement the fielding of improved diagnostics of axially resolved imaging, spectroscopy, and x-ray scattering to measure the conditions and allow for comparison with simulation data. Simulation tools and models (including reduced models) with magnetic fields will need to be developed and tested with focused experiments.

The MDD Program would benefit from the inclusion of LPI experts from across the complex to aid understanding of the laser plasma interactions of the preheat beam. Considering that the laser preheat is an integral part of the MagLIF research, SNL should consider hiring a post-doctoral researcher to develop in-house expertise for the laser preheat stage of the implosion.

The decades-long goal of the magnetically-driven liner fusion effort is to produce yields approaching a gigajoule. It is projected that this would require a driver with at least 130 megajoules of stored energy. The decision to turn away from the use of wire array Z-pinches for indirect drive experiments came as something of a surprise to some in the community as progress was being made using double-ended vacuum hohlraums and dynamic hohlraums. In retrospect however, this decision now seems logical as exploration of the X-ray driven indirect-drive concept is being pursued effectively at the NIF. In addition, more is known now about z-pinch-driven hohlraums than when SNL actively pursued indirect drive a decade ago.

There is an opportunity to explore alternative indirect drive designs with larger absorbed energies on a future larger-scale pulsed-power facility. As was identified in the mid-2000s, the main challenges of an MDD approach includes demonstrating enough pulse shape control to have the requisite reproducibility and drive symmetry. It is important that the scientific capability to resolve these issues be reestablished. This capability would enable a logical transition from LID to MDD in the future, should the SSP pursue "high-yield" fusion at laboratory scale.

3.3.3.2 NNSA Program Office Perspective and Items for Future Prioritization & Action

- The MDD Program's highest priority is to demonstrate laser beam propagation and heating on Z which must include collaborations with LPI and laser experts across the complex.
- A comprehensive diagnostic plan for characterizing plasma properties during MagLIF preheating and during implosion must be developed, with a focus on understanding stagnation.
- A second beam line would enable simultaneous laser preheating of the target and radiographic backlighting, providing extremely important diagnostic information from experiments. A cost and schedule estimate for the development of a second beam line on Z should be prepared for consideration.
- The ability to add tritium or ^{3}He to the fusion fuel and to measure the fusion gamma rays produced in DT and D-^{3}He reactions should be a high priority.
- Shot opportunities on Z should be increased. The MDD Program should dedicate more experiments for understanding and optimizing the power flow in the driver-target coupling, and understanding the scaling of MagLIF performance as a function of design parameters such as current, fuel preheat, magnetic field, fuel density, liner aspect ratio, and liner material over as large a range as possible at the Z Facility. There should also be more experiments that pursue alternative concepts to MagLIF.
- Additional ICF resources should be prioritized to the MDD effort to build a stronger cadre of designers, experimental physicists, and diagnosticians.

3.3.4 Shared Technical Challenges between LID, LDD, and MDD

3.3.4.1 Summary of Reviewer Comments

The ICF Program has traditionally been a 'driver–centric' research field. While the drivers themselves differ, the physical processes involved in achieving fusion through implosion are remarkably similar. It is refreshing to see the creation of working groups such as The National Diagnostic Group and the National Implosion Stagnation Physics Working Group (NISP) to advance the understanding of the physical processes common to all three ICF approaches. Measurement of and the creation of diagnostics for laser-plasma interactions, preheat, and compression and burn physics, are excellent areas for collaborations among the ICF Program elements.

All three ICF approaches must address laser plasma instabilities. LPI has been actively studied within the context of ICF for more than 40 years. The community has made some progress in its study of LPI, however, it needs improved understanding. These processes are being modeled with codes that are reduced models such as PF3D. There have been claims that these codes have been validated against experiment, but they need to be validated against codes with additional physics. While the assumptions might be reasonable at lower laser energy, they could be different at higher laser energy, and at different plasma temperatures, densities, temperatures and density scale lengths, and mixes of material. For example, none of these reduced models can include the effects of self-generated or imposed magnetic fields. Fully kinetic models such as Particle In Cell (PIC) codes have shown that the reflectivity from SRS is in short bursts, and can exceed unity for short times.

The LPI effort was a major driving force in the development of PIC codes. PIC codes are now widely used throughout the plasma physics community and are currently in limited use within the ICF effort. This recent precipitous reduction in the LPI effort is due largely to the inability of eliminating it and the hope that LPI issues could be engineered away. Unfortunately, LPI, including CBET, is arguably the biggest obstacle to high-yield designs. This philosophy has led to a significant decline in expertise on fully kinetic modeling of LPI at and outside the ICF laboratories, and has led to insufficient diagnostics for LPI on NIF.

There is the increasing realization that the stagnation phases of all three approaches are intrinsically 3-D processes. 3-D simulations could provide physical insights for many aspects of the implosion stagnation, especially in cases where there may be turbulence and where energy is flowing as a result of asymmetries. Importantly, experimental efforts focused on understanding physical processes are imperative for each approach. The NISP could help identify these specific areas.

3.3.4.2 NNSA Program Office Perspective and Items for Future Prioritization & Action

- The NISP should develop a comprehensive plan for using the NIF, Z, and Omega, and various computational capabilities, as a scientific tool set to advance fundamental understanding of the physics of the stagnation process and the state of the fuel and ablator near and at stagnation.

- A working group should be formed for LPI physics similar to the NISP in its structure and charge.

3.4 Experimental Diagnostics and Computational Resources

3.4.1 Diagnostics

3.4.1.1 Summary of Reviewer Comments

Adequate diagnostic instrumentation at NNSA's ICF facilities is needed to assess progress, develop theoretical understanding, and validate computational simulations. New diagnostics are often the driver for making new scientific discoveries and reducing uncertainty. The overall rate of scientific progress can often be directly linked to levels of diagnostic investment.

Previous experimental efforts under the NIC were frustrated by the inability to distinguish key differences between experiments. An improved diagnostic suite has enabled many of the advances since the NIC, in particular the 'high-foot' design described earlier. For example, new diagnostics have revealed structures that were not known to exist transforming the understanding of the structure of the plasma.[7]

The National Diagnostics Plan, first published in February 2015, was the result of inter-laboratory cooperation and presents a national strategy for the systematic improvement of diagnostics techniques across all ICF platforms. The plan is divided into three categories of diagnostics – transformative, broad, and local; and incorporates international scientific and engineering expertise to define the diagnostic development requirements for ICF research. The plan presents a reasonable timetable for instrument development and deployment, and identifies eight transformative diagnostics that will revolutionize the data obtained from current ICF facilities. In addition to benefitting ignition efforts, improved diagnostics will provide precision measurements for single physics experiments to improve codes and models for the broader HED portfolio. Diagnostics development is a fertile area for university collaboration, student training, and the recruitment of new staff. A technical working group established by NNSA monitors diagnostic development at every stage from concept, to analysis of alternatives, to scientific use. The scientific, engineering, and fabrication tasks of diagnostic development are divided among LLNL, LANL, SNL, LLE, NRL, and other partners, based on the efficient use of resources.

[7] Important diagnostic platforms now in place at the NIF include: re-emission balls, keyhole VISAR, 2DConA radiography, self-emission x-ray images, primary and down-scattered neutron images, $\Delta\rho$R from FNADS, and outgoing shock imaging. Besides these, other diagnostic platforms under development include foam balls, 5-axis keyhole, gated SXI, late-time 2DConA, early time self-emission, higher resolution imaging at stagnation including KBO (Kirkpatrick Baez Optic) and penumbral imaging, Compton radiography at stagnation, and co-aligned neutron and x-ray imaging.

While there are common needs across all facilities, the implosion geometry at Z provides unique challenges. The MDD Program has succeeded in delivering excellent data for compression and burn. Future improvements in temperature measurements with x-ray scattering and down-scatter from beryllium or deuterium are a priority. There is a need to develop further a diagnostic plan for the MDD effort to characterize plasma properties during preheating and implosion, with a focus on understanding mix.

There is a need to improve the understanding of LPI, making optical Thomson scattering instrumentation a high priority on the NIF. At the very least, two more Near Backscatter Imagers (NBIs) should be added on NIF, one at a new azimuthal angle and another at the opposite pole. Adding another Full Aperture Backscatter (FAB) diagnostic at one of these angles would be useful.

For all ignition approaches, the time between peak velocity of the shell and stagnation is key; when inflowing kinetic energy is converted to thermal energy of the hotspot and fuel, and the whole assembly is brought to its maximum density. Imaging diagnostics to measure hot spot formation and resulting residual kinetic energy, including imaging and spatially, spectrally, and temporally resolved spectroscopy, should be a high priority.

3.4.1.2 NNSA Program Office Perspective and Items for Future Prioritization & Action

- The implementation of the National Diagnostics Plan is a high priority. NNSA is placing emphasis on improving spatial, temporal, and spectral resolution for increasingly stringent tests of theory and simulations.
- Advanced diagnostics to address the needs for fundamental physics should be among the highest priorities. This includes, for example, the observation of the Doppler broadening from x-ray emission lines to produce velocity maps and accurate measurement of residual kinetic energy; the use of particle and x-ray scattering methods to measure the physical properties of dense matter (e.g., by observing Compton and plasmon features); and spatially, spectrally, and temporally resolved focusing spectroscopy.
- Measurements that must be pursued in a sustained and meaningful manner include: accurate P-V and temperature measurements spanning a large region of density-temperature space and measurements that can directly examine the microscopic structure of the HED states.

3.4.2 Computational Resources

3.4.2.1 Summary of Reviewer Comments

Ignition will not be achieved without multi-physics design codes that have sufficient predictive capability to guide complex, integrated physics experiments. More detailed physics will need to be included in codes and models as new concepts are investigated and proven, and as new experimental data – utilizing improved, transformative diagnostics – are acquired. Across LID, LDD, and MDD, there is an incomplete knowledge of the physics being included in various models/codes, the equations being solved, and the physics packages being utilized in specific calculations. Some codes have been developed with little or no external peer review. There is duplication of code and modeling efforts and impediments to accessing codes and computational resources by sites other than the primary site where the capability was developed or resides.

Considerable funds are spent developing ICF design codes. Code and modeling efforts should be coordinated across the laboratories and external partners should be included or considered as potential leads for these efforts. To the extent possible, codes should be available to all ICF researchers with a "need to know" and the proper clearance, both for simulation purposes and for code development. At a minimum, there should be a reduction in restrictions for code-use and source-code availability (at least among the ICF laboratories). This would increase the scrutiny on the constituent models and algorithms that comprise "the code," and create opportunities for interactions from outside the originating code development team. While integrated codes are likely to remain the domain of the labs in general, it would be valuable to promote university-led microphysics code development for the validation of physics packages in integrated ICF codes, perhaps through the Stewardship Science Academic Programs. Ultimately, the codes should not be considered the property of a particular laboratory or person.

A widely-held view is that a code has been validated once it provides agreement with an experiment. However codes involve complex and nonlinear couplings among choices of reduced physics models with fitting parameters and numerical approximations. Furthermore, each reduced model should be validated against meso- and/or micro-scale physics to have confidence in the results. Additional considerations include the range of applicability for the code (is the simulation being set-up, run, and analyzed properly for the application at hand) and the 3-D nature of the features observed in LID, LDD, and MDD experiments. The ICF Program would greatly benefit from routine use of 3-D simulations. These advanced validation efforts are complementary to the fielding of spatially, temporally, and spectrally resolved imaging, spectroscopy, and x-ray scattering diagnostics to measure the conditions and allow for comparison with higher-fidelity simulations.

Many experimentalists, as well as theorists, modelers, and designers, use the HYDRA code to calculate results. Therefore, it is important to further develop the HYDRA code and to implement CBET ray tracing to make quantitative predictions. A wide range of ICF-relevant physics packages are developed and implemented in HYDRA, and the code has been tested against a large database of integrated and focused experiments. The continued development of the code, particularly the inclusion of direct drive-relevant physics, and ensuring its suitability for use with high performance computing benefits the ICF Program and its' SSP mission.

There are many areas where the physics packages need to be further developed or better integrated into the codes. LPI physics is not adequately integrated into ICF codes. Kinetic effects, which are important to properly model hohlraums and may be important in MagLIF targets, are not sufficiently characterized. Particle In Cell (PIC) codes, now widely used throughout the plasma physics community, are in limited use within the ICF Program. Vlasov-Fokker-Planck (VFP) codes now include fully parallelized architectures that expand the distribution function into an arbitrary number of spherical harmonics with implicit field solvers that can use very large cell sizes. PIC and VFP codes can be used to test physics packages or be integrated into the hydro codes. PIC codes can now model more spatial and temporal scales and can run on 1,000,000+ cores and on GPUs and Intel® Xeon Phi™ processors, allowing for the study of some hydrodynamics on relevant scales.

The ICF Program should address the following areas: the relative immaturity of LDD-related physics in some ICF codes; duplication and inefficiency in integration of the Advanced Simulation and Computing (ASC) Program and ICF Program efforts at SNL; optimization of use-time for LLNL ASC resources between capability and capacity platforms; access by LLE and SNL to codes developed at LLNL; and, reinvigoration of LPI efforts. The ICF Program relies heavily on investments made by the ASC Program, so it is worth examining the challenges emerging as a result of constraints imposed by the pursuit of exascale computing and platforms. The move at LLNL to a new computer architecture for the next generation is, in general, a challenge for the SSP.

3.4.2.2 NNSA Program Office Perspective and Items for Future Prioritization & Action

- ICF codes and models have been largely developed in a compartmentalized manner. The ICF Program Office will conduct a deeper review of: (1) the prioritization of computing resources; (2) ways to eliminate historic site boundaries that impede progress, and; (3) opportunities to engage external/academic groups to lead or participate in computational efforts where appropriate. A set of workshops, similar to the successful MHD workshop in August 2015, are needed to evaluate the best path forward for code development, particularly for fully kinetic LPI codes.
- The ICF Program Office will work with the ASC Office to conduct an assessment of the impact to the ICF/HED Program of the transition to next generation computer architectures and the pursuit of exascale.

3.5 Improving Scientific Foundations in HED

3.5.1 Summary of Reviewer Comments

New experimental capabilities are providing opportunities to improve codes and models used to support NNSA's ignition and weapons physics efforts, and to improve scientific understanding of phenomena relevant to a broad range of fields such as laboratory astrophysics and high-pressure physics. The community is now acquiring enough systematic data to discriminate between physics models in regions of interest, an improvement from the prior reliance on single measurements.

Because the interpretation of HED experiments depends on radiation-hydrodynamic simulations, the ICF Program, in close coordination with the Office of Research and Development and the Office of Advanced Simulation & Computing, will need to integrate the best possible physics models into these codes. Plasma transport models must be able to treat mixtures of elements accurately, and models must be extended to include non-Local Thermodynamic Equilibrium (non-LTE) effects through tables or algorithms that can run on future high performance computing architectures. Models for materials behavior, opacity, and transport coefficients must be self-consistent.

In most ICF/HED experiments, the laser or pulsed power driver nonuniformities imprints onto the response of the target. The nature of that interaction needs to be fully characterized before the experiment can be completely understood. The disparate temporal and spatial scales associated with characterizing and understanding interactions of radiation with matter, particularly when compared to hydrodynamic scales, make the simulation of this problem intractable. As a result, this aspect of ICF/HED physics is often oversimplified or entirely ignored.

HED experiments are deeply connected to the method of energy delivery, and each driver has its own idiosyncratic energy delivery, native efficiency, and diagnostic challenges. HED results are best validated through comparing data from different platforms or drivers. This was highlighted in the early 2000s by the controversy over the equation of state for deuterium, as determined from data obtained at Z and Nova. Recent anomalous iron opacity measurements on Z will require validation by NIF experiments. Z and NIF are natural partners for cross-platform validation.

X-ray Free Electron Lasers (XFELs), such as the Linear Collider Light Source (LCLS) at SLAC, uniquely allow for decoupling volumetric heating from the probing of the plasma. For instance, data sets were successfully obtained for Al, Si, and Mg, due to XFEL emission at wavelengths that are not emitted thermally, even though the system is hot. XFELs provide the ability to obtain data on femto-second time-scales with a probe tunable to greater than 10 keV.

A strategy of tolerance or avoidance of deleterious instabilities was used during the NIC. This is understandable given the complexity of the problem. However, laser-plasma interactions and the assembly of hot spot at "stagnation" are fundamental to ICF. Dedicated experiments, new modeling tools, and theory must be used to address this challenging problem head-on.

To establish priorities for planning integrated and focused experiments, it is important to develop a simulation database that evaluates performance degradations of DT implosions due to possible errors and uncertainties in the microphysics. Such calculations might also aid the understanding of existing experimental databases. An important area of focus is bridging between atomistic microphysics and integrated hydrodynamics. Appropriately diagnosed and focused experiments can provide insight into continuum lowering and the ionization state of dense plasmas. This would allow the validation of calculations of physical properties such as conductivity, pressure, and ionization balance used in the radiation-hydrodynamic modeling of implosions.

3.5.1.1 Equation of State physics

NIF, Z, and Omega are producing conditions within materials that only exist in nuclear weapon explosions or at the cores of astrophysical objects. Recent experimental and theoretical results have shown that Thomas-Fermi modeling provides a poor approximation to the EOS of extremely dense materials. Better approximations can improve the fidelity of future ICF designs. Resolved measurements capable of distinguishing between theoretical models will advance the field toward understanding and, ultimately, the appropriate use of simulation tools for predictive capability. Complementary facilities such as the LCLS and APS are providing the opportunity to diagnose the transition of materials through phase changes, with a level of precision capable of distinguishing between advanced theoretical models. Researchers are taking full advantage of these new capabilities to generate experimental data to constrain EOS models in ICF/HED codes.

High pressure EOS studies are relevant to the study of the formation of planetary cores, creating opportunities to engage with researchers outside of the ICF/HED Program. It is clear that the development of high quality equations of state, self-consistent with structure and strength and implemented into global models with phase transitions accurately captured, will challenge researchers for decades to come.

HED experiments and modeling should explore the time-dependent phase transitions and the effects of departures from thermal equilibrium, such as unequal electron and ion temperatures. Most modeling assumes that pressure and energy can be specified as a function of temperature, density, and composition in equilibrium. A multiphase model has the potential to include time-dependent phase information, once supplemented with the appropriate transition rate data. Strength models are usually inconsistent with the EOS and do not provide for time dependency. Unequal electron and ion temperatures exist in HED experiments, but models assume that the pressure is separable into electron and ion components without considering correlation effects, such as screening effects of ion temperature on electron pressure. Current non-LTE radiative models required to simulate hohlraums and doped fuels provide EOS quantities that do not agree with equilibrium models. Dependency on simplistic concepts, such as degree of ionization, should be reduced in favor of more fundamental modeling approaches.

The scientific field of materials modeling relevant to EOS is progressing in an efficient manner. The codes being developed provide reasonably accurate results, in agreement with experiments conducted at several NNSA facilities; and EOS models along the Hugoniot, based on shock wave experimental data, are well in hand. Remaining challenges are the lack of accurate independent temperature measurements in high pressure experiments, and the limit in phase space explored as much of the data collected is from diamond anvil cell platforms or along the Hugoniot in shock wave experiments.

3.5.1.2 Opacity and Transport

Opacity and transport studies are foundational to the field of high energy density physics, and underpin the ability to model and predict ICF system performance. The availability of facilities such as NIF, Z, and LCLS and the development of high performance computing platforms are enabling scientists to improve the understanding of microphysics and atomic processes in extreme conditions.

Opacity models that are based on the assumption of Local Thermodynamic Equilibrium (LTE) agree with one another better than they agree with experimental data. For example, codes predict that the ionization state of carbon in the dense ablator plasma of an ICF implosion is close to two. However, advanced modeling and experiments suggest that the correct ionization state is four, which has consequences for opacity and heat transport. This illustrates the need for advanced models with detailed configuration accounting, non-LTE physics, and continuum lowering physics that are validated through experiments.

Measurements of emission and absorption non-LTE opacity are difficult, as are measurements of plasma transport coefficients such as thermal conductivity, electron-ion coupling, and stopping power in a uniform plasma of known temperature. Experiments designed to measure these properties are highly integrated, and are best suited to validating theoretical models. The recent measurements at the Z Facility of the opacity of iron indicate that the opacity is approximately twice the average calculated by the best models. If the Z data are correct, the approach to modeling opacity needs to be rethought.

The research community clearly understands the significant challenges and opportunities that exist to advancing the understanding of the physics of opacity. Researchers are making excellent use of multiple NNSA experimental facilities and DOE facilities such as the LCLS, to test theoretical models and predictive capability. Data from the Z Facility are in excellent agreement with models of some materials, such as nickel, and are in striking disagreement for other materials, such as iron. The discrepancies between observations and theoretical models are clear opportunities to advance NNSA's understanding.

As with opacity research, scientists studying transport properties are making excellent use of experimental facilities in NNSA and outside NNSA, such as LCLS and the Advanced Light Source (ALS) at Lawrence Berkeley National Laboratory. There is limited experimental data in ICF-relevant regimes, but this is changing; precision measurements are now providing data that is challenging the state-of-the-art microphysics modeling in codes such as DFT-MD and Purgatorio.

Accurate modeling of non-LTE population kinetics, which likely dominates the ionization state and distribution of atomic configurations in virtually all laser-plasma experiments, is an enormous challenge for simulation codes, as the number of levels can become intractable for high-Z plasmas. Benchmarking opacity calculations with experiments has been a low priority. There have been several attempts over the last 35 years to create data to benchmark radiative properties, such as line shapes, population kinetics, and collision physics. These efforts have been thwarted by difficulties with the plasma gradient structure; creating a uniform volumetrically heated sample to diagnose is challenging.

Magnetic fields affect electron thermal conduction, which then affects plasma density gradients and hydrodynamic instability growth rates. Routinely running 3-D simulations at adequate resolution with magnetic fields is resource intensive. Incorporating the possible need for transport modeling, in place of flux-limited diffusion, is a grand challenge. The validity of using a single fluid hydrodynamic model is questionable for simulating performance in low density hohlraums and exploding pusher targets.

The diminishing numbers of scientists trained in the fields of opacity and high temperature, high energy density atomic physics, and spectroscopy is a big concern. There is a severe shortage of young talent in opacity modeling at the national labs, and if left unaddressed, will erode NNSA's strength in this core competency.

3.5.1.3 Hydrodynamics and Burn

The importance of hydrodynamic and burn physics to the ICF/HED Program cannot be overstated, and the program's portfolio is unmatched in its breadth, depth, and standing. However, a systematic approach to enhanced predictive understanding appears to be lacking.

The program lacks sufficient diagnostics for the conditions of an imploding ICF capsule, and research is dependent on numerical modeling to characterize these conditions. Because outputs from numerical simulations are routinely used to infer the properties of imploded targets (such as the hot spot temperature used as an initial condition for inferring fundamental properties), it is important to be able to accurately diagnose the hydrodynamic and kinetic behavior in converging targets, including 3-D flows and viscosity.

Experiments focused on hydrodynamic instability growth are well matched by simulation, but the community seems overly focused on hydrodynamic growth and its impact on implosion and ignition, and not on the underlying general coupled multi-physics problem. A key issue is the inability to assess accurately the full impact of the driver on initial conditions, such as CBET and hot electron pre-heat. This may be more important than the relatively well settled issue of predicting the growth of hydrodynamic instabilities, such as Rayleigh-Taylor (RT), Richtmyer–Meshkov (RM), or Kelvin–Helmholtz (KH) at low or high mode number.

Instabilities can combine in ways that are difficult to predict; but are apparent in astrophysical phenomena, ICF/HED experiments, and nuclear detonations. Much can be learned from astrophysical phenomena, however weapon scientists need more information than can be obtained from astrophysics, and this was clear during the review discussions. LANL scientists have developed a shock-shear platform that demonstrates complex plasma instability behavior in a controlled manner. Used on Omega and NIF, this platform is providing data of value to fundamental science and has value towards answering specific weapons performance questions. Other fundamental science efforts by labs and university groups explore colliding plasmas and various plasma hydrodynamic phenomena, and are of value to the weapons community. New diagnostics, coupled with more sophisticated models, create opportunities to pursue previously unresolved fundamental questions. Many of the challenges in hydrodynamics and burn are common to the ICF/HED Program and other scientific communities, presenting opportunities for generating innovative ideas through collaborations.

In layered implosion simulations, all unstable wavelengths can be resolved in the highly resolved spatial representation of the material structure in the calculation. This provides an opportunity to advance understanding of the evolution of these instabilities into turbulence with the resulting mixing of materials in the target. For example, LES and RANS models are needed to simulate turbulent mixing in deuterated carbon mix experiments. Unfortunately, these models have not been successful in correctly predicting all three DD, TT, and DT reaction yields. Most capsule simulations are done in 2-D using diffusive energy transport and without self-generated magnetic fields.

A growing body of data enabled largely by the nuclear diagnostics developed by MIT suggests the importance of kinetic processes. Developing understanding of these processes will challenge experimental platform development, diagnostics development, and development of multi-scale modeling capabilities. The integration of kinetic or microphysics effects into the modeling of integrated systems in a self-consistent way is a grand challenge and will push the frontiers of high performance computing.

Although "strength models" are used to model time-independent, inelastic deformation of solids, this may not be the correct way to represent the physics of Resistance to Deformation (RTD) under dynamic loading. But the determination of material strength or RTD needs to go beyond time-independent, phenomenological approaches. Understanding RTD or developing accurate strength models applicable to a wide variety of load paths remains a significant challenge and is important. Plane shock wave or ramp compression data are not sufficient to discriminate between different strength models.

3.5.2 NNSA Program Office Perspective and Items for Future Prioritization & Action

- Regarding fundamental science efforts, the ICF Program Office will:
  - Support individual investigators at laboratories who are pursuing fundamental physics research, through capture of specific goals into program plans and annual implementation plans to emphasize this work.
  - Pursue of theoretical quantum molecular dynamics and other approaches for development of equations of state, transport properties, radiation properties (particularly line shapes), and non-LTE physics. Emphasis will be placed on experimental observables that can be used to post-process simulations for direct and detailed comparisons with data.
  - Support university investigators working in the area of HED physics through both experimental time on HED facilities and financial support for graduate students, postdoctoral fellowships, and research costs.
- The ICF Program Office will specifically seek opportunities to validate physics models directly by utilizing all available experimental platforms, making cross-platform comparisons, and developing complementary platforms and diagnostics. This includes:
  - Validating the recent Fe opacity experiments on Z through further experiments on Z and conducting experiments on the NIF, and
  - Understanding and utilizing the unique and complementary characteristics of the LCLS for HED investigations.

3.6 Academic Programs and External Partners

3.6.1 Summary of Reviewer Comments

Partnerships with academia and private industry are instrumental to success in HED science, particularly for ICF. Scientific engagement with partners outside the NNSA laboratories lessens insularity and reduces the potential for group think. Therefore it is important to maintain a vibrant community of researchers external to the national laboratories to serve as a pool of collaborators and as a scientific system of checks and balances.

The DOE Office of Science (SC) and NNSA share an interest in nurturing partnerships and developing researchers in HED science. The interests of emerging scientists should influence program planning in SC and NNSA; the pending retirement of professors in the field makes this somewhat urgent and strengthens the importance of the ICF Program PRDs. The PRDs provide a link between basic and applied science in ICF, and help to establish more effective ties between the academic community and laboratory scientists.

The ICF/HED Program maintains a world-leading set of experimental facilities, from modest facilities such as Trident, Jupiter, and Nike, to larger facilities such as the National Ignition Facility, Z Facility, and the Omega Laser Facility. These facilities enable research at the frontier of discovery science in HED and ICF and attract many of the best and brightest researchers into the field with the opportunity to study matter in the laboratory at states that otherwise only exist in astrophysical systems.

The availability of new facilities provides an extended set of capabilities that NNSA must consider when executing its mission. For example, facilities like LCLS operate on the basis of the peer group proposal process, with the best proposals awarded beam time. ICF should support newer capabilities being built around the world, such as petawatt lasers generating relativistic electrons and other extreme conditions, high energy swift heavy ion sources, and sub-picosecond, intense hard x-ray free electron lasers (XFELs), and use these new capabilities to build new partnerships, provide relevant benchmark data, and recruit from a broader pool of high-quality students.

The Z Facility and the NIF are not considered typical user facilities. At both facilities, in order for an external (non-NNSA) user to conduct an experiment, there must be strongly engaged NNSA laboratory scientists with the understanding and requisite savvy to efficiently support the experiments as well as a genuine interest in the science being explored. They must have a strong scientific interest, the time, the experience, and the stature in the facility to help the academic partner succeed. It is important to provide some open experimental time through a competitive peer review process to capture the full potential for fundamental science on these facilities. Although only a limited amount of time is made available, every fundamental science experiment provides tremendous value to the researcher, student, or partnering agency.

Although NNSA requirements necessarily limit the amount of experimental time dedicated to user-driven discovery science, there is broad agreement among the reviewers that it is an important component of the suite of experimental activities. Another important aspect of user access is for that access to be multi-year with commensurate funding needed for graduate students to complete their work and for laboratory staff to support the experiments. Finally, access to codes by external researchers is needed for experimental design, to broaden the code user base, and to level the playing field for leading edge research.

The state-of-the-art ICF/HED research facilities and the grand challenge of ignition makes NNSA laboratories attractive to talented young researchers and helps to retain the highly-competent staff who contribute directly to the SSP. One strategy to broaden the pipeline is to engage near neighbor disciplines. Many leading professors with good track records of supplying students to NNSA laboratories are nearing retirement. At some universities there is no clear succession plan and the path to sustain the research program and the student pipeline is not apparent.

NNSA invests in university HED science through multiple programs: the Stockpile Stewardship Academic Programs, which includes the Stewardship Science Academic Alliance (SSAA) Program, the National Laser Users' Facility (NLUF) Program, and the Joint Program in High Energy Density Laboratory Plasmas (JPHEDLP); and through support of users groups such as the Omega Laser Users' Group (OLUG) and the NIF Users' Group. More could be done to support the academic programs that train the next generation of scientists and to recruit them, by creating and strengthening partnerships between the national laboratories and universities, grant programs, graduate student fellowships, and by providing more access to experimental facilities. The talent pipeline should be monitored to ensure that individuals with relevant skill sets are available in sufficient quantities; so that the program is better informed to make decisions regarding investments in academic programs.

3.6.2 NNSA Program Office Perspective and Items for Future Prioritization & Action

- A call for Centers of Excellence, which includes the current HED Centers, is scheduled for summer 2016. Academic investments for the HED portfolio will be selected, informed by the results of the 2015 ICF/HED Review, with consideration given to partnering with SC and industry. A Center structure with scientific independence is envisioned, but with exposure to the national laboratories for students, with fellowships and collaborative projects, and with critical skills developed through incentivizing key scientific areas.
- NNSA will consider a sabbatical program through which national laboratory or university scientists could spend dedicated time at another lab or university to foster scientific collaborations.
- NNSA will review ways to better use the full breadth of SC and worldwide scientific capabilities that can achieve the HED conditions for the SSP mission. This will include identifying ways to reward scientists at the laboratories for developing and fostering successful collaborations with researchers at universities and private companies.
- NNSA will explore metrics to measure the health of the staff pipeline, tracking both the number of students entering the laboratory system and the schools and faculty training them. Ideally, future funding decisions will consider this data.
- The NNSA will explore potential user models for the ICF/HED facilities that balance mission-specific requirements with the desire for access from the broader scientific community.

3.7 Program Direction

3.7.1 Summary of Reviewer Comments

3.7.1.1 Integrated Strategic Roadmaps

Since the publication of the 2012 Path Forward Report the program has considerably strengthened the impact and linkages to the SSP. SNL has shifted the majority of the use of Z Facility time from ICF to stockpile-relevant plutonium materials research, opacity studies, and radiation effects science. At the end of the NIC, facility time on the NIF shifted from 85 percent ICF to approximately 50 percent HED experiments, each of which is reviewed and approved by the HED Council – the multi-site body of technical experts and cognizant program managers that provides recommendations as to the use of experimental resources dedicated to HED experiments, in accordance with SSP priorities. The recent reorganization at LLNL has aligned the management and research for ICF with the Weapons & Complex Integration (WCI) Directorate, and this has effectively enforced the appropriate balance of priorities at the NIF. The resulting organizational structure needs time to stabilize to tackle the challenges with the tools and people that are being developed.

The HED Council has been a welcome influence on the direction of research on the ICF/HED facilities. The HED Council has expanded participation in experimental planning and prioritization, and has made a concerted effort to direct experiments to the most appropriate facilities without the past institutional biases. The HED portfolio is producing outstanding results for the SSP and it has a sound strategic plan.

The mission drivers for the ICF Program are quite clear. Pursuing thermonuclear burn in the laboratory, achieving ignition, and multi-megajoule fusion yield have important implications to national security. Achieving ignition in the laboratory is arguably one of the preeminent scientific challenges of our time and would represent an extraordinary demonstration of U.S. excellence in science and technology relevant to nuclear weapons. It would further NNSA's scientific capabilities, assure allies, and deter potential adversaries. Some of the excellent young scientists and engineers drawn to ignition research at NNSA's state-of-the-art HED facilities will move to nuclear weapons design work. The HED Council plays a key role in planning and decision-making for the non-ICF portfolio. ICF Program planning could be improved and the roles and responsibilities of the ICF Council – the multi-site body of technical experts and cognizant program managers that perform a cursory review of planned experimental activities for each ICF facility – could be revised/retooled to be more useful. Establishing improved roadmaps and decision processes would help to focus the workforce on the research priorities.

Program planning for the near term is critical, but it is also important to define the program 10 to 20 years from now. Ignition is one important step along the path and not the final end point.

Over the long term, the program is aimed toward a high-yield capability whether or not ignition is reached on the NIF, and major facility upgrades must be considered over a long time-scale. A fusion source on the order of 500 megajoules or greater will be important for the health of the program in an extended era without nuclear tests. Such a source is unlikely to be achieved in the next decade, but maintaining high yield as an ultimate goal should guide program thinking and direction in the interim.

The sophistication of approach, roadmaps, and decision processes varies widely among the three approaches to ICF ignition. The LID approach presented overarching goals to improve understanding and models of ignition target behavior to either demonstrate ignition or show what is needed in capability and understanding to ignite a target. However, there were few details on how a finding would result in a change in program direction. There was also a threat of dilution of intellectual energy as the number of sub-approaches increased. The LDD approach showed a roadmap and decision process based on goals for the hot-spot pressure and mitigation of cross beam energy transfer, but little to no peer review of that approach has taken place. The MDD approach presented a range of goals over the next five years aligned to the PRDs, but like the LID Program, a detailed roadmap and decision process still needs to be developed. Unlike the LID Program, however, the MDD Program suffers from a narrow research focus mostly due to resource constraints.

The roadmap for each approach (LID, LDD, and MDD) must be woven into an overarching roadmap driven by the vision described in the directors' letter at five-, 10- and 15-year waypoints. This roadmap should meet mission requirements, be inspirational, and be appropriately paced and balanced given the many technical challenges in ICF.

3.7.1.2 The Naval Research Laboratory

Over 50-years, the Naval Research Laboratory's (NRL) scientists, engineers, codes, diagnostics, and facilities have acted as a science and technology bridge between the DOD and DOE. It has provided expertise and "corporate memory" for ICF and weapons physics, pulsed power science, high power electron and ion beams, dense z-pinches, nuclear weapons effects testing, non-LTE physics, and related theories, codes, and diagnostics. NRL contributes in many areas to ICF and laser physics: in LDD and hybrid x-ray/direct drive approaches using coated capsules, investigating CBET and LPI at Nike, developing diagnostics including the Virgil M-band spectrometer for the DANTE at the NIF, and experiments and calculations in non-LTE atomic physics for nuclear weapons effects (K-shell) on Z. Scientific leadership by the NRL might be strengthened by focusing on a smaller number of high impact efforts, and through better integration internally between its research "branches."

3.7.1.3 Additional Opportunities for Technical Leadership

Independent of the small percent of funding that the ICF Program provides to academic programs for activities described in Section 3.6, NNSA should consider funding independent researchers focused on high-risk, high-impact *applied science with concrete deliverables* to the program. These independent researchers could lead working groups and teams of external and laboratory researchers focusing on key physics issues such as:

- Novel diagnostics to probe non-LTE plasmas,
- Advancing the kinetic theory of plasmas and computational capabilities for laser plasma interactions,
- Developing diagnostics to spatially, spectrally, and temporally resolve the physics of hot spot assembly and stagnation, and
- Physics validation of existing models in ICF codes in multiple areas, such as LPI and MHD.

Additionally, the ICF Program would benefit from increased competition in integrated experiments to encourage laboratory researchers within the ICF Program and researchers external to it, to propose novel ideas, have those ideas reviewed, and be awarded experimental time and funding to support their research.

3.7.2 NNSA Program Office Perspective and Items for Future Prioritization & Action

- The 2015 ICF/HED Review has informed and matured the National ICF Program Framework. The four-element Framework is as follows:
    - *Ten-year High Energy-Density (HED) Sciences Strategic Plan.* This classified requirements document outlines deliverables for the ICF/HED Program in three-, five-, and ten-year time frames, and is derived from the 25-year SSMP.
    - *National Transformative Diagnostics Plan.* This resource-loaded plan describes eight transformative diagnostics that benefit all ICF approaches. Local and broad diagnostics are managed within the next two elements of the Framework.
    - *Integrated Experimental Campaigns.* This element, frequently depicted as a Gantt-chart, contains the approach-specific experimental campaigns for highly integrated experiments with the primary goal of achieving thermonuclear burning plasma conditions and that push the limits of NNSA's capabilities and facilities. Typically progress is assessed by demonstrating improvements in integrated performance parameters, such as yield and shape. The five-year goal of this element is to determine the efficacy of NIF for ignition and a credible physics scaling to multi-megajoule yields for all ICF approaches.
    - *The ICF Priority Research Directions (PRDs).* This approach-specific six-part work breakdown structure enables cross-approach coordination and opportunities for external collaborations at the working level. The PRDs enable the development

of physics-based milestones that integrate compendiums of experimental research executed at each ICF/HED facility with the overall efforts to improve models, codes, and simulations. The PRDs are:

    - Driver-target Coupling
    - Target Preconditioning
    - Implosion Hydrodynamics
    - Stagnation and Burn Physics
    - Intrinsic and Transport Properties
    - Measurement, Modeling, Validation, and Approximation

The Framework will be used to develop a roadmap in fiscal year 2016 with priorities, metrics, milestones and deliverables, as well as specific "decision trees" to support out-year investments. NNSA will periodically sponsor workshops on the progress toward ignition, covering all three ICF approaches. The next major workshop, related to the PRDs, will be held in June 2016 in Santa Fe, NM.

- The ICF Council Charter will be revisited to assess Council roles, responsibilities, accountabilities, authorities, and overall value. One additional role for the ICF Council could be to host a review process to award facility time for new ideas outside the mainline ICF Program efforts, as is done for general use time at SC facilities.
- NRL's portfolio will be reviewed to identify the highest impact activities and to recommend new opportunities, such as strategic collaborations in atomic physics and spectroscopy or building collaborations with LLNL and LLE in the area of radiation source development, with supporting experiments at NIF and Omega.
- In addition to the SSP-driven requirement to maintain exceptional scientific capabilities in HED science, NNSA will stand up efforts in fiscal year 2016 to assess long-term requirements in five major areas:
    - Maintaining proficiency in secondary design and the ability to assess performance in the long term in the context of no new nuclear testing.
    - Evaluating the long-term experimental needs for threat-condition hostile environments and nuclear survivability of non-nuclear components.
    - Coalescing the vision for future capabilities for NNSA dynamic material properties research to enable safe, high-hazard materials science experiments.
    - Defining a clear experimental program in burn physics to support boost science.
    - Avoiding technological surprise.

4 Next Steps

The 2015 ICF/HED Review identified nearly 40 areas for future prioritization and action. In fiscal year 2016, NNSA plans to develop an "after actions" plan and a schedule for implementation. Several areas have been identified where priorities should be re-evaluated, including:

- Transformative diagnostics, including spatially, spectrally, and temporally-resolved imaging and spectroscopic diagnostics to observe "stagnation" at low, medium, and high convergence.
- Obtaining cross-platform data for fundamental physics validation of models/codes while improving access to codes/models, where appropriate.
- Reviving development efforts for codes to model Laser-Plasma Instabilities (LPI).
- Increasing the number of designers and experimentalists working on magnetically-driven implosions and laser-driven direct drive programs.
- Enhancing peer review by academia and other institutions.
- Developing applications for fusion yields produced on existing platforms.
- Assessing the long-term requirements case for "high yield".
- Identifying methods such that all HED capabilities (domestic and international) may be considered as NNSA defines the means by which it will execute SSP-related experiments.
- Developing robust cadre of top researchers in key areas of atomic physics, spectroscopy, laser plasma instabilities, and low-energy nuclear physics.
- Shaping academic program investments to ensure resources are optimally deployed.

Reviewers were not asked to consider resource constraints when providing comments or recommendations. To affect all recommendations contained herein would exceed current budget profiles. The principal next step is for NNSA to identify specific resource requirements to prioritize these recommendations within existing budgets. This prioritization process will begin in FY 2016.

Acronyms, Abbreviations, and Terms List

A-C

Al	Aluminum
ALS	Advanced Light Source, Lawrence Berkeley National Laboratory
APS	Advanced Photon Source, Argonne National Laboratory
ASC	Advanced Simulation and Computing Program
Be	Beryllium
B·r	The product of the magnetic field, B, and the radius, r
CBET	Cross Beam Energy Transfer

D-E

DANTE	Soft X-ray spectrometer used to measure radiation drive temperature
DARHT	Dual-Axis Hydrodynamic Radiographic Test Facility, Los Alamos National Laboratory
D.C.	District of Columbia
DD, D_2	Deuterium-Deuterium
DESY	Deutsches Elektronen-Synchrotron, a national research center in Germany
DFT-MD	Density Functional Theory-Molecular Dynamics
D^3He	Deuterium - Helium-3
DOD	Department of Defense
DOE	Department of Energy
DT	Deuterium-Tritium
ELI	Extreme Light Infrastructure, Laser User Facility with facilities in the Czech Republic, Hungary, and Romania
EOS	Equation of State
eV	electron Volts

F-G

FAB	Full Aperture Backscatter
Fe	Iron
FLASH	A free-electron laser at DESY that generates soft X-rays
FNADS	Flange Nuclear Activation Diagnostic System
FY	Fiscal Year
GB	gigabar
GB-ns	Gigabar - nanosecond
GPUs	Graphic Processing Unit(s)

H-I

^3He, He-3	Non-radioactive isotope of helium with two protons and one neutron
HED	High Energy Density (Physics)
HYDRA	LLNL multi-physics simulation code
ICF	Inertial Confinement Fusion

ICF Laboratories	The NNSA Laboratories, the Laboratory for Laser Energetics, and the Naval Research Laboratory
ICF/HED	Inertial Confinement Fusion/High Energy Density
Intel® Xeon Phi™	Intel® Xeon Phi™ Coprocessor, from Intel Corporation

J-K

Jupiter	Jupiter Laser Facility at LLNL
KBO	Kirkpatrick Baez Optic
keV	kilo electron Volt
KH	Kelvin-Helmholtz Instability
kJ	kilo Joule
K-shell	The first shell of electrons surrounding the nucleus of an atom

L

LANL	Los Alamos National Laboratory
LASNEX	Computer code used in ICF that simulates interactions and effects between x-rays and a plasma.
LCLS	Linac Coherent Light Source, at SLAC National Accelerator Laboratory
LDD	Laser-Driven Direct Drive
LEH	Laser Entrance Hole
LEP(s)	Life Extension Program(s)
LES	Large Eddy Simulation, model for turbulence
LID	Laser-Driven Indirect Drive
LLE	Laboratory for Laser Energetics
LLNL	Lawrence Livermore National Laboratory
LPI	Laser-Plasma Interaction(s)
LTE	Local Thermodynamic Equilibrium

M

M	Millions
MagLIF	Magnetized Liner Inertial Fusion
M-band	refers to the spectra from M-band emissions, x-rays in the 1.5 to 6.0 keV range
MDD	Magnetically-Driven Direct Drive
Mg	Magnesium
MG-cm	Mega gauss-centimeter
MHD	Magnetohydrodynamics
MIT	Massachusetts Institute of Technology

N

NA-10	Defense Programs, within NNSA
NA-11	Office of Research, Development, Test, and Evaluation, within NA-10
NA-12	Office of Stockpile Management, within NA-10
NA-19	Office of Major Modernization Programs, within NA-10

NBI	Near Backscatter Imager
NIC	National Ignition Campaign
NIF	National Ignition Facility, located at LLNL
Nike	Krypton fluoride (KrF) Laser, located at NRL
NISP	National Implosion Stagnation Physics Working Group
NLUF	National Laser Users' Facility, at Omega Laser Facility, LLE
NNSA	National Nuclear Security Administration
NNSS	Nevada National Security Site
non-LTE	non-Local Thermodynamic Equilibrium
Nova	High-power laser located at LLNL, built in 1984 and dismantled in 1999
NRL	Naval Research Laboratory

O-P

OLUG	Omega Laser Facility at University of Rochester's LLE
Omega	Omega Laser Facility at the Laboratory for Laser Energetics, University of Rochester
PF3D	A laser-plasma interaction (LPI) code used to simulate experiments
PIC	Particle in Cell
PRD(s)	Priority Research Direction(s)
P-tau, P-τ	The product of the plasma pressure, P, in atmospheres, and the energy confinement time, τ, in seconds. This product is called the Lawson Criterion.
Purgatorio	LLNL microphysics code

Q-R

RANS	Reynolds-averaged Navier-Stokes Equations
rho-R, ρ-R	Product of the mass density and radius in the hot spot of an ICF implosion
RM	Richtmyer-Meshkov Instability
RT	Rayleigh-Taylor Instability
RTD	Resistance To Deformation

S

SC	Office of Science, U.S. Department of Energy
SDD	Symmetric Direct Drive
Si	Silicon
SLAC	SLAC National Accelerator Laboratory at Menlo Park, CA (originally named *Stanford Linear Accelerator Center*)
SNL	Sandia National Laboratories
SRS	Stimulated Raman Scattering
SSAA	Stockpile Stewardship Academic Alliances Program
SSAP	Stewardship Science Academic Programs
SSD	Smoothing by Spectral Dispersion
SSMP	Stockpile Stewardship Management Plan
SSP	Stockpile Stewardship Program

STS	Stockpile-to-Target-Sequence
SXI	Static X-ray Imager

T-U

TPD	Two Plasmon Decay Instability
TR	Technical Report
Trident	Trident Laser Facility at LANL
TT	Tritium-Tritium
U.S.	United States

V-W

VFP	Vlasov-Fokker-Planck Model
VISAR	Velocity Interferometer System for Any Reflector
WCI	Weapons and Complex Integration (WCI) Directorate at LLNL

X-Y

XFEL, XFELs	X-ray Free Electron Lasers
XTD	X Theoretical Design (XTD) Division at LANL

Z

Z	Z Pulsed Power Facility at SNL
Z	atomic number of a chemical element

1-D	One dimensional
2-D	Two dimensional
3-D	Three dimensional
2DConA	Two Dimensional Convergent Ablator, one of the Horizontal and Vertical Axis Radiography Platforms on the NIF
$\Delta\rho R$	Variation in ρR in an ICF implosion

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OFFICE OF DEFENSE PROGRAMS

2015 Review of the Inertial Confinement Fusion and High Energy Density Science Portfolio: Volume II

May 2016

DEPARTMENT OF ENERGY | NATIONAL NUCLEAR SECURITY ADMINISTRATION | DEFENSE PROGRAMS [NA-10]

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Appendix A Review Materials

A.1 Reviewers

Group 1 - Progress Toward Ignition Federal Lead: Lois Buitano	
Name	Affiliation
Jerry Chittenden	Imperial College
Siegfried Glenzer	SLAC National Accelerator Laboratory
Jim Hammer	Lawrence Livermore National Laboratory
Nelson Hoffman	Los Alamos National Laboratory
Warren Mori	University of California, Los Angeles
Andrew Randewich	Atomic Weapons Establishment
Sean Regan	Laboratory for Laser Energetics
Bob Rosner	University of Chicago
Susan Seestrom	Los Alamos National Laboratory, Retired
Steve Slutz	Sandia National Laboratories

Group 2 - Non-Ignition HED Science and Long Term Planning Federal Lead: Njema Frazier	
Name	Affiliation
Dave Crandall	NNSA, Retired
Jill Dahlburg	Naval Research Laboratory
John Harvey	DOD, Retired
Jeff Quintenz	NNSA, Retired

Group 3 - Scientific Foundations Federal Lead: Kirk Levedahl	
Name	Affiliation
Sean Finnegan	Office of Fusion Energy Sciences
Yogi Gupta	Washington State University
Stephanie Hansen	Sandia National Laboratories
Dick Lee	University of California, Berkeley, Retired
John Sarrao	Los Alamos National Laboratory
George Zimmerman	Lawrence Livermore National Laboratory

A.2 Review Schedule

Review Kickoff Meeting – Washington D.C.

May 18, 2015

08:00 AM	*Coffee*	
08:30 AM	Welcome & Introductions	
08:45 AM	2015 Review Summary	Keith LeChien
09:00 AM	Federal HED Program Perspective	Keith LeChien
09:30 AM	10-year Strategic Vision	Alan Wan
10:15 AM	*Break*	
10:30 AM	Laboratory Leadership Panel: Design Agency Perspectives on ICF/HED and the National Program	Charlie Nakhleh, Keith Matzen, Charlie Verdon
11:45 AM	*Lunch*	
12:45 PM	Scientific Hypotheses: Laser-driven Indirect Drive Priority Research Directions	John Edwards
02:00 PM	Scientific Hypotheses: Laser-driven Direct Drive Priority Research Directions	Craig Sangster
03:15 PM	*Break*	
03:30 PM	Scientific Hypotheses: Magnetically-driven Direct Drive Priority Research Directions	Dan Sinars
04:45 PM	ICF Program laboratory panel: Approaches, progress, collaborations, and outlook in National Program	John Edwards, Craig Sangster, Don Haynes, Dan Sinars
05:30 PM	Wrap Up	

May 19, 2015

08:30 AM	*Coffee*	
09:00 AM	Welcome & Recap	Keith LeChien
09:15 AM	Sustaining Stockpile Confidence with HED Science	Alan Wan
10:00 AM	*Break*	
10:15 AM	Plutonium Science	Scott Crockett
10:55 AM	HED and Boost Science	Frank Graziani
11:35 AM	Secondary Physics	John Scott
12:15 PM	*Lunch*	
01:00 PM	Addressing UGT Anomalies	Steve MacLaren
01:40 PM	Outputs, Environments and Effects	Brent Jones
02:20 PM	*Break*	
02:35 PM	Understanding Fundamental Processes Which Support Both Ignition and Weapons Science	Rip Collins
03:35 PM	Cross Program and Platform Integration	Charlie Nakhleh, Keith Matzen, Charlie Verdon
04:20 PM	Wrap Up	

May 20, 2015

08:30 AM	*Coffee*	
09:00 AM	Next Steps Overview	Keith LeChien
09:30 AM	Group Breakouts - Define Deep Dive Topics and Format	Federal Leads Moderate
10:30 AM	*Break*	
10:45 AM	Group Breakouts - Continued	Federal Leads Moderate
11:45 AM	*Lunch*	
12:30 PM	Outbriefs	Keith LeChien
01:30 PM	Wrap Up	

Laboratory Site Visits

Group 1: Progress Toward Ignition

Date	Site	Site Leads
July 28 – 29, 2015	Sandia National Laboratories	Dawn Flicker, Dan Sinars
July 30 – 31, 2015	Lawrence Livermore National Laboratory	John Edwards, Alan Wan
August 3 – 4, 2015	Laboratory for Laser Energetics	Craig Sangster, David Meyerhofer

Group 2: Non-Ignition HED Science and Long Term Planning

Date	Site	Site Leads
July 14 – 15, 2015	Lawrence Livermore National Laboratory	John Edwards, Alan Wan
July 16, 2015	Los Alamos National Laboratory	Don Hanes, Steve Batha
July 17, 2015	Sandia National Laboratories	Dawn Flicker, Dan Sinars
July 23, 2015	DOE Headquarters with Presentations by the Laboratory for Laser Energetics and the Naval Research Laboratory	Craig Sangster, David Meyerhofer, Tom Mehlhorn

Group 3: Scientific Foundations

Date	Site	Site Leads
July 14 – 15, 2015	Lawrence Livermore National Laboratory	John Edwards, Alan Wan
July 16, 2015	SLAC National Accelerator Laboratory	Michael Dunne, Siegfried Glenzer

A.3 Reviewer Instructions

FY 2015 ICF/HED Review
Guidance to Reviewers

Please use the attached template as a guide to provide your findings, comments, and recommendations to the Group Lead for your assigned Group.
The findings, comments, recommendations will be published in the Appendix of the Summary Report for the Review, and will include only minor edits, as appropriate. Note that comments that you do *not* want published should be provided in the section labeled "Specific comments for HQ only".
You may provide comments and recommendations on the other Group sections. In that case, your input will be provided to the appropriate Group HQ Lead for consideration and possible use.
If you have questions, contact any of the Group Leads or Keith LeChien.
Your input is due on Tuesday September 1, 2015. Send your completed review via email attachment to the ICF Director and all of the HQ Group Leads:

Keith LeChien,	ICF Director
Lois Buitano,	Group 1 HQ Lead
Njema Frazier,	Group 2 HQ Lead
Kirk Levedahl,	Group 3 HQ Lead

CLASSIFIED findings, comments, and recommendations must be prepared and submitted in accordance with Security Policies. Please submit classified portions of your review to the ICF Director and HQ Group Leads via the NESAN system. If it is not possible for you to provide classified through NESAN, please contact Daniel Jobe to make other arrangements.

2015 ICF/HED Review

Group: 1 – Progress Toward Ignition

Reviewer Name: _______________________________

Charge for Group 1:

The primary charge is an assessment of the scientific hypotheses that guide today's ICF Program of work, and prospects of achieving ignition with existing scientific capabilities and facilities, or, if indicated, by specifying what would be required to do so, based on quantitative scientific analysis. We also request an evaluation of program balance among ICF approaches.

Provide findings and recommendations related to experimental and computational efforts that address scientific hypotheses related to the development of robust and reliable burning plasmas.

Assess the effectiveness of the ICF Program's cross-platform and cross-laboratory collaboration.

Specific Comments for HQ only.

I. Assess the scientific hypotheses and the prospect for achieving ignition with existing scientific capabilities and facilities; or, if indicated, what would be required to achieve ignition and supporting analysis. Provide an evaluation of program balance among ICF approaches.

Ignition Approach: *Laser-driven Indirect Drive*

Findings:

Comments:

Recommendations:

Ignition Approach: *Laser-driven Direct Drive*

Findings:

Comments:

Recommendations:

Ignition Approach: *Magnetically-driven Direct Drive*

Findings:

Comments:

Recommendations:

Roadmaps and Decision Processes

Findings:

Comments:

Recommendations:

Program Balance
Findings:

Comments:

Recommendations:

II. Assess the integration of experiments and codes
Diagnostics
Findings:

Comments:

Recommendations:

Computational Models and Predictive Capability
Findings:

Comments:

Recommendations:

III. Assess cross-platform and cross-laboratory collaborations
Findings:

Comments:

Recommendations:

IV. Specific comments for HQ only

2015 ICF/HED Review

Group: 2 – Non-ignition HED and Long Term Goals

Reviewer Name: _______________________________

Charge for Group 2:

The primary charge is an assessment of the alignment of the ICF/HED program with stockpile stewardship program and the broader nuclear weapons program. Assess the contribution to stockpile stewardship in the non-ignition HED sciences in the near, medium, and long term.

In their January 20, 2015 letter, the laboratory director's described several specific multi-decade goals for the ICF/HED program within the context of the broader stockpile stewardship program. Assess both the scientific and programmatic progress – and plans – in today's ICF Program to meet those goals.

The formal title of the ICF Program is "Inertial Confinement Fusion and High Yield." "High-yield" at laboratory scale is more than a decade away, but having this ultimate goal shapes program decisions many years in advance of their perceived need. Assess the need of "high-yield" capabilities at laboratory scale as a long term goal of the ICF/HED program given evolving nuclear threats, and the overarching boundary condition of no additional nuclear testing.

Specific Comments for HQ only.

I. Alignment of ICF/HED program with SSP and broader weapons program

Workforce Development

<u>Findings:</u>

<u>Comments:</u>

<u>Recommendations:</u>

Program Management

<u>Findings:</u>

<u>Comments:</u>

<u>Recommendations:</u>

II. Planning associated with tri-lab Director's letter

Long-Term Direction

<u>Findings:</u>

<u>Comments:</u>

<u>Recommendations:</u>

III. High Yield for SSP

Applications of Yield

Findings:

Comments:

Recommendations:

IV. Specific comments for HQ only

2015 ICF/HED Review

Group: 3 – Scientific Foundations

Reviewer Name: ________________________________

Charge for Group 3:

The ICF/HED program is highly applied and focused on programmatic deliverables, but the field of high energy-density science is quite exploratory. The primary charge is to identify opportunities to improve the underlying physics; the impact of simulations, models and codes; and experimental capabilities (including targets and diagnostics) to best increase the integrated rate of progress on programmatic deliverables.

Identify areas where partnerships with external entities (academia, industry, other government, international, etc.) may be strengthened to support these opportunities.

Specific Comments for HQ only.

I. Underlying physics understanding and integration.

EOS

<u>Findings:</u>

<u>Comments:</u>

<u>Recommendations:</u>

Opacity and transport

<u>Findings:</u>

<u>Comments:</u>

<u>Recommendations:</u>

Hydro and burn physics

<u>Findings:</u>

<u>Comments:</u>

<u>Recommendations:</u>

Global and driver physics

<u>Findings:</u>

<u>Comments:</u>

<u>Recommendations:</u>

II. Partnerships with external entities

Community: codes

Findings:

Comments:

Recommendations:

Community: Experiments

Findings:

Comments:

Recommendations:

Community: Collaborations

Findings:

Comments:

Recommendations:

III. Specific comments for HQ only

Appendix B Reviewer Reports

B.1 Group 1 Reviewer Reports

Reviewer Report: Jeremy Chittenden

I. Assess the scientific hypotheses and the prospect for achieving ignition with existing scientific capabilities and facilities; or, if indicated, what would be required to achieve ignition and supporting analysis. Provide an evaluation of program balance among ICF approaches.

Ignition Approach: Laser-driven Indirect Drive

Findings: The last three years has seen a period of significant scientific progress in inertial confinement fusion experiments on the National Ignition Facility. The volume of high quality published research that has emerged over this period has been most impressive. To a large part this has been achieved by concentrating on the use of higher adiabat, lower convergence shots, whose performance more closely matches the predictions of simulations, such as those characterized by the 'high-foot' radiation pulse. This is to be contrasted with the lower adiabat, higher convergence experiments undertaken during the National Ignition Campaign where the susceptibility to instability growth led to a shot-to-shot irreproducibility which removed systematic trends from the data. The densities and temperatures achieved in the hotspot within 'high-foot' capsule implosions are sufficient for a large fraction of the plasma heating to come from alpha particle fusion products. This has been quite rightly recognized by the fusion community as a significant achievement. The high adiabat of the fuel however means that the hotspot density which can be achieved in these implosions is too low to allow these designs to be used directly as high yield platforms. The 'high-foot' platform does however present a well-established 'base-camp' design from which to progress towards higher yields along different trajectories in parameter space which avoid many of the issues experienced during the National Ignition Campaign.

Despite the successes of the high-foot design the fusion yield remains significantly lower than predicted by unperturbed calculations. Continuous improvements both to the diagnostic capabilities on the NIF and the ability to simulate the multi-dimensional effects of perturbations, have improved the overall ability to discern which factors are making the most significant contributions to the degradation in performance. Principle amongst these are thought to be the time dependent radiation drive asymmetry arising from Cross Beam Energy Transport (CBET) in the hohlraum and perhaps to a lesser extent the imprint on the capsule surface formed by the capsule support tent.

The lack of control over the time dependence of the CBET within the gas filled hohlraums has led to a program to develop alternative hohlraum designs with lower gas fill pressures as a means of mitigating this issue. The reduction in the tamping effect of the gas introduces a new set of challenges into the design of these hohlraums and requires accurate modelling of the plasma expanding from the hohlraum wall and the collision of this with the blow off from the capsule, as well as the impact this has on laser propagation and the potential for the late onset of CBET in the blow off plasmas. These issues can in turn be mitigated by the use of denser ablator materials on the capsule such as high density carbon or beryllium, which require a shorter radiation drive pulse and therefore allow the laser energy to couple to the hohlraum before it is filled by high density blow off plasma. A program is also underway for advanced hohlraum designs which reduce the plasma expansion velocity and which also have the potential to produce a more Planckian radiation spectrum with reduced high photon energy

Comments: Much of the progress that has been achieved can be attributed to a fairly pragmatic approach where problems which appear insurmountable are circumvented rather than tackled head-on. This was the case with the adoption of higher adiabat lower convergence targets to improve stability and again with the development of the low gas fill hohlraum to reduce CBET. Such an approach is laudable and doubtless offers the path to most rapid progress. It is possible however that such problems may need to addressed once again should they be encountered again on various possible paths of progression from the stable 'base-camp' of the high foot design towards higher yields.

It is tempting to believe that the factors limiting performance in the highest yielding designs are well established, indeed the evidence for time dependent radiation drive asymmetry and associated swings in capsule shape is fairly compelling. Similarly there does appear to be a correlation between the shape of the tent's contact with the capsule and the structure of the capsule observed in radiography images, although the variation in performance with tent thickness is perhaps not as well understood. There remain however a number of other effects which may also be contributing to reduced performance such as the fill tube, hot electron preheat, inaccuracies in the equation of state of deuterium etc. Such effects could well prove to be important, but do not necessarily provide clear diagnostic signatures. The present approach of first addressing the demonstrably large amplitude perturbations arising from drive asymmetry and the capsule mounting before attempting to evaluate other effects whose contribution might be masked by these larger effects therefore appears reasonable.

The issue of cross beam energy transfer was one of the first problems to be encountered during early experiments on the NIF. At the time a series of work arounds were introduced into the design tools and the experimental beam energy balance that produced a radiation drive that was symmetric in a time integrated sense. Other issues such as the capsule instability then became the principle limiting factors in performance. Having eliminated or circumvented these issues, the program has now returned to a point where CBET, or rather the time dependent radiation drive asymmetry which arises from it, is again the limiting issue. Viewfactor experiments, where one laser entrance hole is removed to allow end-on soft X-ray imaging, have concentrated on addressing assessing the efficiency of converting laser energy to X-rays as well as the time integrated symmetry of the radiation emission from the hohlraum wall. There has been little attention given in such experiments to assessing the time dependence of the radiation symmetry which is responsible for introducing swings in the capsule shape during implosion. It is not clear to me whether all of the options to try to mitigate time dependent drive asymmetry have been fully explored. Perhaps it is possible to use different pulse shaping on the inner and outer beams to provide some time dependent control of CBET, or to design a shimmed capsule with a graded ablator or dopant thickness? Whilst the effects of the capsule mounting tent on performance may not be as significant as the time dependent radiation asymmetry, engineering solutions designed to reduce perturbation levels can be directly evaluated through inflight radiography diagnostics. The relative stability of the current best performing capsules means that the perturbation induced by the capsule mount will be at the limit of diagnostic resolution when the implosion is approaching the axis. Measurements to evaluate symmetry improvements from different capsule mounts will therefore be considerably enhanced by improvements to diagnostic imaging systems for the 2DConA or other back-lighters. Alternatively the capsule may have to be re-designed to increase instability growth in order to enhance the amplitude of the perturbation to diagnosable levels

Choosing to address the CBET issues by concentrating on low fill hohlraums has brought about the need for development programs not only for the hohlraum platform itself but also for capsule designs based on high density carbon and beryllium ablators. The relative immaturity of these designs compared to the gas filled hohlraum and the CH ablator capsule, means that these development programs represent a significant fraction of the overall ICF campaign over the next 3-5 years. Whilst there is optimism that initial experiments have demonstrated reduced radiation asymmetry with low fill hohlraums, there is no guarantee of increased overall yield in integrated experiments. In addition to their suitability for use in the low fill hohlraum, high density carbon and beryllium ablators offer improved stability properties compared to CH and therefore provide the potential to broaden accessible parameter space by allowing higher implosion velocities or higher convergence ratios to be used. Important considerations for the design of the next generation of hohlraum wall materials are not only reductions in wall blow off plasma, but also a reduction in m-band or other high photon energy components which will reduce the need for dopant layers within the capsule and maximize the stability benefits of choosing a high density ablator.

Considerable progress in the theory and simulation of indirect drive has been driven by the large volumes of high quality diagnostic data provided by the NIF. There remain however a significant number of limitations to predictive capability which inevitably mean that the experimental exploration of parameter space is constrained to incremental departures from a place of known performance or is in some way empirically led. Areas where the biggest uncertainties lie are the hohlraum modelling and the stagnation phase. In hohlraum modelling it is difficult to be quantitative about the degree of laser plasma instability and cross beam energy transport, which means that the X-ray conversion efficiency and symmetry are typically dealt with using either ad-hoc multipliers or semi-empirical fixes. These approaches provide a good representation of experiments where minor variations are made from a base design, but must be re-calibrated when more significant changes in design are made. With the perturbation amplitudes apparent in current experiments, the stagnation process is an intrinsically three dimensional process. Where discrepancies lie between experimental observation and 3D simulation, it is not clear whether these are due to deficiencies in the way in which the hotspot is modelled or whether the discrepancies arise before the start of the deceleration phase, i.e. the models of the stagnation phase are simply working from the wrong set of initial conditions. Simulations of the emitted neutron spectra are an important prediction for whether key indicators of the hotspot temperature and velocity may be observable. Anisotropy of the neutron spectrum is a clear indication of a net center of mass velocity in the hotspot which is indicative of a low mode asymmetric implosion. An isotropic distribution does not eliminate the possibility of higher mode asymmetry giving rise a more turbulent residual velocity at stagnation. Differences between the DD and DT ion temperatures inferred from neutron spectra indicate that the calculated spatial temperature distribution may be incorrect.

Recommendations: There will always be a need for a balance in programs such as this between keeping the number of designs to a manageable level where they can be systematically investigated within the resources available and the need to maintain the flexibility to be open to new ideas. Within some parts of the program there is a potential to only taking small steps away from a regime that is known to work. This is a conservative approach which will only follow a trajectory through parameters space if the path is a contiguous series of positive results. Scientifically I find this approach admirable, but my concern would be whether this leads to incremental progress which would limit the exploration of parameter space over the 3-5 year time frame.

The low gas fill hohlraum designs and the high density ablators represent a logical, relatively low risk approach to tackling the principal factors limiting performance over the next few years. At this stage there is no way to accurately predict the performance of integrated experiments, but in the event that these designs reach a similar performance ceiling to the current high foot shots, then it is important that alternative concepts, which may be higher risk in nature, are sufficiently matured to be then considered. It is encouraging to see that alternative concepts based on radically different capsule designs or which are not targeted at high gain yields, such as the double shell and Big Foot designs are coming forward. I think it is important to maintain a balanced portfolio of options at different stages of readiness and with different levels of risk and payoff. It is also important to encourage input from a broad and diverse range of contributors into the development of new ideas and to maintain the flexibility to introduce new concepts into the shot plan.

Ignition Approach: Laser-driven Direct Drive

Findings: Laser driven direct drive provides a intuitively complimentary approach to indirect drive which offers advantages for increased energy delivery to the hotspot and therefore reduces the convergence required for ignition. The quoted program goal for laser driven direct drive is to scientifically and technically justify the reconfiguration of the NIF for Spherical Direct Drive. This is being pursued through spherical direct drive (SDD) illumination experiments on the Omega laser at LLE and polar direct drive (PDD) illumination experiments on the NIF at LLNL. In this regard PDD is not being directly pursued as a path to ignition itself but rather as a platform to assess the scientific case for adopting SDD on the NIF. The laser driven direct drive program has seen some significant achievements over the last three years. The implementation of the PDD platform on the NIF is a major accomplishment as is the attainment of 50 Gbar hotspot pressure on the Omega laser.

Amongst the principle objectives of the program are goals related to both hydrodynamic implosions and to cross beam energy transport (CBET) arising from laser plasma instabilities (LPI). For hydrodynamic implosions, the goal for SDD integrated DT cryogenic shots on Omega is the demonstration of an implosion which is hydrodynamically equivalent to a SDD implosion on the NIF at 1.8MJ. For PDD experiments on NIF the aim is to perform a high convergence ratio (Cr~20), non-cryogenic implosion. It is progress towards the understanding and mitigation of energy losses due to CBET however which appears to be the principle driver in determining overall progress towards the program goal. In particular, as stated in the laser direct drive white papers, it appears that without significant CBET mitigation, there is no credible concept for direct-drive ignition at NIF-scale energies. For this reason the principle aim of PDD experiments on the NIF is to provide a platform to test strategies for CBET mitigation on density scale lengths which are significantly larger than can obtained on Omega and are within a factor of two of those which will ultimately be encountered in SDD experiments on NIF.

The strategy for mitigation of the CBET in PDD on the NIF is different to that in SDD on Omega. As the capsule implodes and becomes smaller than the laser spot sizes, there is increased overlap of the beams and hence an increase in the level of CBET. To counter this effect, in the near term LLE are developing ‘zooming’ phase plates which produce a reduced focal spot late in time as well as a full-aperture zooming system based on a grating with an axicon, over the 3-5 year timescale. PDD shots on the NIF will instead concentrate on the use of increased range of laser wavelengths as the approach to CBET mitigation. Should the NIF be reconfigured to SDD however the CBET mitigation strategy would instead be more closely aligned with that on Omega and would employ combination of zooming and wavelength detuning.

Mitigation of the CBET could itself give rise to plasma conditions where the two plasmon decay (TPD) instability generates significant hot electron preheat. Methods of reducing the impact of these effects will be addressed in focused physics experiment in SDD on Omega and in PDD on NIF by introducing layers of intermediate Z material.

Comments: Proving the scientific case for investing in SDD on the NIF and in particular proving that the known issues such as CBET can be mitigated represents a significant scientific challenge particularly where not all of the physical conditions necessary for such a test can be accessed with existing facilities. Significant investment in the PDD platform on the NIF will be required in order to enhance the ability to explore mitigation of CBET through wavelength detuning approaches. In the event of conversion of NIF to SDD however, the approach to CBET mitigation would be different and would require the extrapolation of zooming techniques developed on Omega to plasmas of much larger density scale lengths. It is inevitable that when scaling up a design to a larger platform, not all of the parameters ranges that will be encountered can be fully explored beforehand. It is therefore important that the data obtained in both PDD on NIF and SDD on Omega are utilized to inform and constrain theoretical and computational models which will be essential for underwriting the scientific case for SDD on the NIF.

The direct drive program has extensive experience and computational capabilities for the modelling of CBET and TPD preheat which are benchmarked against extensive data sets from experiments on Omega. Predicting the behavior of LPIs in plasmas with density scale lengths which would be a factor of four larger in SDD on NIF will stretch the capabilities of these models. It is therefore important that full use of the data from PDD on NIF is made in benchmarking the extrapolation of the models to the midpoint between Omega SDD and NIF SDD. Similarly it is clear that high quality computational capabilities exist at LLE to model the hydrodynamic convergence of SDD implosions on Omega in both 2D and 3D. What is less clear is the level of confidence in the ability to model polar direct drive experiments with the added complexity that the variation in incidence angle has on laser transport and ablation processes as well as CBET. It is possible that improved convergence of simulation and experimental observation for PDD will require larger scale 3D simulations to resolve the combined impact of these effects on laser imprint and implosion symmetry.

Recommendations: Reconfiguring the NIF for spherical illumination direct drive experiments, would require not only a substantial financial investment but also a significant interruption to indirect drive research as well as a range of other non-fusion programs. The bar will therefore be set very high for the scientific case to be sufficiently persuasive to undertake this change. The current timetable for direct drive for the next five years is well matched to the objectives of exploring CBET mitigation and moderate convergence shots in PDD on the NIF as well as developing zooming techniques and achieving high pressures in hydrodynamically equivalent shots in SDD on Omega. Meeting these objectives however will not necessarily provide a complete and compelling case for SDD on the NIF and it may be that such a decision point would not be reached until further into the future. Should it prove necessary however to bring forward such a decision point then this can only be achieved by investing sufficient funds to allow an acceleration of both the SDD program on Omega and the PDD program on NIF.

Ignition Approach: Magnetically-driven Direct Drive

Findings: The last few years have seen considerable progress in the development of the MagLIF fusion concept at Sandia. The achievement of fully integrated shots incorporating liner implosion, magnetization and laser preheat represents a significant milestone. A central component of the MagLIF design is the magnetization of the fuel which reduces heat losses from the fuel to the liner in the present experiments on Z and could also reduce the requirements for ignition on a next step facility by providing confinement of the alpha particles.

In common with other inertial fusion concepts, the first fully integrated MagLIF experiments produced fusion yields which were significantly lower than predicted by 2D MHD simulations. Whilst these yields were also low compared to those other fusion Z-pinch plasmas at similar currents (such as deuterium gas puff experiments on Z), the MagLIF experiments were the first to demonstrate reasonable yields in a small plasma volume, dense enough and magnetized enough to provide a scalable path to ignition.

A number of candidate mechanisms thought to be inhibiting the fusion performance have been identified, based on experimental observations and 3D MHD simulations. These include the non-uniformity and reduced efficiency of the laser energy absorption, hydrodynamic mix of the liner and fuel, mass loss through the laser entrance hole, enhanced radial heat flow due to extended Ohm's law effects and reduced convergence due to 3D asymmetry at stagnation.

Sandia have presented an extensive program plan and set of goals to investigate these effects through diagnostic and computational improvements as well as targeted physics experiments.

Comments: The decision to turn away from the use of wire array Z-pinches for indirect drive experiments came as something of a surprise to some in the community as significant progress was being made at the time with double ended vacuum hohlraums and dynamic hohlraums. In retrospect however this decision seems logical as exploration of the X-ray driven indirect drive concept is now being pursued very effectively on the National Ignition Facility. Nevertheless, should it prove that the pursuit of ignition and high yield using indirect drive on the NIF is ultimately limited by the maximum energy that can be coupled to the target, there may still be an opportunity to explore alternative indirect drive designs, with larger absorbed energies, on a future larger scale pulsed power facility. It is important that the scientific capability to mount such a campaign in the future is retained at Sandia.

The change in direction of the fusion program at Sandia to magnetically driven direct drive has led to the development of a viable alternative approach to laser driven approaches which uses the efficient coupling of energy to the fuel to good effect. The larger scale of the targets involved in this approach means that there is potential for relatively high yields (100kJ) in non-igniting experiments on the current Z facility and very significant (GJ level) yields should an ignition experiment be successful on a 50-70MA class driver.

The MagLIF fusion program at Sandia is largely concentrated upon evaluating a single computational design. Whilst many of the design aspects for MagLIF are constrained by the generator and laser parameters available, the main constraint appears operational in that there are insufficient shots available to thoroughly evaluate more than one design. This is a little concerning in that should the current limitations to performance ultimately prove insurmountable, there would be a limited choice of mature alternatives. It is not immediately clear to me how alternative designs, which go beyond simple variations on a theme, could be grown from a nascent idea to a viable alternative given the current constraints.

Whilst there is a broad program designed to address those factors so far identified as potentially limiting performance, much of the initial emphasis has been placed on measuring the fraction of the laser energy absorbed by the deuterium gas during the preheat phase. This is perhaps not surprising since this issue can be addressed relatively straightforwardly in offline experiments in collaboration with LLE. It is important however that sufficient resources are applied to developing the diagnostics required to address the other concerns in parallel, so that these are well underway should the laser absorption issue prove not to be solely responsible for the present performance. It is also important that other approaches to heating can be considered should the problem with the laser heating prove insurmountable. Just as an example, I have found through my own simulations of MagLIF that an end-on dynamic hohlraum provides a more energy rich and isotropic heating source than a laser.

Recommendations: Delivery of increased currents, magnetic fields and laser energies to MagLIF are important goals for the future of the concept and for the ongoing development of the facility, but it is important they are not the principle drivers for assessing success. In the present experiments the fusion yield remains a small fraction of that predicted by 2D MHD design calculations. If this fraction remains the same in experiments at higher drive parameters, then little will be achieved either in terms of improved understanding or in the viability of a next-step generator. As with the other approaches to ICF, assessment of progress towards improved understanding of those factors currently limiting performance will be more important than milestones based on numerical metrics. As with the other inertial fusion approaches, direct diagnosis of the hotspot conditions is extremely difficult. This is made even more so by the large ρR of the liner surrounding the fuel at stagnation. Results from the NIF have shown that there is a wealth of information embedded within the neutron spectra. Sandia have made considerable progress in this regard with measurements of primary DD spectra and secondary Triton reactions. The introduction of Tritium handling capabilities at Sandia however would mark a considerable improvement not only through increased yield, but by introducing a range of new diagnostic options for assessing hotspot ion temperature, plasma motion and beam-target contributions.

Over the past few years Sandia has assembled a portfolio of compelling arguments for the benefits of building a next step pulsed power facility based on results in material science, pulsed neutron and X-ray sources and fusion platforms. If such a project is to succeed, it is critical that there is sufficient investment in programs to evaluate the pulsed power technology in the near term, in order to be able to design and build a new facility in a few years' time. The construction of large scale LTD modules in the near term is essential for the assessment of not only the scalability of such technology to the currents and voltages required, but also the economy of scale in the manufacturing of large numbers of modules which will be a major driver for determining the cost of a new facility. Similarly experiments to either assess the ability of present or alternative designs for the convolute, diode stack and MITL to withstand the additional power flow requirements, will need to be undertaken in the near term.

Roadmaps and Decision Processes

Recommendations: The major decision point which is visible on the horizon will be how long to continue with indirect drive on the NIF in its current configuration. At present there is a clear and well thought out program for the next 3-5 years to address intuitive approaches to improving the fusion performance. There is a high confidence in the ability of the program to continuously deliver high quality and high profile work which will provide a steady improvement in our understanding of the science. What cannot be assured at this stage is that the current set of designs will not encounter similar performance ceilings to those designs already tried and therefore the maximum yield obtained may not increase during this time. In several design scenarios reduced performance can often be compensated for by increased drive energy. Should the energy available from the NIF prove to be an ultimate limit then options for increasing the energy coupled to the capsule include reengineering the laser to operate at green wavelengths or reconfiguration of the facility to spherical direct drive illumination. The scientific case for adopting any proposed alternative approach would already need to be well established in order to be able to inform the decision making process. The timescale for developing a compelling scientific case for implementing direct drive on the NIF is at present determined by the five year program to evaluate mitigation strategies for CBET. It is possible however a significantly longer program will be required to comprehensively provide all of the information required to inform a decision. It is important therefore to consider whether these timescales are sufficiently short to meet the required timetable for informing a decision about reconfiguring the NIF.

At present there is no capability worldwide which can completely asses the scientific feasibility of direct drive ICF using spherical illumination at the scale required for ignition. One discussion which has surfaced a number of times within the European IFE academic community is whether there is a need for intermediate scale (sub-ignition class) laser facility of the order of a few hundred kJ which could thoroughly test the scientific issues associated with a spherical direct drive approach. This conversation arises because the absence of any existing ICF implosion capability of the scale of Omega within Europe makes the extrapolation to PDD experiments on LMJ more problematic than in the case of NIF. Should the decision point for implementing SDD on the NIF however be pushed back to well beyond the 5-10 year timeframe it might be worth considering whether evaluation of different approaches to addressing the current issues were better undertaken at a larger scale. This would strengthen the case and reduce the risk associated with conversion of the NIF to spherical direct drive.

Magnetically driven inertial confinement offers a viable alternative approach to achieving high yield fusion in larger scale cylindrical implosions with the possibility of a different class of GJ high yield experiments. Sandia has in place a detailed program to address current factors limiting fusion performance in the present MagLIF design and to maximize the current, magnetization and laser heating available on Z. Should these experiments prove successful, there would be a fair degree of confidence that the MagLIF concept could be extrapolated to provide significant fusion yields on a higher current pulsed power driver. It is important to note that there are a number of other valuable applications of a next stage pulsed power driver in material science and X-ray sources where there is also confidence in the extrapolation to higher current. In addition there are other fusion concepts such as high yield indirect drive or non-igniting pulsed neutron sources such as deuterium Z-pinches which could also be exploited on a larger scale pulsed power driver. It is important that sufficient resources can be applied to assessing the pulsed power technology of large scale LTD drivers in the near term in order to provide a mature design that could inform the decision process on the same timescale that MagLIF experiments on Z are anticipated to reach their design yields.

II. Assess the integration of experiments and codes

Diagnostics

Findings: The National Diagnostic Plan is an excellent example of inter- laboratory cooperation to establish a national strategy for the systematic improvement of diagnostics technique across all ICF platforms.

Comments: In the words of Lennon and McCartney - “Living is easy with eyes closed. Misunderstanding all you see”. There are numerous examples where the development of a new diagnostics has revealed structures that were not known to exist beforehand and have transformed our understanding of the structure of the plasma. In the absence of a fully predictive computational model, it is the development of new diagnostics which is the principle driver for making new discoveries and reducing uncertainty. It is often the case that the overall rate of scientific progress can be directly linked to level of diagnostic investment.

The list of eight priority diagnostics identified by the National Diagnostic Plan management group are indeed transformative in that they would revolutionize the quality of data and the ability to inform and constrain models on all three approach to inertial fusion.

At present one of the principle limits to our understanding of the stagnation phase on the NIF is knowing the exact form of the perturbation of the dense fuel during the last few hundred microns of the implosion. This structure determines the pressure of the hotspot that will form and the symmetry of the dense fuel that must confine it and retains a fossil record of the drive asymmetries and capsule defects which have provided a source for the perturbation. Improvement of the spatial and temporal resolution of radiography at this phase of the implosion can have a critical impact on constraining 3D models of the stagnation phase.

Computational Models and Predictive Capability

Comments: There has been an increasing realization that the deceleration and stagnation phases of all three approaches are intrinsically three dimensional processes. In some sense this is good news as three dimensional models with suitable perturbations tend to fit the experimental data better than 2D. The degrees of freedom in a 3D perturbation are however far greater than in 2D and the data required to constrain the form of the perturbation is much harder to quantify. 3D models do not therefore offer predictive capability and are unlikely to do so without being constrained by more high quality diagnostic data.

Recommendations: It is important to maintain a range of different code capabilities. There is a case to be made that if all designers are using the same general purpose radiation hydrodynamics code, such as HYDRA then this maximizes the user base and enhances the overall experience level as well as code development. This approach has to be tensioned against the need for codes to be cross checked against other models with alternative computational methodologies and physics packages. Maintaining a healthy diversity of different models is essential, as is maintaining the capability to develop independent code capabilities. There is also a need to maintain specialist codes for specific problems such as laser ray trace hydrodynamics at LLE and MHD at SNL.

III. Assess cross-platform and cross-laboratory collaborations

Comments: High Energy Density Physics is a notoriously ‘driver –centric’ research field. All too often researchers will adopt the thought process of ‘what can I do with the machine I have?’ rather than ‘which machine can I use to access the physics I’m interested in’. Whilst there are many differences in the drivers themselves, the physical processes involved in achieving fusion through implosion are remarkably similar. It is therefore most refreshing to see an approach developing between the US labs involved where working groups such as The National Diagnostic Group and the National Implosion Stagnation Physics Group are established to help advance the understanding of the physical processes common to all approaches.

Recommendations: Each of the three approaches to inertial fusion is presently engaged in a scientifically driven research campaign where it is the progress achieved in understanding the physical processes at work and how these influence the fusion performance which is the principle measure of the success of the program. Milestones based on purely numerical metrics are not necessarily the most effective way of assessing the performance of such a program. Non quantitative measures of progress which assess improvements in the level of understanding can however be subjective and inaccurate. It is only through an active peer review program that independent assessment of the quality of the research can be achieved. Peer review should not be limited to purely retrospective assessment of achievements but should be seamlessly built into processes for evaluating new concepts, determining the balance between campaigns and assessing priority research directions. It is only by providing the transparency which is brought by external participation in the decision making process that a complete appreciation of prioritizations within the ICF program can be recognized by the external HEDP community.

Reviewer Report: Siegfried H. Glenzer

I. Assess the scientific hypotheses and the prospect for achieving ignition with existing scientific capabilities and facilities; or, if indicated, what would be required to achieve ignition and supporting analysis. Provide an evaluation of program balance among ICF approaches.

DOE-NNSA has made significant investments in major facilities and high performance computing to ensure present and future scientists have the means to successfully execute the Stockpile Stewardship Program. Of high importance is the demonstration of ignition and burning fusion plasmas in the laboratory. The laboratory directors have expressed that the demonstration of fusion in the laboratory is a high-priority goal and have pointed out the importance for the US to be the first nation to succeed. The program is pursuing three viable approaches: laser-driven indirect drive, direct drive, and magnetic direct drive implosions. During the review, the ICF programs have clearly communicated their present understanding of the physics issues in each area and have described their experimental plans that are based on precision experiments and designs using computations with state-of-the art radiation-hydrodynamic modeling. The complete program that was laid out gives reasons for optimism that significant progress will be made in the near future. If successful, fusion yields due to alpha heating in indirect dive implosions on NIF will increase towards conditions favorable for nuclear burn waves that will approach physics regimes important for nuclear weapons.

<u>Summary statements on the specific question for each ignition approach</u>

For Laser-driven Indirect Drive: What understanding/performance metrics/criteria are used to measure progress/determine endpoint for HDC, Be, CH ablators target designs, and for vacuum/near-vacuum hohlraum designs? What is the path to high yield and high areal density implosions?

The highest priority of the indirect drive program should be the demonstration of 100 kJ yield in indirectly driven implosions. These plasmas will test our understanding of alpha heating physics and are critically important for measuring alpha stopping and initiation of nuclear burn for achieving high DT fusion gain. The program has developed lower convergence high-velocity implosions that are suitable candidates; it is recommended to develop 2 platforms in parallel to achieve this goal within the next 3 years. A precision implosion diagnostics program should be pursued vigorously to quantify what it will take to launch a burn wave. The path to high yield and high areal density will require additional improvements in hohlraum drive symmetry and radiation hydrodynamic modeling that will need to be pursued in parallel.

For Laser-driven Direct Drive: Is 80 Gbar hot-spot pressure in OMEGA DT layered cryo implosions, in and of itself, a reasonable go/no-go metric for proceeding to NIF experiments? What are good understanding/performance metrics/measures of success for symmetric/polar direct-drive? What are the scientific questions that need to be addressed and decision path for the choice for polar drive and/or symmetric drive ignition on NIF?

Demonstration of 80 Gbar pressure on Omega DT implosions will be a very significant result. This result would be a reasonable metric in favor of symmetric direct drive implosions on the NIF provided that a) progress in yield and pressure of indirectly driven implosions is slow and struggles at delivering 100 kJ yield, and b) radiation-hydrodynamic and laser-plasma interaction predictions for direct drive implosions are successfully tested with adequately performing cryogenic polar direct drive implosions.

The direct drive laser approach promises ignition and significant fusion yield for plasma conditions that appear to be easier met than indirectly driven implosions. However, relaxing the plasma pressure requirements in the proposed way makes driver and experiment fielding requirements significantly harder to meet. An aggressive campaign of the direct drive program on the NIF is in order to assess the full scope of performing these extremely challenging experiments.

For Magnetically-driven Direct Drive: The path to ignition with MDD appeared to be based upon driver enhancement goals (ever larger/higher power facilities). What are the scientific benchmarks or understanding goals underpinning the MagLIF approach to ignition?

The present approach promises ignition and significant fusion yield on future facilities for plasma conditions at much lower pressures than required for indirectly driven implosions that are presently being performed on the NIF. The costs of fusion with reduced plasma pressure requirements in MDD implosion are higher experimental complexity and the need to develop predictive modeling that will need to include 3-D physics with magnetic fields. This task is extremely challenging and will require close collaboration between the laboratories.

At the highest priority, the MDD program must demonstrate laser beam propagation and heating on Z. The goal is to deliver integrated performance with effective kilojoule-laser heating of the fusion target. Much of the non-predictive behavior of some past experiments is presently being explained by insufficient laser beam propagation in the target. This factor will need to be ruled out to enable future optimization studies and to determine the scaling to high yield. To make quantitative measurements of implosions will require simultaneous laser heating and laser backlighting experiments.

Ignition Approach: Laser-driven Indirect Drive

Findings: Since 2010, two experimental laser-driven indirect drive campaigns have been completed on the National Ignition Facility at Lawrence Livermore National Laboratory. The teams executed series of indirectly driven layered DT implosions with the goal to achieve fusion burn in the laboratory and demonstrate fusion gain that exceeds 1 MJ yield (~3 x 10^{17} DT fusion neutrons). As of today, these experiments have not reached the burn regime, but significant progress has been made, most importantly the first unambiguous demonstration of alpha heating. The highest fusion yield shots have achieved close to 10^{16} DT fusion neutrons in implosions with 26 kJ of fusion yield. Half of the yield is from alpha heating. This result is important and encouraging because a plasma regime with significant alpha heating is a critical experimental step needed to launch a nuclear burn wave followed by rapid 10-fold increase of temperatures and large fusion yields from burning the surrounding dense fuel.

It is important that the NIF experiments have used laser and target parameters that were very close approximations of the laser-plasma interaction and radiation-hydrodynamic design calculations for ignition. These findings indicate that predicting the physics of ignition through simulations is much more challenging to than previously thought. Low-adiabat implosions (so-called low-foot implosions) show large areal densities close to simulations and close to those needed for high-fusion gain implosions. However, the experiments have resulted in low fusion yields (~10^{15} DT fusion neutrons) suggesting that the hot spot of the implosions is not adequately forming. Specifically, hydrodynamic mix or shell break-up occurring during the final compression phase from radii of 100 microns to about 25 microns are leading candidates to explain the behavior. Utilizing highly accurate target implosion diagnostics and focused experiments have led the way towards providing measurements of 1) mix from comparisons of x-ray and neutron yield, 2) non-uniform fuel areal densities from variations in scattered neutrons, and 3) x-ray radiograph images of shell perturbations. These measurements have helped providing the hypothesis for explaining low yield in low-foot implosions. Importantly, the x-ray radiographs have shown evidence for shell perturbations caused by the capsule tent, which holds the capsule in place inside the radiation cavity, i.e., the hohlraum.

Significant progress has since been made and alpha heating has been observed when switching to higher-adiabat DT implosions (so-called high-foot implosions). This strategy trades large areal densities with conditions that have significantly smaller growth-factors for hydrodynamic mix and thus behave more favorably for hot spot formation. These implosions have resulted in the highest neutron yields so far from laboratory experiments of ~10^{16}. Focused experiments have corroborated the picture that these implosions are more stable by showing no or reduced capsule tent features in 2-D radiographic imaging and direct growth factor measurements have validated the low-growth rate scenario for mix. These experiments have shown good agreement with simulations and post-shot simulations agree with yield data to within a factor of ~2. However, when using high-foot implosions to further heat the hot spot by increasing implosion velocity and convergence the capsules start to fail and the fusion yield has been observed to decrease. The leading hypotheses for the failure are hohlraum drive asymmetries and perturbations seeded by the tent. Without any further improvements and developments the high-foot implosions would need ~3x more energy in the fuel to ignite. Although improvements in optics will occur over the next 5 years, significant progress towards ignition on the NIF should be expected with the existing scientific capabilities and facilities.

The indirect drive experimental program is presently pursuing 4 goals with high priority. The first thrust area takes advantage of the knowledge base generated during the low-foot and high-foot campaigns and will be fielding so-called big foot implosions on the NIF. These experiments will test the hypothesis that high convergence results in shell breakup due to hohlraum drive asymmetries. Due to lacking of an optimized hohlraum, these shots will use near vacuum hohlraums with diamond ablators that show very little laser-plasma interaction physics effects and where the total hohlraum radiation drive is close to predictions from radiation hydrodynamic modeling. The goal is to demonstrate stable implosions with adequate hot spot formation at a limited convergence ratio of ~20. The shots should achieve high implosion velocity of 450 km/s at high adiabat with 100 kJ of DT fusion yield.

As high priority, the indirect drive ICF program will be developing a predictable hohlraum with improved symmetry control to ultimately achieve high convergence implosions for a burning fusion platform. The phase space consists of variations in fill pressure, case-to-capsule ratios and capsule ablator materials including CH, Be and HDC. The design space is large and it will be challenging to make progress without detailed measurements of the shell behavior close to stagnation in non-layered implosions. Other effects could confuse the answer especially effects from the capsule tent, or variations in laser-plasma interactions especially for targets with different materials and scale lengths.

It is recognized that the program will need to develop and field precision diagnostics of the hohlraum plasma conditions and drive symmetry. This includes imaging, spectroscopy, x-ray and particle scattering measurements. These diagnostics are expected to greatly improve knowledge of the implosion by visualizing shell break-up and the effects on hot spot formation and resulting residual kinetic energy. Importantly, the diagnostics are expected to provide new insights into the modeling of the high-energy density science experiments. In addition to measuring the conditions of fully integrated DT implosions they will provide capabilities for critically testing predictions of the modeling and theory in focused experiments. For example, accurate measurements of the hohlraum temperature near the laser entrance hole with Thomson scattering will allow distinguishing between competing models for the hohlraum plasma and drive parameters and will contribute resolving the issue of missing energy in the hohlraum.

The program has committed to the long-term development of the radiation-hydrodynamics code HYDRA for the design of implosions. A wide range of ICF-relevant physics packages is being developed and implemented. The code has been tested against a large database of integrated and focused experiments. This knowledge is kept in a configuration-controlled database. The continued development of the code, its future capability for use with high performance computing, and the inclusion of direct drive-relevant physics will benefit the whole National ICF program and is of general importance for the stockpile stewardship mission.

<u>Comments:</u> The pursuit of big foot implosions, or more generally of implosions with reduced convergence, is an important new feature of the program. Investigating a regime that is better understood and where the integrated performance data follow more closely the radiation-hydrodynamic modeling appear to be the correct strategy for improving fusion yield performance and possibly isolating important physics mechanisms. Improving the physics models in the design codes is critically important for successful ignition attempts. This thrust reflects the change in the guiding principles of the indirect drive program away from high-yield high-gain implosions to a regime that is better understood and that allows steady progress towards ignition. Preparing implosions that use wetted foams or double shells follow a similar idea of making implosions more 1-D like by reducing convergence. However, while important for future directions, wetted foam targets are challenging to build and the big foot implosions will likely test the performance of low convergence ratio implosions first. Similarly, the plan to pursue double shell implosions sounds attractive, but the program will need to build more confidence with focused experiments before investing significant resources in target fabrication and before new precision diagnostics are ready to make quantitative measurements in this more challenging regime. A key question is to resolve if sufficient drive can be generated on NIF to successfully implode double shell capsules.

The development of a more predictable hohlraum is an important activity. However, a detailed plan will need to be developed that includes milestones and success criteria. Without performing large case-to-capsule implosions it will not be clear how 1-D implosions really look like on NIF and if other performance cliffs are important. This area of research includes many variables calling for the need to develop a clear direction that defines the main program and provides focus and priorities for resources. The pursuit of new ideas should be encouraged and managed with well-defined finite shot resources.

Importantly, the poor performance of implosions with Be ablators give raise to concern that laser-plasma instabilities, i.e., CBET, stimulated Brillouin scattering, stimulated Raman scattering and hot electron generation are a dominating factor. The issue of Be ablators and the differences in performance compared to near vacuum hohlraums will need to be better understood. There are doubts about the early interpretation of the missing radiation drive in hohlraums; view-factor experiments have led to the conclusion that it is more important to focus on the physics of the hohlraum's conversion of laser light into x rays as opposed to the ablation model for the capsule. This early conclusion is questionable since subsequent experiments in near vacuum hohlraums show no discrepancies with the x-ray drive models. This is not a surprise because the hohlraum drive model was motivated by a large experimental database that included empty hohlraum shots. More importantly, the view factor shots have shown that laser-plasma interactions in gas-filled hohlraums are not well understood and implosions in near vacuum hohlraums will deliver more drive to the capsule.

Alternatively, the lack of predictability of implosions could be explained by capsule tent perturbations. Solving this issue seems to have taken a back seat during the high-foot campaign. Preparations for improving the capsule mounting should be more advanced and better prepared with radiation-hydrodynamic simulations of various realizations for the capsule mounts.

There are other trends in the current experimental database that are not completely understood. Examples are pressure versus coasting time or the pressure data from the adiabat shaping experiments. It is important to develop hypotheses and to pursue experimental tests that should be preferably performed with near vacuum hohlraum implosions and with improved capsule mounts.

Recommendations: Fielding big foot implosion experiments on NIF should be pursued at highest priority. The results will provide an important test of the new figure of merit replacing implosion velocity with capsule convergence. If validated, this result will have important consequences for future planning and will motivate fielding designs on NIF to deliver 100 kJ DT yield in the next three years. At a minimum, two independent 100 kJ designs should be pursued, for example, high-density carbon in near vacuum hohlraums and a high-foot beryllium design.

It is a high priority to vigorously pursue the National target diagnostic plan. This program has delivered a plan to develop, build, and field a new generation of precision diagnostics to measure hohlraum and implosion performance. In particular, future experiments will need to accurately measure the hohlraum plasma conditions to test radiation-hydrodynamic modeling of drive symmetry. Further, future diagnostics must measure the integrity of the shell during the formation of the hot spot. Importantly, the program should develop advanced diagnostics that can measure the residual kinetic energy with Doppler spectroscopy and test the microphysics modeling used for the dense plasmas of the implosion utilizing particle and x-ray scattering methods. New diagnostics specifically aimed for providing precision data on novel future implosions for NIF such as double shell targets or directly driven implosions should also be within the scope of the program.

The ICF program should put a high bar on focused physics campaigns to make sure that the results are not masked by other effects (laser-plasma interactions, tent etc.). It is important that shots deliver critical experimental tests of the physics models used in the radiation-hydrodynamic codes. In addition to examples mentioned above, a focused campaign with precision measurements of 1-D implosions especially with large case-to-capsule ratio experiments should be a priority.

The ICF program will need to implement the best possible physics models into the radiation-hydrodynamic simulations. New experimental and theoretical results from other experimental programs have shown that orbital free (Thomas-Fermi) modeling provides a poor approximation. This is a challenging area where better approximations appear possible with the potential to produce results that will affect future ICF designs. Further, many calculations of NIF capsule implosions result in an ionization state of carbon in the dense ablator plasma close to 2 while advanced modeling and experiments have shown Z=4. Possible consequences on opacity and heat transport will need to be investigated. In particular, advanced models such as those that use detailed configuration accounting, non-LTE physics, and continuum lowering physics should be routinely used and tested in experiments, possibly at Omega and medium sized facilities.

To develop guides for experiments planning of integrated shots and focused experiments it is important to develop a simulation database that evaluates performance degradations of DT implosions due to possible errors and uncertainties in the microphysics. Such calculations should also help to better understand the existing experimental databases. An example where assessments are needed is the difficult regime of Warm Dense Matter where uncertainties in the heat conductivity could affect the growth of Rayleigh Taylor instabilities. In addition, simulations of hot-spot formation and burn truncation could be affected as a result of larger conduction losses than calculated with present models.

Ignition Approach: Laser-driven Direct Drive

Findings: The direct drive ICF program has demonstrated significant progress in their understanding of crossed beam energy transfer (CBET) that has led the team to successfully field a series of precision cryogenic implosion experiments at the Omega laser facility resulting in significant hot spot pressures of 56 ± 7 Gbar. These pressures are the result of recent additions including improved illumination uniformity using SG-5 phase plates and a "poor-mans" form of zooming by turning off Smoothing by Spectral Dispersion (SSD) at the peak of the laser drive. The experimental DT fusion neutron yield has reached 45% of the clean calculated 1-D value. Especially in the regime of moderate convergence ratios (<17) and moderate fuel adiabats (α > 3.5) the experimental pressure values are very close to 1-D performance. In this regime, pressure values of 90% and areal density values of 100% of 1-D calculations are reported.

Higher convergence and low-adiabat implosions show performance cliffs. Simulations indicate that mix due to so-called target debris or capsule impurities is affecting the attainable hot spot pressures. A reduction of up to a factor of 5 was found. Further, the program is actively investigating 3-D effects due to low-mode asymmetries induced by, e.g., laser power imbalance, target offsets, and beam miss-pointing effects. Comparisons with simulations indicate burn truncations significantly reducing the attainable pressure. In general, the database shows distinct trends that are not completely understood. For example, smaller capsules with 800-micron diameter show the best performance while larger diameter capsules perform poorly despite the fact that CBET is reduced for larger diameter targets. Reducing the target diameter will come at the cost of reduced fuel and consequently less overall yield. If scaled to full-scale ignition experiments this trend will make it harder to get to alpha heating.

The direct-drive team has developed a project and campaign plan to reach implosions with peak pressures of 80 Gbar at the Omega laser. If this performance is achieved it would provide a major impetus to become a candidate for the development of a burning fusion platform. The main technological advances needed include sub-aperture and full-aperture zooming to increase the kinetic energy of the implosion. Also, reductions of low-mode and high-mode mix, and optimizations of the capsule adiabat to increase the hot spot pressure are important. In addition, the program is pursuing high-Z capsule coatings and dopants to reduce mix.

The predictions for direct drive ignition on NIF (gain of 1) show that the target requirements are in a significantly less challenging hydrodynamic regime than current indirectly driven capsules. The design use a convergence ratio of less than 25 and hot spot pressures of about 150 Gbar where our current understanding from indirectly driven NIF experiments suggest that stable well-performing implosions are feasible. On the other hand, the requirements on the laser, e.g., drive uniformity, laser colors, power balance, and on target and target fielding, e.g., fast shroud on cryostat, target alignment, and vibration control are much more stringent than for indirect drive implosions. In addition, the two-plasmon decay (TPD) instability will need to be mitigated.

Comments: TPD remains a key physics risk factor for direct drive implosions. This deleterious laser-plasma instability drives large-amplitude electron-plasma waves that cause hot electron preheat effects of the fuel affecting compressibility and laser-target coupling. It is important to adequately address the threshold and scaling for TBD with laser intensity, plasma scale length, and for zoomed laser beams. The current strategy is to mitigate TPD by utilizing mid-Z layers in the ablator. The mid-Z layer is effective in raising the coronal plasma temperature that in turn will lead to increased Landau damping of plasma waves and consequently reduced hot electron preheat. The predicted increase in temperature has been observed with Thomson scattering. Despite the obvious attraction of the mid-Z layer strategy, it appears to kick the can down the road. For once, it will require use of fill tubes where very little modeling expertize exists for direct drive implosions. Further, there is a lack of existing performance data to support the viability of direct drive implosions that use capsules with fill tubes. A complete assessment must further analyze the effects on shock timing and possible generation of reverberating shock waves in the ablator and exacerbated hydrodynamic instabilities.

Assuming that TPD can be mitigated either through mid-Z layers or with laser beam smoothing at moderate laser intensities it must be of high importance to prepare directly driven layered DT implosions on NIF. The NIF Polar Direct Drive (PDD) working group has adopted a program to work on shock timing, PDD phase plates, laser imprint studies (Multi-FM), and wavelength detuning experiments. The solution of these issues will require facility development efforts. These modifications are under way at a moderate level. However, the scope of work falls short of what is actually required for a successful direct drive program on NIF.

A NIF shot request of 12 shots on 5 days in FY16 does not match with the ambitions of the program and is not consistent with the goal to develop a directly driven burning fusion platform.

Direct drive implosion physics and the experimental campaigns on Omega are well prepared with radiation-hydrodynamic modeling and laser-plasma interaction calculations. At this point, LLE uses a no-name code for the calculations of 3-D effects; the quality of the results and the predictive capabilities of this tool have not been demonstrated. No benchmark calculations, comparisons with other hydrodynamic simulations or with experimental data are presently available.

Recommendations: The direct drive program will need to move much more aggressively towards delivering quantitative experiments on NIF. The direct drive program will need to develop an integrated experimental and facility plan that will result in the technical feasibility demonstration of cryogenic precision implosions on NIF with the correct color separation, beam smoothing, zooming optics, fast cryogenic shroud, and layering with fill tubes. The program must develop milestones and show that the performance matrix can be met in integrated experiments. This includes control of target debris, CBET mitigation, and laser power balance. In addition, engineering solutions will need to be developed on the NIF to meet the laser and target pointing and vibration requirements.

The direct drive program should further move towards developing and demonstrating effective TPD mitigation for NIF plasma scale lengths, plasma temperatures, and laser pulse durations. Mitigation strategies that include modifications of the baseline direct drive ignition target such as mid-Z payers or fill tubes will need to be addressed with simulations and experiments. The plan will further need to assess all DD facility modifications and test the performance with laser and target experiments wherever possible.

Further, the team must pay special attention to areas where the direct drive requirements are most stressing to the NIF laser and where they go significantly beyond present capabilities. New mitigations must be developed if power balance of 2.5% on target or beam and target pointing offsets of < 5 μm, or the fully requested wavelength tuning range cannot be delivered.

The experimental effort on the NIF will need to match the quality of experiment preparation, experiment performance and post-shot analysis at Omega. For NIF implosion studies, detailed simulations must be performed for actual experimental fielding scenarios. Sensitivity studies that include pointing offsets, power balance etc. will need to be performed to guide the experimental team with realistic pre-shot predictions. These shots must be an integral part of the NIF direct drive plan discussed above with the expectation to deliver answers to specific milestones. The results of experiments should then form the bases for developing optimized sets of requirements for attempts at alpha heating and ignition.

For this purpose, the program will need to employ and develop simulation tools that have been tested extensively against data. For example, for applications that will use the code HYDRA it will be important to further develop the code and to implement CBET ray tracing to make quantitative predictions. These tools should be tested against experiments on NIF.

Extensive LPI modeling of CBET will be required and NIF experiments that directly measure the coronal plasma conditions at the location of the CBET process should be performed. The team should further develop experiments to make quantitative measurements of CBET on NIF to test the models that are presently applied with fair success at Omega.

Extensive LPI modeling of TPD will be required and NIF experiments that directly measure the effects of hot electrons such as hard x-ray imaging or x-ray fluorescence measurements should be developed.

Extensive hydrodynamic modeling of the effects of fill tubes will be required and NIF experiments that directly measure the effects on neutron yield and areal density should be developed.

Hydrodynamic modeling of mix will need to be put on solid footing. This includes benchmarking against other codes and data. Experiments at the Nike laser could help testing the models. The development of mix mitigation strategies in directly driven implosions such as Au layers will need to be carefully conducted to assure that all other requirements on adiabat, velocity, TPD and CBET instability are met.

Ignition Approach: Magnetically-driven Direct Drive

Findings: The magnetically-driven direct drive ICF program is a recent development of the National program that combines magnetization, laser heating, and implosions to reach fusion plasma conditions. Specifically, the Z-pinch discharge magnetizes the fusion fuel and a beryllium liner with a magnetic field of 10-30 T in axial direction. The role of the field is to provide early confinement by inhibiting thermal conduction losses and magnetization of the ions, and to help stabilizing the main compression of the liner. Between magnetization and compression an optical laser heats the fusion fuel with a kilo-joule beam (2-10 kJ). Current experiments with D_2 fuel indicate temperatures of 200 eV due to the laser heating. The heating effect largely reduces the requirements on compression and simulations show that fusion temperatures can be reached with a radial compression ratio of about 30. This translates into implosion velocities of 100 km/s for at 100 Mbar magnetic drive pressure to achieve stable and quasi-adiabatic compressions. The current system can couple 100 kJ energy into the heated fuel.

With high priority, the program is pursuing the physics of driver-target coupling including target pre conditioning, implosion experiments, stagnation and burn, modeling and simulations. This includes focused and integrated implosion experiments on multiple facilities and the development of new diagnostics and simulation tools. At a lower priority the program is developing the scaling to high pressures on Z and possible high yield gas and ice burning physics on future facilities.

The first experiments on the Z facility have resulted in remarkably good performance reaching D-D fusion yields of 2-4 x 10^{12} at temperatures of ~2.5 keV. On Z, it is thought that conditions suitable for 100 kJ DT fusion yield with a pressure-time product of P-tau > 5 Gbar-ns and a field-radius product of BR > 0.5 MG-cm can be achieved (for the foreseeable future, DT fusion yield estimates are based on experimental demonstrations of DD equivalent yield). An important milestone is to deliver a 25 MA current to a successful target; if successful, a significant increase in yield is expected over the presently fielded 18 MJ current shots.

For 50:50 DT implosions, possible fusion yields from gas (~5 MJ) and ice burning (~ 1GJ) ignition are exciting prospects, but not attainable with present facility capabilities. On Z, a five-year schedule was presented to demonstrate performance with DT fuel at the 1% percent level of tritium. A 50:50 DT fuel shot would imply utilizing a significant amount of radioactive fuel and is stated as a longer-term goal.

The MagLIF experimental team has fielded a series of shots with the goals to improve the performance and fusion yield of magnetized implosions. However, it became apparent that variations of laser beam propagation and small laser heating of the fuel is a limiting factor for improving integrated implosion performance. The team has developed a strategy to demonstrate laser beam propagation at Omega and NIF with the goal to subsequently field optimized experiments on Z.

Other experiments have investigated implosion performance after some modifications were made to the Z-pinch discharge. Examples include coated wires and different gap distances; these studies have shown that the performance of the implosion is not fully predictable. This could be a reflection of variations in laser heating performance. However, modifications could also give raise to failures; in one example problems due to impurities were reported.

Comments: The time scales in the 5-year planning for fielding experiments with tritium to study fusion gain and alpha heating are too long. To benefit from progress of our understanding of NIF and Omega implosions and to provide leadership in compression and burn physics it will be important to increase the fusion gains from MagLIF implosions as quickly as possible.

Z pinch implosions provide a harsh environment making development of diagnostics a very challenging task. The program has succeeded in delivering excellent data of compression and burn. The program is an important contributor to the National Diagnostics program. Future improvements of temperature measurements with x-ray scattering and down-scatter from beryllium or deuterium are a priority.

The program would greatly benefit from the use of 3-D modeling to develop mitigations of instability features in the implosion. These developments should go along with fielding improved diagnostics of axially resolved imaging, spectroscopy and x-ray scattering to measure the conditions and compare with simulations. Importantly, simulation tools and models with magnetic fields will need to be developed and tested with focused experiments. Collaboration on HYDRA is an obvious choice. Magnetic fields also need to be included in reduced models, for example in the isobaric pressure model.

Recommendations: It should be the highest priority to field experiments with effective and reproducible laser heating as quickly as possible. Laser beam propagation is not a new problem and has been solved by NIF Early Light experiments in 2007. The laser beam smoothing requirements are well understood. Well-tested analytical models and 3-D simulations are available. The team should take advantage of the capabilities that exist within the National complex and use the tools to reduce the scope of the laser-plasma interaction investigations by designing phase plates and polarization smoothing optics. An integrated test should be demonstrated within less than 1 year.

Further, availability of a designated expert on laser-plasma interactions may be helpful to the program. This expertize could be in form of a designated laboratory expert, collaborator or a scientists at a University.

Also at high priority, the magnetically driven direct drive program must develop predictive modeling of the integrated implosion.

Experiments on Z occur at a lower repetition rate than laser experiments and the program has access to a limited number of shots. It must be a priority to field 2 laser beam lines (one for backlighting, one for laser heating) to deliver extremely important diagnostic information on every shot.

II. Assess the integration of experiments and codes

Diagnostics

Findings: The National Diagnostics program has developed a well thought-out plan to develop new diagnostics for measuring critically important quantities whose knowledge will allow major progress towards ignition. The program has brought together the scientists from all areas of the NNSA science complex and contributions of all programs and researchers have been vetted in several National Diagnostics workshops. This process has led to the first eight foundational diagnostics that will provide unprecedented information on implosion physics and compression and burn regimes, map out the plasma conditions created by both laser and pulsed power drivers, and enable dynamic information over a range of relevant conditions on the properties of materials utilized in nuclear weapons. The data provided by these diagnostics will validate and improve the physics contained within the multi-dimensional simulation codes developed by the ASC Program and both uncover and quantify important phenomena that lie beyond our present understanding.

Comments: The first eight foundational diagnostics span a wide range of capabilities addressing a range of critically important physics questions. The new capabilities will allow assessing the performance of integrated implosion experiments and support focused physics experiments that aim to measure single parameters or quantities that determine the physical and structural properties of fusion plasmas conditions. It will be important that the diagnostics are precise enough to critically test theory of dense plasmas achieving an accuracy of the data that can clearly distinguish between the results of simulations and different theoretical models. In addition, the diagnostics must visualizes the evolution of the compressed material and determine nuclear fuel assembly and hot spot formation.

It will be important to continue pushing the boundaries towards improved precision, higher resolution and diagnostics information that is based on fundamental physical principles. For example, new techniques will need to be considered that observe the Doppler broadening from x-ray emission lines to produce velocity maps and accurately measure residual kinetic energy. Further, particle and x-ray scattering methods should be adopted that allow measurements of the physical properties of dense matter by, e.g., observing Compton and plasmon features. Applications of these techniques in focused experiments can provide insight into density functional quantum molecular modeling, continuum lowering and the ionization state of dense plasmas. These methods are suitable to test calculations of physical properties (conductivity, pressure, ionization balance) to be used in radiation-hydrodynamic modeling of implosions.

The availability of precision diagnostics will be important to deliver the program with the best possible understanding of the behavior of integrated experiments and of the underlying physical processes. The availability is critical to remove multipliers and solve missing energy problems that still await a resolution. In the past, imaging and scattering methods have removed many uncertainties in our fundamental understanding of laser-plasma interactions and fusion plasma conditions. This is expected to also apply in present fusion experiments. In particular, competing modeling and resolution of missing laser energy in hohlraums appear possible with accurate temperature measurements of hohlraum plasma conditions with Thomson scattering.

Recommendations: The implementation of the precision diagnostics proposed in the National Diagnostics Plan should be pursued at high priority. The program serves as a major attraction of talent in experimental physics and provides a conduit to pursue unprecedented explorations of plasmas and material conditions critical to the missions of DOE/NNSA.

Computational Models and Predictive Capability

Findings: The commitment towards developing radiation-hydrodynamic codes and in particular of HYDRA by the ICF program at LLNL should be of great benefit of the community. Examples for testing the models and database that goes into the code have been discussed in comments above. Importantly, the code development includes physics critical to direct drive such as CBET and magnetic fields.

Comments: It will be important to perform benchmark calculations of the conditions for all three ICF implosion geometries and make the results available for researchers of the national program. Similarly to the analysis and archiving of the experimental database from NIF experiments it appears that the program would benefit from a dedicated person who keeps track of simulation performance with changing drive and as improved physics models become available. It was noted that several experimental plots from NIF performance data were shown without simulations results.

Recommendations: The continued development of HYDRA is a critically important activity that should be pursued at high priority.

III. Assess cross-platform and cross-laboratory collaborations

Findings: The extremely challenging measurements and diagnostics of laser-plasma interactions, preheat, and compression and burn physics are an excellent area for intense collaborations between the ICF program elements. The development of the eight foundational diagnostics demonstrate that collaborations on diagnostics are a very important area for pushing the boundaries of experimental physics, assuring quality measurements among the laboratories, and making critically important measurements that inform future designs for ignition.

The development of radiation hydrodynamics modeling is an equally important area for cross-laboratory collaborations.

Comments: Platforms for determining parameters of subsequent ignition shots are too specific to each specific approach and are not necessarily informing the other campaign. However, focused physics experiments on physical properties such as conductivity, opacity, EOS etc. are important for all approaches and are possible areas of collaboration among the scientists of the complex.

Recommendations: Besides including the relevant physics into HYDRA it is important to make predictions for problems that affect multiple laboratories. Effects of the fill tube in direct drive and comparisons to indirect drive, or laser beam propagation in MagLIF target and in indirect ICF hohlraums are examples that need accurate assessment and simulation predictions.

The collaboration on focused physics experiments to determine physical and structural properties or models thereof are important for ICF and have wide impact into related fields of science such as laboratory astrophysics and high-pressure physics. It provides an excellent opportunity to test assumptions and methods against those used in other fields of science and to collaborate on developing accurate physics models.

Reviewer Report: James Hammer

I. Assess the scientific hypotheses and the prospect for achieving ignition with existing scientific capabilities and facilities; or, if indicated, what would be required to achieve ignition and supporting analysis. Provide an evaluation of program balance among ICF approaches.

General remarks: The prospect for achieving ignition with existing capabilities remains uncertain. The groundwork being laid in the current program, however, does make it possible that ignition and substantial yield will eventually be achieved on either an existing device or a future laboratory driver. Reaching that goal will, of course, depend on the structure and level of support for the program. Many of the needed structural changes have already taken place, but the trends toward improving the scientific underpinnings and inter-laboratory collaboration should continue to be strengthened. A commitment by the NNSA for steady support of the program, with neither draconian cuts nor crash "success-oriented" programs, will be essential for fostering the needed climate and ensuring long term success.

We simply do not know enough at the present to define a guaranteed ignition-capable driver, or even whether the required technology is laser or pulsed-power based. The reason for that uncertainty is because of the core nature of the ignition challenge. Creating ignition conditions at the energies available from laboratory drivers requires extreme concentration of that energy, which in turn requires a high degree of control and precision in implosions in order to reach high convergence. To ignite, some of the fusion fuel must be hot enough for thermonuclear burn, but also it must be dense enough and long lasting enough that the release of fusion energy will heat surrounding fuel to burning conditions. These requirements lead to a connection between how much energy is needed, and the extent to which the energy must be concentrated or converged. If energy is plentiful in the imploding fuel, a fairly modest degree of convergence can reach ignition, or conversely, an energy-starved system will require extreme control and high convergence. Ignition has not been achieved on the NIF predominately because, given the amount of energy available, the control of the imploding fuel has not been precise enough to reach the needed convergence. The causes of that lack of control are incompletely known, and the scientific program to unravel the causes will dominate the indirect drive program for the next few years. Success at identifying and curing these problems would make ignition on the NIF much more likely. The other major stumbling block, aside from concentration of the energy, is the tendency for cold fuel or other material to mix into the hot, burning fuel and quench the burn. Control of this phenomenon is another challenge for all ignition concepts, although mix does not appear to be dominant in recent NIF experiments.

The energy-convergence connection is at the heart of the concepts competing with indirect drive. Direct laser drive is more efficient at converting incident laser light into imploding fuel energy, so the convergence requirements are less. On the other hand, direct drive has additional sources of non-uniformity, such as imprint of laser non-uniformities on the implosion. These are not yet well enough understood to know if direct drive wins out over indirect drive. Until recent years, the lack of control of magnetically-driven implosions had eliminated pulsed power as a contender, even though pulsed power is by far the most energy rich technology. Pulsed power drivers with more than ten times the energy of NIF are readily imaginable. Recent advances in control of metal liner implosions have put pulsed power into contention as a path to ignition and high yield. In the case of all three drive methods, the question comes down to the quantitative issue of whether the precision in controlling implosions matches the available energy.

The optimal approach to ICF ignition is not, at one extreme, an open-ended scientific program, or at the other extreme, an exercise in systems engineering where the fundamental science is believed to be sufficiently understood to march step by step to ignition. The National Ignition Campaign failed by hewing too close to the latter approach with excessive confidence in the existing physics understanding. The optimal balance point is where the emphasis is on finding a "good enough" physical model to guide the integrated ignition experiments. This balancing entails focused experiments to help isolate the important physics, model improvements, and integrated experiments that start from well-behaved sub-ignition "base camp" designs then take small enough steps to avoid extrapolating beyond the limited predictive power of the models. A base camp, in analogy with mountain climbing, is a starting place for the assault on ignition, which in our case means two things: 1) a design where the existing models have demonstrated predictive power in the sense of small changes have predictable effects and 2) the models predict that ignition could be reached by a series of incremental changes away from the base camp. Though likely incorrect in detail, if models show no path to ignition for a design, then experience shows that is probably the case, i.e. the models are unlikely to be pessimistic. Some might argue that a base camp design simply hides physics which will prevent modified versions from reaching ignition. In a sense this is true, but the main reason for the incremental approach is to be able to sort out what effects dominate degradation as changes are made. When the design is "over the cliff" in several ways simultaneously as occurred in the National Ignition Campaign, then it is very difficult to attack the problem. Also, the point of the base camp strategy with small steps is not risk aversion so as to avoid experiments that "fail", i.e., give lower performance. The point is to approach failure modes in a way that can help teach us how they fail.

Because the target designs require the interaction of many complex physical processes, the fate of an ignition approach rests largely on the fidelity of the multi-physics models used to design experiments and help interpret data. The complexity and the need for precision and high convergence rule out a purely empirical approach. The models in the design codes are necessarily approximate, with many simplifications, idealizations and often numerical "knobs" that are adjusted to cover unknown or unresolved physics. There are also many choices in model parameters, types of model and mesh resolution made by the design physicist using the code, so the accuracy and effectiveness depend on the skill of the user as much as the code itself. The choices and models represent what has been judged by design physicists and code developers to be "good enough" to serve as design tools for experiments, and as a consequence must be closely tied to experimental validation. The issue of getting the right answer for the wrong reason is always very present when using codes calibrated in this way. In fact, one could say the codes always get the right answer (when they do) for the wrong reason, and it is simply a matter of degree of "wrongness." Still, these codes in the right hands can be powerful tools for designing experiments, and for many problems the degree of "wrongness" is insignificant. The codes work best when the extrapolation is small from what has been experimentally validated. They failed to predict capsule behavior in the National Ignition Campaign because the extrapolation from the earlier validated parameter regimes was too great. Prior to the National Ignition Campaign, there was significant disagreement within the design physics community about whether the required extrapolation was too large. The pessimists have been vindicated. One of the tasks now for indirect drive is to use experiments much closer to the ignition regime to find the weak points in the earlier model choices and idealizations, repair the deficiencies, then use the codes to steer integrated experiments closer to ignition in small steps consistent with their predictive power. The situation for the alternates to laser indirect drive is analogous. Experiments that probe our ability to model implosions and ignition-relevant physics can test and validate the models for both laser direct drive and magnetically-driven implosions. Those validated models should enable modest experimental extrapolations for those concepts on the path toward ignition.

A well-proven target design strategy that complements the model-improvement strategy is to alter the experiment until the existing model becomes valid. A recent example of this strategy is the decision of the indirect drive campaign to focus on designs with shorter laser pulses and near-vacuum or low-gas-fill hohlraums. It appears that CBET (cross beam energy transfer) and laser backscatter are significantly reduced for these designs, making the hohlraum easier to model in some respects. Whether this strategy by itself can lead to ignition remains to be seen. The prudent approach is to work in both directions: change the design to reduce undesirable complex phenomena while at the same time improve our understanding of such phenomena.

Following the National Ignition Campaign, the indirect drive program regrouped and pursued a base camp strategy starting with a more conservative implosion, the High Foot design, along with the "small steps" philosophy described above. This strategy proved effective on two fronts: 1) capsule performance improved over an order of magnitude, breaking into the fusion alpha particle heating regime; and 2) experiments revealed the importance of symmetry control as a dominating factor in limiting performance. The second revelation has highlighted the need for better hohlraum models (the model-improvement strategy) as well as the design change strategy noted above. These two strategic elements, applied to all concepts, are the best chance for success but may not lead to rapid improvements in performance. Following the incremental path requires taking the long view on the part of program leadership. Improving models is time and labor intensive, with new focused experiments needed, as well as the painstaking analysis of an extensive suite of data, a certain amount of trial and error, and potentially new theoretical frameworks to account for neglected physics. An alternative strategy – identify a potential "magic bullet" fix and launch an integrated ignition campaign around that fix – has been tried without success. I strongly recommend the long haul, three pronged strategy of 1) data driven model improvement, 2) changing as needed to more model-friendly designs and 3) taking small steps away from reasonably well understood base camps in ignition attempts. Progress along this path cannot be measured solely by numerical figures of merit such as yield or stagnation pressure, but should also be assessed with the time honored and more nuanced method of peer review.

The small-steps approach applies to designs on the ignition path. In parallel, the program should be exploring new design features or concepts that could be integrated into a new or modified ignition-relevant base camp. A reasonable amount of risk-taking is warranted in trying things that are far afield from the usual designs. A potential example is the use of foam liners or shine shields in hohlraums. I should disclose that I have been participating in a Laboratory Directed Research and Development project on alternative hohlraums employing foams. Since the ICF design space is large and many things could be and have been proposed, the highest priority new concepts should be those that could potentially improve or reduce the importance of the most deleterious behaviors, e.g., uncontrolled asymmetry in indirect drive implosions. Experiments exploring new design features should be similar to other focused experiments where the goal is to test understanding of the underlying physics and ability of models to capture relevant behavior. Combined with the other experimental activities, tests of new design features can gradually increase the design physicist's "bag of tricks," i.e. techniques that can be employed with confidence in integrated designs. On occasion, the increasing "bag of tricks" may enable a qualitative leap in performance or control. Such leaps cannot be forecast or planned for, but the program can choose to create a climate that invites innovation.

Finally, on the note of program balance and collaboration, it makes good sense that the ICF program diversify since it is not clear that laser indirect drive on the NIF will reach ignition. Since LLNL represents the largest part of the program, it is reasonable that some of the first steps in diversification would come from increased involvement of LLNL scientists in laser direct drive and magnetically-driven implosions. Here let me disclose that I am involved in an existing collaboration between LLNL and SNL on magnetic drive. I can attest that the LLNL investment in codes, diagnostics and target fabrication are already being heavily leveraged to the benefit of magnetically-driven implosions and that we hope to increase that interaction. Similar interactions on laser direct drive could also be launched. Feedback in the other direction can be expected as well. Experience with fuel magnetization at SNL may help motivate NIF magnetized fuel experiments. The extensive expertise at LLE in the control of laser uniformity and bandwidth has already influenced direct and indirect drive experiments at NIF and will continue to do so.

Over the long term, I believe the program should be aiming toward a high-yield capability, whether or not ignition is reached on NIF. I base that belief on my understanding of the long-term needs of the Stockpile Stewardship Program. In my opinion, a fusion source of 500 megajoules or greater will be essential for the health of the program in an extended era without nuclear tests. Such a source is unlikely to be achieved in the next decade, but keeping high yield as an ultimate goal can still guide our thinking in the interim, e.g., driver technology choices could be made consistent with the high yield goal. Ignition is one important step along that path, but not the final end in itself.

Ignition Approach: Laser-driven Indirect Drive

Findings:

1. The change in the program after the National Ignition Campaign has born fruit through the High Foot campaign in two important ways: by accessing the alpha heating regime and through the greater clarity about what is preventing further advances, i.e., low mode asymmetry.
2. Improvements to the diagnostic suite over the last few years have had large positive effects on the program. Striking examples include the 2DConA imaging experiments which revealed the tent feature and the great wealth of information coming from neutron diagnostics.
3. Progress in numerical figures of merit, such as yield and the Lawson criterion, are likely to be episodic, given the nature of the program. For instance, the High Foot campaign appears to have taken advantage of comparatively low-hanging fruit in capsule performance, and further improvements could require more fundamental improvements in hohlraum radiation symmetry.
4. The new structure of the indirect drive program is a step in the right direction with 3 focus areas: integrated experiments, ignition science that pursues focused experiments, and physics integration.

Comments:

Physics integration covers the effort to analyze data and extract physically meaningful information, as well as improve models. This is a crucial activity that was not previously given enough attention. Before the restructuring, too many scientists were in "feed the beast" mode where a great many calculations were done to support upcoming shots, and the fidelity of those calculations was of secondary concern.

Each time a new diagnostic is fielded, unexpected structure emerges. These revelations have proven very useful and informative, and a steady pace of development of new diagnostics should be a priority for the program.

Even after the conclusion of the National Ignition Campaign, there was a tendency toward "ignition fever" in the program where some felt that ignition was just over the horizon and could be reached if only a particular feature of the implosion were tweaked. This led to substantial expenditure of shots and resources without corresponding benefit. The more humble but effective method of establishing base camps with incremental steps is preferable.

Recommendations:

1. Measure progress first and foremost through peer review assessment of scientific progress rather than numerical metrics such as yield.
2. Continue a three-pronged strategy of 1) data driven model improvement, 2) changing as needed to more model-friendly designs and 3) taking small steps away from reasonably well understood base camps in ignition attempts.
3. I encourage design physicists in both the integrated experiments area and physics integration to pursue more hohlraum model variations, especially those that can be tied to observations other than drive and hot spot shape. For instance, in hohlraums the gold bubble may intermix with the gas (or ablator for near-vacuum hohlraums) in a way not captured by standard hohlraum calculations due to Rayleigh Taylor instability or finite mean free path effects. Applying mix models to the gold–gas interface could change both laser propagation and the volume and location of x-ray emitting matter, hence the capsule symmetry. Viewfactor and other hohlraum imaging experiments may be able to constrain these models by comparing data to synthetic images. It is already known that the standard model does not accurately predict emission images in some cases.
4. Pursue the "BigFoot" design that increases hot spot rho-R at the expense of the cold fuel. One risk of this design is that mix at the fuel-ablator interface may have gone undetected in earlier experiments but the thinner ice layer could expose higher Z material to the hot spot. X-ray and potentially nuclear diagnostics capable of detecting hot spot- ablator mix should be fielded.
5. Examine implications of backscatter variation on stagnation symmetry. The observation that, for gas filled hohlraums, non-reproducible variations in inner beam backscatter are more than three times the incident beam variation, suggests this could be an important contributor to radiation drive non-uniformity and stagnation asymmetry, e.g. as observed on the FNADS nuclear diagnostic. The backscatter variation is especially of concern if the observed shot to shot variation is also representative of non-reproducible quad to quad variations. Improving backscatter variation measurements could be justified if models suggest the inferred variation is a problem, e.g. by instrumenting another inner beam quad with backscatter diagnostics.
6. Consider a large case-to-capsule, sub-ignition experiment that reduces drive asymmetry to levels that would not dominate performance. Such an experiment could answer the question: is it possible to get 1D performance from a symmetric implosion at greater than convergence 30? The answer could offer a preview to the next stumbling block after hohlraum symmetry is improved for ignition designs, or conversely, give more confidence that ignition will be likely once adequate symmetry is obtained.
7. Encourage innovation that could enable creation of new base camps. This would entail exploring more ways to increase the design physicist's "bag of tricks." Foam features in hohlraums are already being considered along with magnetizing capsules and hohlraums. A broader suite of modifications to the laser itself could be explored, e.g., new methods of modulating the laser to decrease CBET and backscatter.
8. Foster a robust basic science effort in important physics areas underpinning ignition. This would include, among others, non-LTE (Local Thermodynamic Equilibrium) atomic physics, computational fluid dynamics, kinetic theory of plasmas and Laser Plasma Interactions (LPI). The community has despaired of making significant headway in LPI but it remains a critical concern and 1) taking the long view, new experimental approaches and computational capabilities may arise and 2) as in target design it may be possible to change the experiment. An example of the latter would be rapid modulation of the laser with high contrast (factor of ~ 100), to make theory more applicable.

Ignition Approach: Laser-driven Direct Drive

Findings:

1. Direct drive experiments on OMEGA have continued to make steady progress in performance metrics such as stagnation pressure, and should be robustly supported as the main experimental capability for integrated spherical direct drive (SDD) experiments over the next five years.
2. Since NIF polar direct drive (PDD) implosion experiments appear to have low chance of reaching ignition, and the cost and time required to prepare for PDD are substantial, NIF direct drive experiments should shift toward assessing the potential of SDD on NIF.

Comments:

Several factors drive the finding regarding PDD: the belief, as expressed by LLE management, that PDD is unlikely to be a path to ignition; the desire to diversify the program over the next five years to include laser indirect drive, laser direct drive and magnetic direct drive; and the guidance from NNSA to expect roughly flat budgets over the next several years. Committing to fully equip NIF, within three years, with hemispheric detuning, SSD and PDD phase plates on all quads would require many tens of millions of dollars. The effect in a flat budget environment would be to narrow rather than diversify the program since that substantial sum would need to be extracted from the rest of the effort. Much of the effort expended on PDD would not apply to SDD, e.g. phase plates, and the resources expended on PDD implosions would distract from getting the best possible understanding of the LPI and imprint issues which will dominate the ultimate decision on SDD. Focusing on SDD physics would require fewer costly changes to the laser, i.e. smoothing on less than the full complement of NIF beams, and give SDD its best chance of building a strong physics case. The proposed early steps on the NIF direct drive program are already (and appropriately) aimed in the direction of understanding LPI and imprint issues. Further efforts to improve LPI could also be of major benefit to laser indirect drive and MagLIF. Changing the NIF direct drive program to focus on SDD would not be an irrevocable decision. If at any point the prospects for PDD ignition are found to improve, the program plan could be modified and the SDD-focused research that had taken place would undoubtedly be of benefit to future PDD experiments.

SDD on NIF would offer the best chance for ignition with direct drive, but the decision to reconfigure NIF for SDD will undoubtedly require a high bar given the cost and impact on the HED program. It is difficult to foresee all the elements that will drive that decision, but progress on the OMEGA SDD experiments and a good understanding of SDD relevant LPI and imprint are clearly requirements. Focusing on those two things for the next five years would seem to be the most prudent use of resources.

Recommendations:

1. Measure progress first and foremost through peer review assessment of scientific progress rather than numerical metrics such as yield. Major decision points, such as reconfiguring NIF for direct drive should require advances in performance metrics, such as stagnation pressure and the Lawson parameter as well.
2. Discontinue experiments preparing for NIF PDD implosions (shock timing, hemispheric detuning, 48 quads of SSD and polarization smoothing) and increase the number of NIF shots and diagnostics leading to high fidelity tests of laser plasma interaction (LPI) physics (CBET and TPD) at the correct scale lengths and plasma conditions for ignition SDD. The bulk of these could be planar and hemispherical experiments and include tests of high Z overcoats or buried mid Z layers as described in the program plan. Tests of imprint for ignition SDD conditions should be included. Smoothing on enough quads to enable high fidelity tests would be needed, but the deployment could be paced by experimental progress.
3. Develop improved LPI diagnostics at NIF, e.g. Thomson scattering at short time scales for a more detailed characterization of LPI processes. Laser direct drive is so intimately tied to LPI that decisions on SDD or PDD ignition experiments will need the strongest possible foundation. A focused LPI effort as part of SDD research with better diagnostics to both characterize LPI and study mitigation could also be of major benefit to the laser indirect drive and MagLIF efforts.
4. Encourage innovation that could enable creation of new ignition base camps.
5. Increase multi-laboratory collaboration on laser direct drive in the areas of target design, diagnostics, data analysis, and target fabrication as well as underlying science in these areas.

Ignition Approach: Magnetically-driven Direct Drive

Findings:

1. Magnetically driven implosions (MDIs) have made great strides in recent years in their ability to concentrate energy effectively and in producing hot, dense, thermonuclear plasmas.
2. MDIs are at an earlier stage of maturity than laser indirect drive, but stand to benefit greatly from the ICF program investment in codes, diagnostics and target fabrication.
3. The potential for pulsed power technology to deliver large total energy opens the possibility of ignition at lower implosion quality than required for lasers, as well as high total yields.

Comments:

As acknowledged by the proponents, the greatest risk of the MagLIF concept is the mix of material, either liner, window or even dense DT fuel into the hot fuel which could quench the burn. This can occur early in time due to filamentation of the laser, laser ablation of adjacent material or swirling of the gas caused by non-uniform heating. Late in time, the stagnation is also more prone to mix than standard ICF. The reason for that greater sensitivity is that igniting designs have lower hot spot rho-R than standard ICF, which translates into a longer burn duration to generate enough fusion heating to ignite. The ratio of required burn time to the sound transit time of the hot spot is inversely proportional to the hot spot rho-R. Accordingly, the low MagLIF hot spot rho-R means there might be significantly more time for hydrodynamic mixing than in standard ICF.

The space of potential MDI designs is large, however, with different strengths, sensitivities and failure modes than MagLIF. That breadth of possibility combined with the potential of the technology for reaching much greater implosion energy than lasers makes MDIs an area of new opportunity that should be exploited by the national program.

Recommendations:

1. Measure progress first and foremost through peer review assessment of scientific progress rather than numerical metrics such as yield. Major decision points, such as construction of a higher current driver should require advances in performance metrics such as stagnation pressure and the Lawson parameter as well.
2. More completely flesh out a diagnostic plan for characterizing plasma properties during MagLIF preheating and implosion, with a particular focus on understanding mix.
3. Increase multi-laboratory collaboration on MDIs in the areas of target design, diagnostics, data analysis, and target fabrication as well as underlying science in these areas.
4. Encourage innovation that could enable creation of several ignition base camps.
5. Consider an (initially modest) effort to explore magnetically driven indirect drive. More is now known about z-pinch-driven hohlraums than when SNL actively pursued indirect drive a decade ago, and such an effort would benefit from what has been learned in the laser indirect drive program on NIF. Estimates suggest that scaling up the mass, energy and size of a High Foot capsule, while preserving the demonstrated implosion quality (convergence ratio, shock history, stagnation pressure, etc.) would lead to ignition at about 3 times the energy of NIF. Construction of a pulsed power device at 3 (or even as much as 10) times the NIF x-ray energy appears to be manageable. The high efficiency of pulsed power and absence of expensive and fragile optics tends to give it a large advantage over lasers in cost per unit energy. Some of the main challenges of a magnetically-driven indirect drive approach would include demonstrating pulse shape control, reproducibility and symmetry.

Reviewer Report: Nelson Hoffman

I. Assess the scientific hypotheses and the prospect for achieving ignition with existing scientific capabilities and facilities; or, if indicated, what would be required to achieve ignition and supporting analysis. Provide an evaluation of program balance among ICF approaches.

Ignition Approach: Laser-driven Indirect Drive

Findings: *It has proven much more difficult than expected to produce adequately symmetric implosions of NIF indirect-drive ignition capsules.* This fact is viewed by the LLNL leadership as the main impediment, so far, to achieving ignition at NIF. While the symmetry of imploded capsules is *reproducible*, and varies *systematically* under small changes in initial and boundary conditions, it has not been *predictable* by numerical simulations. This finding applies generally to ignition-scale low-foot and high-foot capsules in high-density gas-filled hohlraums (0.96 mg/cm^3 < ρ_{gas} < 1.6 mg/cm^3). The unpredictability is believed to result from the strong effect of cross-beam energy transfer (CBET) in such hohlraums, and the high sensitivity of CBET to the details of local conditions, e.g., the temperature, density, and composition of the rapidly evolving plasma in the hohlraum. CBET moves significant energy from the outer beams to the inner beams in the hohlraum, but there is not a reliable predictive model for the phenomenon. The result is that laser energy is deposited in unpredictable (albeit reproducible) locations in the hohlraum, giving rise to an unpredictable (albeit reproducible) pattern of radiation drive falling non-uniformly on the capsule surface.

It furthermore seems clear that the asymmetry in a capsule implosion, induced by the non-uniform radiation drive, is exacerbated when the capsule is required to implode to a very small radius, i.e., a very high convergence ratio (CR). This fact is intuitively reasonable, born out by simulations, and verified in experiments.

Alternative ignition designs will receive greater emphasis in the near future. Given the difficulty in achieving adequate implosion symmetry in NIF hohlraums, the indirect-drive ICF design community at LLNL and LANL is reducing its emphasis on high-convergence capsules, with their tendency to exacerbate drive asymmetry. There is instead a new emphasis on capsule designs that sacrifice convergence and high gain in favor of robustness at lower convergence, with lower gain. The first LLNL high-velocity "Bigfoot" capsules (CR ≈ 25) will be tested in August 2015, while the LANL wetted-foam high-vapor-pressure capsules (CR ≥ 15) will be tested in the first half of FY2016, and six shots have been requested for LANL double-shell capsules (pusher CR ~ 10) in FY2016. These capsules all employ shorter laser pulses than past ignition-scale low-foot or high-foot implosions, and it is believed that the resulting lower density plasma environment will result in more predictable radiation drive symmetry.

Additional physics phenomena may play important roles. Other difficult-to-predict phenomena have emerged as possible significant obstacles to ignition at NIF. These include (1) laser-plasma instabilities and associated scattered laser light, likely accompanied by hot electron production, transport, and x-ray generation; (2) hydrodynamic perturbations induced by the capsule support structure ("tent"), which can be modeled only approximately in moderate-resolution simulations; and (3) non-LTE atomic physics processes involved in x-ray production in the gold plasma and the mixed gold-helium-ablator plasma. Interestingly, one often-invoked phenomenon that is believed to have been ruled out as a limitation on the performance of the high-foot capsules is the hydrodynamic turbulent mixing of ablator material into the hotspot.

In retrospect, perhaps NIF should have been designed with 45% of its energy in the inner beams. The difficulties involved in inner-beam propagation in high-fill long-pulse hohlraums, uncovered through extensive work on hohlraum performance, modeling, and simulations over the past several years at NIF, have led LLNL researchers to conclude that a better design would have put 45% of NIF's energy in the inner beams. As actually designed, NIF has 33% of its energy in the inner beams. The coming generation of improved hohlraum designs, using lower gas fills, is expected to make optimum use of the 33% as-built fraction.

Comments: *Hohlraums with intermediate gas fill (ρ_{gas} ≈ 0.6 mg/cm^3) may not be as easy to understand as is hoped.* We should be skeptical that complex systems (e.g., hohlraums) can be easily understood in small regions of parameter space embedded in large parameter regions that are baffling. Reviewers were told that high-density-gas-filled hohlraums (ρ_{gas} ≥ 0.96 mg/cm^3) were hard to understand because of CBET and LPI scattered light, leading to more oblate implosions than simulations predict. Reviewers were also told that low-density or near-vacuum hohlraums (ρ_{gas} ≤ 0.03 mg/cm^3) were hard to understand because of the likelihood of long-mean-free-path (kinetic) interpenetration of ablated capsule and hohlraum wall material, leading to more prolate implosions than simulations predict. Then reviewers were invited to believe that hohlraums with intermediate gas density

($\rho_{gas} \approx 0.6$ mg/cm^3) represent a “sweet spot” between the extremes, where the effects of CBET and kinetic plasmas are both small and “symmetry is reasonably well modeled”, in the sense that the 0th-order shape, and its sensitivity to small variations around a baseline, are predictable. But this appears to be a rather optimistic interpretation of the data. It seems at least equally plausible that, at intermediate gas density, the oblate-tending effect of LPI and CBET simply offsets the prolate-tending effect of kinetic interpenetration. If this is true, then the apparent ability to predict symmetry in this regime is fortuitous, and larger parameter variations in this vicinity may give unexpected results.

It is doubtful that any advantages of beryllium ablators will manifest themselves, as long as beryllium ablators are tested with hohlraums and laser pulses optimized for other ablators. To derive maximum benefit from the properties of beryllium that make it potentially appealing (such as its high ablation pressure at a given drive temperature), it will first be necessary to develop ignition-relevant beryllium target designs in hohlraums (e.g., with large size and low temperature) that are tailored to exhibit the desirable features of beryllium. For beryllium to warrant continued investigation, it will be necessary that these designs be superior in significant respects to optimized target designs using any other ablator. The superiority must be apparent first “on paper” (i.e., during the computational design stage), and then ultimately borne out in experimental tests.

Recommendations: *Hohlraums with low to intermediate gas fill ($\rho_{gas} < 0.6$ mg/cm^3) should be thoroughly characterized, guarding against unjustified optimism. The improved in-line CBET model should continue to be developed, and a corresponding effort in developing models for plasma motion in hohlraums is needed.* The low- and intermediate-fill regimes should be investigated thoroughly with a skeptical view of their predictability, until compelling evidence is accumulated. It is commendable that LLNL researchers are continuing to improve their ability to model CBET by developing an in-line CBET model, to replace the unwieldy present model that depends on post-processing an initial hohlraum simulation to derive CBET estimates for input to a second hohlraum simulation. But it appears that a commensurate effort on models for kinetic plasma motion and interpenetration is also warranted. Examples of such models already exist in at least one LLNL radiation-hydrodynamics code. In the “Hohlraum Era”, research on this kind of enhanced physics modeling capability should be paramount.

Beryllium ablators should be tested with a hohlraum and laser pulse optimized to exhibit a clear advantage over other ablators, if such designs can be found. Designing new beryllium-specific hohlraums will require intensive computational design effort, followed by experiments. Possible directions could include large low-temperature hohlraums optimized for capsule absorbed energy or drive symmetry, or higher temperature hohlraums with the capsule optimized for hydrodynamic stability. Then, if such designs can be found, the experimental process of hohlraum qualification and validation of the optimized target design should follow. The validation process alone is not cheap; LLNL hohlraum designers estimate that characterizing a new hohlraum and laser pulse requires about 1 year, 12 NIF shots, and a team of about four computational designers, not counting experimenters.

Ideas for reducing the effect of the capsule support structure should be pursued, with the goal of identifying an improved alternative to the current “tent”. Reviewers were presented with many promising concepts for less intrusive support structures. These concepts should be investigated, with the expectation that an unambiguous signature of improved performance, namely higher yield, will be clearly apparent. Since high yield can be spoiled by many effects, it is necessary to conduct these experiments under stringently optimized and reproducible conditions (e.g., with good ice surfaces and well controlled laser pulses).

In the Hohlraum Era, new kinds of hohlraum diagnostics should be investigated. LLNL researchers are to be commended for initiating novel experiments, such as the ViewFactor series, that have provided a clearer picture of the behavior of hohlraums and the conditions within them, including the behavior of the laser spots and laser entrance holes. But more efforts in this area may be required, in order to adequately constrain hohlraum models. For example, thin-wall hohlraums would allow time-dependent imaging of the most brightly emitting portions of the wall, important in accounting for radiation drive asymmetry. Marshak-wave arrival measurements at the outside of the wall would show directly the rate of energy absorption by the wall, important in accounting for the energy budget in the hohlraum. These and other techniques should be considered as possible future initiatives, as the predictive capability for hohlraums continues to mature.

Ignition Approach: Magnetically-driven Direct Drive

Findings: *The MagLIF target concept being investigated at Sandia is an intriguing but complicated approach to fusion ignition. Extensive research will be necessary to adequately assess, let alone realize, its prospects for success*. The MagLIF concept is rather new, with less than ten years of research focused on it to date. In certain attributes (low implosion velocity, thick imploding shells, low required peak fuel pressure) it appears to be a more conservative approach to laboratory fusion than laser-driven approaches. However, several poorly understood phenomena play crucial roles in the operation of a MagLIF target, including high-intensity laser heating of a deuterium plasma, implosion of a magnetized liner/plasma assembly undergoing magnetic flux loss, and magnetohydrodynamic instabilities such as magnetic Rayleigh-Taylor and electrothermal instability. The present limited capability for experimental diagnostics and predictive simulations cannot yet give the depth of understanding of target performance that would allow confident extrapolations to larger scale facilities. But Sandia researchers, with collaborators from LLNL and academic institutions, have a well thought-out, detailed plan to address the long list of physics issues and developments needed to move the MagLIF concept closer to fruition. They have recently leveraged their on-going NNSA-funded research with support from ARPA-E.

Comments: *At current funding levels, progress on MagLIF will be somewhat slow, but increased funding is probably not warranted unless certain key advances can be demonstrated*. Given the long history of research in laser-driven ICF approaches, and the comparatively nascent stage of MagLIF, it makes sense to continue the present program balance for the near and intermediate future. But Sandia MagLIF researchers have identified a set of ambitious goals that, if achieved at Z, might compel a revised national strategy, or at least a high-level review of fusion progress with that end in mind. These goals (to be achieved at Z unless otherwise indicated) include:

- coupling more than 2 kJ into the magnetized fuel with Z Beamlet, compared to the present level of ~0.2 kJ;
- demonstrating ~30 kJ preheating at NIF;
- achieving burn-averaged ion temperature greater than 4 keV;
- achieving yield greater than 100 kJ;
- demonstrating a continuous, nearly uniform stagnation column at CR > 20;
- determining the non-thermal component of fusion yield;
- controlling the seeds for acceleration and deceleration instability, and demonstrating the ability to simulate their evolution in 2D and 3D;
- demonstrating a validated capability to simulate magnetic flux loss (Nernst-Ettingshausen terms) and current flow in low-density plasmas;
- demonstrating predictable scaling of yield as drive conditions are varied;
- and numerous other goals as well.

Recommendations: *Addition of a capability to perform laser preheating and backlight imaging on the same shot would be a major step forward and should be pursued with high priority*. The state of the preheated fuel (including, for example, its density and temperature distributions, B_z, and induced velocity fluctuations) probably exerts a strong influence on the later evolution of the implosion. Observing it will require a simultaneous preheating/backlighting capability.

The capabilities (1) to add tritium or ^{3}He to the fusion fuel and (2) to measure the fusion gamma rays produced in DT or D^{3}He reactions would allow observation of the fusion reaction history in the implosion, and should be pursued with high priority. At present there is no capability at all to observe the reaction history in a MagLIF implosion. Such a measurement will provide a hugely important constraint on models and understanding of the implosion.

Roadmaps and Decision Processes

Findings: *Summary of some hypotheses motivating Roadmap and Decisions in indirect-drive ignition research.* Since our primary charge is to make “an assessment of the scientific hypotheses that guide today’s ICF program of work”, it is useful to try to list what some of those hypotheses are. I did not see an aggregation of specific hypotheses explicitly presented at any sites, but this section seems like a good place to begin such a list.

Hypothesis I: The past poor predictability of NIF ignition-scale capsule implosions stems largely from poorly predicted drive asymmetry arising from several sources including the tent perturbation, aggravated by high convergence.

Hypothesis II: New shorter-pulse low-convergence target designs, such as Bigfoot, high-vapor-pressure wetted foams, and double shells, together with lower hohlraum gas density, will behave more predictably than past longer-pulse high-convergence designs, and conform more closely to simulations.

Hypothesis III: Hohlraums with gas fill density in the intermediate range around 0.6 mg/cm^3 will be largely free of various difficult-to-understand phenomena limiting our predictive capability at present, such as LPI, CBET, and kinetic plasma interpenetration.

Hypothesis IV: Ablator materials such as high-density carbon and beryllium provide attractive alternative paths to an ignition design.

II. Assess the integration of experiments and codes

Diagnostics

Findings: *Researchers have made truly remarkable progress in developing and using advanced diagnostics for understanding laser-driven ICF implosions. Ambitious progress on additional diagnostics is on-going at NIF*. Considering just diagnostics for asymmetry in capsule implosions, for example, a wide range is already in place, including re-emission balls, keyhole VISAR, 2DConA radiography, self-emission x-ray images, primary and down-scattered neutron images, $\Delta\rho R$ from FNADS, and outgoing shock imaging. Besides these, other diagnostics under development include foam balls, 5-axis keyhole, gated SXI, late-time 2DConA, early time self-emission, higher resolution imaging at stagnation including KBO (Kirkpatrick-Baez Optic) and penumbral imaging, Compton radiography at stagnation, and co-aligned neutron and x-ray imaging.

Recommendations: *For NIF implosions, the all-important epoch between the time of peak velocity and the time of stagnation deserves highest priority in the development of new diagnostics*. New diagnostics focusing on this time window (late-time 2DConA, early time self-emission) should receive the greatest emphasis, as this is the crucial period during which inflowing kinetic energy is converted to thermal energy of the hotspot and fuel, while the whole assembly is brought to its maximum density. This is also the time period when low-mode asymmetry probably exercises its greatest deleterious effect. Yet this period has received little diagnostic emphasis so far (probably because capsule conditions, including brightness and optical thickness, are changing so rapidly), which may be why understanding it has proved elusive.

III. Assess cross-platform and cross-laboratory collaborations

Findings: *There are numerous good examples of highly effective cross-platform and cross-laboratory collaboration.* These include the participation of LANL scientists in developing new diagnostics (e.g., neutron imaging and gamma-ray measurements) and fielding experiments at NIF, and in designing new kinds of ignition capsules, such as those with beryllium ablators or low convergence ratios (e.g., high-vapor-pressure and double-shell designs). Academic researchers, including the groups at MIT and at LLE, have likewise developed vitally important diagnostics, such as high-resolution spectrometers for charged particles, neutrons, and x rays. Another example is the participation of LLNL scientists in the Sandia MagLIF effort, through developing MHD computational capability, investigating the mechanisms of laser preheating, and simulating and analyzing the performance of magnetized targets, while performing relevant experiments at other facilities. Regarding cross-platform collaboration, the OMEGA laser has been absolutely crucial in allowing new diagnostics to be tested and optimized, and then rapidly implemented at NIF. Huge savings in terms of NIF shots and program costs have undoubtedly been realized as a result.

Reviewer Report: Warren Mori

To: Keith LeChien ICF Director
Lois Buitano Group 1 HQ Lead
Njema Frazier Group 2 HQ Lead
Kirk Levedahl Group 3 HQ Lead

From: Warren Mori

Re: Group 1 Progress towards ignition for the 2015 ICF/HED Review

This is an individual report based on attending three separate deep dive presentations held respectively at Sandia National Laboratory (July 28 and 29), Lawrence National Laboratory (July 30 and 31) and the Laboratory for Laser Energetics (August 2 and 3). I was not able to attend a the May 18-20 meeting at NNSA headquarters because my clearance had not yet been processed; however, I met with some of you at NNSA headquarters at a later date to obtain details of the planning of this review as well as to read through the presentations from this original meeting. My report is also based on documents provided online to the members of Group 1 as well as on private conversations with some scientists from the NNSA laboratories.

The primary charge of our Group is an assessment of the scientific hypotheses that guide today's ICF program of work, and prospects of achieving ignition with existing scientific capabilities and facilities, or, if indicated, by specifying what would be required to do so, based on quantitative scientific analysis. This includes a request for an evaluation of program balance among ICF approaches and an assessment of the effectiveness of the ICF Program's cross platform and cross-laboratory collaboration.

I believe that achieving ignition through inertial confinement fusion is a worthy goal from a purely science based view. With three major experimental facilities currently operational (NIF, OMEGA, and Z) this is a very exciting time for HED science. Significant progress continues and this progress is due to the experiments generating data and the improvement in diagnostics. This leads to new hypotheses and the design of new experiments and the identification of where codes need to improve. The ICF program is generating large numbers of peer reviewed publications and, with the delays in ITER, it is in a strong position to recruit talented PhD graduates as well as to entice entering graduate students into HED science. In this report, I will offer recommendations in areas I think the program can be improved and not comment on the many areas in which the program is working very effectively.

I was very impressed with the breadth of activities and science being conducted at each facility. I was also happy to hear from and meet numerous young staff and post-doctoral researchers at each laboratory. Personnel at each laboratory were very responsive and helpful in answering questions. It was my impression that the research staff of each laboratory believe in what they are doing and are optimistic about the success of their respective ICF concepts. Although I spent 6 days visiting the sites, heard numerous presentations, talked to lab researchers, read through numerous documents, and read some peer reviewed publications, it is still not possible for me to assess with confidence the likelihood of ignition. It is clear that within the next 5 years, ignition can only be achieved with ID on NIF. Both DD and MD need new facilities to demonstrate ignition, SDD on NIF and Z+ at SNL. Currently, laser plasma instabilities (Stimulated Raman Scattering) and cross beam energy transfer (CBET) remain an obstacle to creating the necessary drive symmetry (time dependent) that would provide the needed fuel conditions for ignition for the high foot design. These are also major issues for the low foot design (higher convergence ratio), where hydro instabilities and mix are also an issue. Achieving ignition using either the high or low foot designs are exacerbated by the lack of predictive ability in modeling individual effects of CBET and SRS, or their combination. The tent, which holds the capsule in place, is also is believed to effect surface perturbations when it explodes. I feel that this issue can be fixed somehow.

The strategy being employed at LLNL is to explore "many" different ideas and iterations. They fall under three categories, each of which is believed could ignite the hot spot. Within each category the goal is to find an experimental platform for which there is agreement with "1D" calculations and to use this as a jumping off point to then gradually push towards higher yield and then ultimately ignition. The first category is based on pushing the high foot concept towards ignition by experimenting with different gas fills, ablators, hohlraum sizes and shapes, hohlraum walls, and drive profiles. The second category is to go to even lower convergence ratios, higher implosion velocities, and larger hot spots where the hot spot itself has enough mass to provide greater than ~100kJ (a starting point) of yield. The third category is to increase the laser energy (there were other ideas along this energy path). Some hydroequivalence arguments could be made regarding how present results might scale. I do not recall seeing these.

In addition, although not discussed the 2ω operation for both a near vacuum (NVH) or a low gas filled (LGF) hohlraum could be considered.

Even with all of the information, LLNL scientists cannot say with certainty which path is more likely to eventually lead to ignition of the hot spot and cold fuel and the odds of success. An attempt to assess the overall "risk" of a new path was presented in the form of a spreadsheet. Changes from existing platforms are listed in columns, an each change is assigned a low, medium, or high risk based on the confidence level of the understanding of its effectiveness. It was stated that to adequately study one "concept" takes at least 8 shots. Unfortunately, there are only ~30 high energy shots per year. For example, while going to near gas or low gas filled hohlraum (NVH and LGF) did eliminate some issues with respect to time dependent symmetry, symmetry issues remain. The levels of SRS for gas fills less than .6mg/cm-3 is not believed to be important. It may be too early to argue that LPI is a non-issue for NVH and LGF. As expected, even with the shorter laser pulse lengths, wall motion is an issue without the gas fill (wall bubbles form). Furthermore, a lack of agreement between codes and experiments also remains. It is believed that a major source of this disagreement is due to the interpenetration of the wall and the ablating material. Eliminating LPI/CBET from the hohlraum should make modeling "exponentially" easier, as the LPI is not in play. Using the NVH and LGF to "validate" some of the hydro codes should be a high priority. If the design codes cannot accurately model these designs it seems unlikely it is difficult for the ID effort to move forward. There are many ideas on how to mitigate the wall motion with LGF however, with the limited shot rate a solution will only be found with predictive code capability.

There were presentations on capsule only simulations where the x-ray drive included 2D and 3D asymmetries. These show how asymmetric implosions get worse at large convergence ratios presumably because of lost energy into RKE and increased heat condition from the larger surface area of the perturbed hot spot surface. These showed that 3D (including the azimuthal and polar angles) is worse than simple 2D (only the polar angle) asymmetries. There were also hohlraum simulations in which the time dependence on the laser energy in different cones is altered to see how the results compare to data. Both of these approaches should be continued as they will help to determine where in time and space the LPI/CBET needs to be more closely examined. Another issue that was discussed in several talks is the understanding of the stagnated fuel conditions. These talks illustrate the tremendous progress on diagnostics and how the increased knowledge narrows the window for the predictions for the codes. I did not hear in any presentation about how the time dependent asymmetry (or lack of symmetry in the early part of the pulse) leads to asymmetry to the entropy (and a higher average entropy) which will make compression more difficult. It seems that this is also an issue.

Currently, the predicted CBET for NIF scale coronas in direct drive targets makes ignition not feasible even with 1.8 MJ of laser energy using symmetric direct drive (SDD). The strategy for progressing towards ignition being employed by LLE is more straightforward since gas fill, hohlraum size, and drive profiles are not in play. The proposed path forward is to attempt to mitigate CBET on OMEGA (first using zooming and then a graxicon), and increase P_{hs} and E_{hs} to values which, when scaled to NIF energy (a factor of 30) using hydroequivalence, lead to ignition. This requires hot spot pressures of ~>120 GBar. Currently, pressures of ~60GBar are being seen without CBET being mitigated. This path forward assumes that any physics that does not follow hydroequivalence does not affect the drive pressure, the symmetry of the implosion, and the implosion itself. This includes CBET, LPI, heat transport in the conduction zone, thermal conduction in the hot spot, and the mean free path to hot spot size for the DT ions. My impression is that LLE believes that the biggest issue with hydroequivalence is CBET and LPI (hot electrons from 2ωp) so they have put forth a program to study these for NIF scale targets using polar direct drive (PDD) shots on NIF. However, these studies will still have shorter density scale lengths than that expected in an SDD ignition sized target. Other "non hydroequivalence" physics could be important so it is important to capture some of these in 3D simulations. As was learned from NIF, scaling some physics to higher energy is not simple.

The long term (decades) goal of the magnetically driven liner fusion effort is to produce yields of ~GJ per shot which it is projected would take at least a 130MJ pulsed power driver. The nearer term goal is to develop the scientific case to build Z+ which would be a 50MJ facility that could produce ignition (more output energy than is absorbed [3-5MJ]). The 24MJ Z facility couples about .5MJ on target. The MagLIF concept is much newer than laser driven ICF and there is a smaller experiment and computational database. Therefore, less is known about the potential issues. Therefore, the next 5 years are very important and the proposed plan does include a well thought out list of physics uncertainties, diagnostic needs, and modeling challenges. These are broken up into driver target coupling (moving towards 26MA of current), preconditioning (preheating the fuel), implosion (understand what leads to non-uniform compression and how this effects the stagnated properties), stagnation and burn, and modeling and simulation. An important part of this last topic is developing scaling and some hydro-equivalence. The topics are vast and they cannot all be studied with existing manpower. Experiments are critical to indicate where to focus the computational and follow on experimental efforts. Integrated experiments are beginning to be undertaken at Z with the goal to demonstrate Br greater than .5MG-cm, 4 keV ion temperatures, pressures exceeding 5 Gbar, and P□ greater than 5 Gbar-ns. A major obstacle has been the coupling of the preheating of the gas by the laser. In current experiments the Z Beamlet (~2kJ laser) is used. This needs to be improved to >6kJ to achieve the pressures and P□□ to detect enough neutrons for the diagnostics. More than 20 kJ needs to be coupled in the Z+ designs so possible experiments at NIF (30kJ) or on OMEGA-EP (6.5kJ) can also be carried out. Since there seems to be flexibility in pulse length, they should be able to make progress by lowering the average laser intensity and using "smoothing" techniques. This effort could benefit extensively from the techniques used to mitigate LPI in laser driven ICF as well as from the experimental infrastructure. It should be mentioned that some LPI codes may not be able to straightforwardly model plasmas with embedded magnetic fields.

The effort on diagnostics seems well organized and well designed. There is a National Diagnostic Plan (NDP) is a living document with ongoing collaborations. It has a management group and is based on input from user groups, HED science workshops, and diagnostic centered workshops. However, if developing a path towards ignition is viewed as a top priority then I argue strongly that additional backscatter diagnostics as well as Optical Thomson Scattering are needed urgently. These diagnostics are not going to be a priority of HED science users of NIF. I believe that for the most part the NDP and the process seem very well thought out. Progress, theoretical understanding, and simulation validation can only be achieved with adequate (sometimes duplicated) diagnostics. This coordination should be duplicated by the design code efforts.

Finding: Based on document 62_WP_ALL_ Group _1, the balance of the program in terms of FTEs is reasonable to zeroth order.

Comments: The categories of manpower are broken up into experimentalists, designers, code developers, and theorists (note that the numbers at LLNL for code development and theory are switched, it should be 6 code developers and 4.5 theorists). To accurately assess the correct balance it would be useful to know the meaning of the designer, code developer, and theorist. For example, how many designers and code developers also do theory and do theorists use codes? The power of computer simulations is to study the changes to an output due to changes to an input as well as changes to physics packages, while being able to have perfect diagnostic capability. Is this type of exploratory simulation (not modeling a particular experiment) done by designers, theorists, or both? In which category do those doing LPI belong? It is noteworthy that there are 6 code developers at LLNL while 7.3 at LLE. Furthermore, at LLNL these are broken up into those working on Hydra and Lasnex. What about PF3D or other LPI related codes? Who is carefully looking at the results from design calculations and ensuring that the models within the codes are "validated"? I will comment on this later. It also seems to me that having only .3 FTE on code development at SNL seems low, unless the needs of MD ICF on code development are being met through the 6 code developers at LLNL. There is much knowledge at LANL in ICF. Based on what was presented at the deep dives it seems that it would be useful to engage this expertise more. There is also much knowledge and expertise at NRL on DD. The role of this effort in the ICF program should be better defined.

Recommendation: The management at each lab should attempt to allow FTEs to have at least 2 days a month to think freely about other ICF concepts and new concepts, and new theories. This is in addition to any realignment in activities agreed to by lab management. For example, designers at LLNL should also be able to work on DD and MD during these 2 days a month. Experts on SRS at LLNL and LANL (more on the LANL effort on LPI later) should be able to study SRS on DD scale targets on NIF and experts at LLE on 2ω_p be able to study 2ω_p for ID plasma and laser conditions. I also do not feel that the designers and hydro code developers have the necessary understanding of LPI. The efforts across experiment, design, code development, and theory, should not be as compartmentalized as they appear. This could be improved through interlab sabbatical programs as well as having "designers" work on some LPI problems and LPI "theorists" work on some design problems. In some cases, "experimentalists" should be encouraged to run design codes as this can help them better understand how to interpret data and allow them to know when code results can be trusted.

Finding: LPI has been actively studied within the context of ICF for over 40 years. In fact this effort was a major driving force in the development of PIC codes. PIC codes are now widely used throughout plasma physics and are currently in limited use within the ICF effort. This recent precipitous reduction in the LPI effort (particularly at LLNL and LANL) is due largely to the inability of eliminating it, and the hope that LPI issues could be engineered away. Unfortunately, LPI including CBET is arguably the biggest obstacle to high yield designs. This philosophy has led to a significant drop off in the expertise on fully kinetic modeling of LPI (at and outside the ICF laboratories) as well as led to insufficient diagnostics of LPI on NIF. However, laser plasma interactions (including cross beam energy transfer) remain a major issue for each ICF concept (ID, DD, MD). Without a critical mass of effort, LPI is not being adequately understood or addressed (experimentally, computationally, and theoretically). It is worth noting that there is now minimal effort at the labs on PIC calculations. Until very recently LANL had a strong effort on PIC modeling of LPI but it no longer appears to be supported.

Comments: For example, in the low and big foot campaigns, there was significant (at least 20% of the energy was reflected after including the CBET) SRS from the inner beams. Perhaps more importantly, when comparing 15 shots with the same nominal target and laser conditions, there was 15-20% variations on the back scatter energy. In addition there are variations of in the amount of light absorbed (or rescattered) as it moves back to the laser entrance hole. So there is a ~10% variation in the amount of light hitting the wall which is 5 times greater than the variation of the laser from shot to shot. The natural assumption is that this variation exists on a single shot across the various inner beams. The angular variation in SRS and plasma conditions could have been known on single shots if more NBIs and FABS were added. CBET in direct drive and the hot electrons from two plasmon decay (and the high frequency hybrid instability [HFHI]) are currently believed to be biggest challenges for hydroequivalence. In MD, propagating and absorbing the heater laser without it hitting the walls and backscattering is believed to be one of the biggest challenges. Importantly, these processes are modeled with codes that are reduced models (such as PF3D and LSPE [ZAK]). There have been claims that they have been validated against experiment (I comment more on this later with respect to all codes). While I sympathize with the need for codes to model time and space scales needed to make statements on experimental measurements, these codes and their assumptions need to be “validated” against codes with more physics. While the assumptions might be reasonable at lower laser energy they could very well not be at higher laser energy, different plasma temperatures, different densities, different temperature and density scale lengths, and different mixes of material. For example, none of these reduced models can include the effects of self-generated or imposed magnetic fields. Fully kinetic models such as PIC codes have shown that the reflectivity from SRS is in short bursts and can in fact exceed unity for short times. This is in contrast to reduced model LPI codes. Such intense bursts of reflectivity can lead to rescatter and a spectrum of plasma waves that can lead to more energetic hot electrons. While the average reflectivity (the experimental measurement being used to validate a code) might be similar between PIC and a reduced model, the details can lead to very different conclusions regarding the inside of the hohlraum and the symmetry of the drive. The reduced models attempt to add some kinetics back by including a kinetic damping term (or a nonlinear frequency shift) in the time envelope operator. However, these are local operators, and the physics is not local. The reduced models do help to identify physics such as cooperation of plasma waves from overlapping beams. Validating the reduced models on smaller time and space scales against the more kinetic models would be useful. Despite these differences which point to a need to keep kinetic modeling capabilities in the mix, there has been a decline in PIC efforts on LPI at the labs. It should also be stressed that not all PIC codes are the same. I could list many more areas where the reduced models may miss physics. It is very likely that LPI will remain unpredictable and a problem, unless one can find a fully saturated nonlinear state that is not deleterious (need more laser energy) or determine mitigation strategies that keep it in the noise in a linear regime, e.g., STUD pulses. For example, even if $2\omega p$/HFHI is found to not be a problem on OMEGA scale plasmas, it may be a problem on NIF SDD scale plasmas (600um scale lengths). At this scale length (and temperatures) it will likely be far above threshold, and it will turn on earlier in the pulse. In addition SRS may occur. Even if SRS leads to a few percent reflectivity this could leave an imprint on the target. It is also worth noting that as today’s computers get bigger, the capability of PIC gets greater (the best PIC codes scale to 1,000,000 cores). The 3D simulations done with the ZAK/EMZAK (where the overlapping beams are shown to be important) can be carried out with a 3D PIC code using 10,000,000 core hours per 5ps. On a computer with 300,000 cores, this would only take 30 hours (about a day to complete). Such simulations will only get better on future machines with GPUs and Intel Phis.

Recommendation A: There should be a reconstitution and reinvigoration of the LPI efforts. These efforts should be coordinated across the labs. It should include having joint/coordinated code development efforts and making the lab codes open source (or openly shared within the labs) using Github or an equivalent. This would also allow outside groups at universities to participate. For example, EMZAK under the LPSE will be able to model SRS and SBS in underdense plasmas. This code does not envelope the spatial derivatives or the ion acoustic wave equation. Results from EMZAK could be compared against PF3D for some volume. Within the ZAK models of LPSE, electrons (and ions) are pushed through the full fields (the fast component at ω_p is added back to the fields) and test particles are pushed through them (I do not believe that collisional effects are included in this process).

This evolves a distribution function that is globally averaged and then used to estimate Landau damping of the plasma wake at each k component. In reality, the evolution of plasma waves depends on the local distribution function. Pockets of expertise in kinetic modeling exist within the university community. This includes PIC as well as Vlasov Fokker Planck. Engaging this expertise to study specific parameters of relevance to ICF should be encouraged. A small working group should work to develop test simulations for comparing codes against each other where possible and working to understand why they do or do not agree. This can be time (FTE time) consuming, so this must be factored into the prioritization of what is to be studied. There should be an effort at ensuring that these codes are easy to use so that non LPI experts such as designers could run them. The Anomalous Absorption Conference should be supported and by NNSA and the ICF labs. A small workshop on LPI theory and modeling should be held where common and unique issues for ID, DD, and MD are discussed and hopefully a consensus on some priorities is formed. Ideas which may eliminate LPI such as STUD pulses should be explored and discussed.

Recommendation B: Additional viewing angles for backscatter should be implemented as soon as possible on NIF. At the very least two more NBIs should be added, one at a new azimuthal angle and another in the opposite pole. Adding another FAB at one of these angles would also be useful. Adding Optical Thomson Scattering capability should also be given top priority. To understand LPI and its true effects it is necessary to know the plasma conditions as well as the angular variability of the backscatter (time and frequency resolved). I realize that there is a National Diagnostic Plan (which as I noted earlier is very well thought out), however, I cannot think of a reason why the backscatter diagnostics should not be a high priority. If the cost is formidable then a working group should be formed to find a solution.

Finding: Ignition will not be achieved without multi-physics design codes that have some degree of predictive capability to guide experiments. Furthermore, as new concepts are investigated, new experimental data is acquired, and diagnostics improve, then more physics will need to be included. There appears to be lack of true coordination in code development across the labs. This leads to a duplication of efforts as well as an incomplete knowledge of what physics is being included, what equations are being solved, and what physics packages have been implemented.

Comments: We heard about a multitude of codes and duplication of efforts. Some codes are 2D while others are 3D. Some scale better on parallel computers. In addition, while there is much in common there are important differences in the physics that is needed to be included between ID, DD, and MD ICF. Due to different priorities, code developers responsible for a code that has primarily been used for ID may be delayed in including physics needed for another concept. We also heard claims that certain packages are better in certain codes. These two issues lead to others using less desirable codes or developing their own code. The development of design codes needs to be better coordinated and more transparent.

Recommendation: The equations for each code should be widely disseminated within the ICF complex and to the extent possible should be published in the peer reviewed literature. To the extent that classification is not an obstacle, the details of the physics packages (non-LTE, flux limiters, inclusion of magnetic fields) should be described. A National Design Code Plan (with classified and unclassified parts if necessary) with a management team should be generated. The first step would be a workshop focusing on short term and long term priorities. It appears that the code of choice moving forward is Hydra. However, MD (as well as ID and DD) requires magnetic field generation and transport. Within Hydra, what is the form of Ohm's law? Does it contain Nersnt and Gran n x Grad T terms etc.? What are the details of the non-local heat transport? Does it include transport across B and along B? Researchers at LLE are not using Hydra because they need better ray tracing and non-local heat transport models. They are running their own 3D code (we do not know what equations are being solved) but as I understand it, it assumes a symmetric corona and adds an imprint to the extent possible the codes should be managed using Github or an equivalent. After the NDCP is generated, then a sabbatical program that allows researchers from other labs to have extended visits (three months to a year) at another lab to work side by side (this is the only way that joint work will truly occur) to improve the software. I heard arguments that it is good for each lab to have their own version of codes or to have two or more codes. I am sympathetic to this argument, as I too have argued that monolithic codes can be dangerous. However, as codes get very complex and more physics needs to be added, the real challenge is validating each package (and not the whole code) and then verifying that they have been integrated into it properly. Furthermore, computer power is improving at the expense of hardware therefore it is too FTE intensive to maintain more than one large code that will run effectively on next generation hardware. Software engineering and data structures are chosen such that new physics packages can be added without these developers worrying about parallelization and the hardware.

Finding: Much of the DD strategy for moving forward is based on the concept of hydroequivalence. The idea is to scale OMEGA results at 60kJ to NIF at 1.8 MJ. This can also be used for ID concepts (and with modifications MD concepts) if the laser energy is scaled to higher energy or to 2ω operation. As noted in the original papers on hydroequivalence there is much physics that will not scale. This includes among other things, CBET, LPI, heat transport in the conduction zone, thermal conduction in the hot spot, and the mean free path to hot spot size for the equilibration of the D and T ions.

Recommendation: A small workshop on hydroequivalence with researchers across DD, ID, and MD should be held to rank the areas of concern and decide what capabilities need to be in the design codes to better quantify and include their effects.

Finding: There appears to be a widespread view that a "code" is validated when it provides agreement with experiment. However, these codes involve complex (and nonlinear) couplings between choices of physics packages with fitting parameters as well as numerical choices.

Comments: Modeling ICF is a very challenging problem. However, some form of predictive capability on ICF ignition using codes may only be achieved if each physics package is validated against more "accurate" models (those that make less approximations or assumptions). This is true across the entire spectrum of codes used. For example, while there is a current attempt to include inline SRS backscatter and hot electron models using ray tracing, this cannot provide true predictive capability. This will provide additional knobs (such as flux limiters, CBET limiters,…) to change to tune the agreement and it will allow one to see how the reflectivity feeds back onto the plasma conditions which then modifies the backscatter. There is merit in this so that designers can develop an intuition of the complex interplay of physics. However, this cannot lead to true predictive capability, as SRS (or other LPI processes) cannot be accurately modeled in this fashion. Unless each reduced model is validated against meso- and/or micro-scale physics models for SRS/CBET, non-local heat transport, magnetic field generation, interpenetrating fluids, etc. then the integrated result cannot be correct in general. There has been tremendous advances in kinetic modeling (Vlasov Fokker Planck [VFP] and PIC). VFP capability now includes fully parallelized codes that expand the distribution function into an arbitrary number of spherical harmonics. There are implicit field solvers such that very large cell sizes can be used. These codes can be used to test physics packages or even be integrated into the hydro codes. PIC can model more and more spatial and temporal time scales (they can run on 1,000,000+ cores and on GPUs and Intel Phi processors) such that some hydro processes can be studied with time and space scales of relevance.

Recommendation: Efforts should be made to free designers, LPI researchers, HED plasma theorists time so they can try to validate each physics package, i.e., to improve them or to better understand their limitations. A small workshop on identifying relevant problems to test a package (a unit test) should be held. This can be time consuming, so this must be factored into the prioritization of packages. A workshop in which discussions on what it means exactly when there is agreement between simulations and experiments would also be useful. For example, it is often claimed that there is agreement between a simulation and an experiment but only a 2D code is used. If a 3D simulation is performed it will undoubtedly be different.

Finding: Although there is an apparent spirit of cooperation between the labs, more should be done to promote cooperation across theory, target design, experiments (including diagnostics), code development, and computation.

Comments: True collaboration and focused effort arises when people sit down and work side by side.

Recommendation: The labs should develop a Sabbatical program for lab or university personnel to spend dedicated time at another lab or university.

Finding: Sometimes there is a lack of computational resources on open computers.

Comments: There was concern about running simulations on 2000 cores for 4 weeks or 4000 cores for 2 weeks. These are very small simulations (100,000 core hours). I have heard mixed answers regarding the ability of Hydra to be run in the open only in the Restricted Zone or on open computers at national leadership class facilities such as Blue Waters and Titan.

Recommendation: This should be clarified and if non NNSA DOE and NSF facilities can be used then LLE researchers should apply of computer time at these facilities through the DOE INCITE program.

Reviewer Report: Andrew Randewich

Introduction

The findings from this review are focussed on the balance, metrics and decision making of the Inertial Confinement Fusion (ICF) and High Energy Density (HED) Programme. They span from the working level shot selection on NIF, Z and Omega through to the mechanisms for making National decisions on the overall Programme direction.

The review has been primarily informed by: briefings given in Washington DC May 18 to 22, "The National HED Strategy – Vision 2032" by Alan Wan, Discussion with Alan Wan at JOWOG32S June 3, The Programme Balance white paper by Richard Town, a telecon with John Edwards July 13, and discussion with Bill Goldstein June 10. The reviewer is also a member of the NIF Management Advisory Committee (MAC).

Ignition

Regarding the charge from part I. "*assessments of … prospects of achieving ignition*".

This review is about ICF and High Energy Density physics in general rather than Ignition per se, but the group charge explicitly raises questions on the latter. It must be emphasised that Ignition is a hard problem! Today it remains impossible to state with authority whether or not it will be possible to achieve Ignition on NIF. Nevertheless the position is far from negative. Of significant note is that nationally, but in particular at LLNL, reorganisation has been implemented. The new Programme, capable leadership and functional organisation are highly effective and it is explicitly noted that the alignment of the ignition and other Stockpile Programmes as separate from NIF operations is to be commended. The resulting structure needs to be given stability to tackle the challenges with the tools and people that are being developed. Reviews often happen when change is required but this one is happening immediately after substantial upheaval; it may provide advice and guidance, but continuous change must be resisted.

Recommendation: Resist making multiple successive changes to the HED Programme; rather allow the leadership to exploit the beneficial effect of previous changes.

Importantly, improvements in the tools for calculation are enabling understanding that was not previously possible; an example would be the influence of the capsule support "tent". Nevertheless the approximations that must be made in calculations remain significant (in all three fusion approaches), resulting in the endurance of design "knobs" and leaving a truly predictive capability a distant possibility at best. Improvements in tools for calculation will be discussed later.

Looking at NIF in particular, the diagnostics are now much better than those available to the early National Ignition Campaign (NIC) Programme, which had a minimal set. Diagnostics will also be discussed more in the section on Balance between Codes and Experiments. The improvements in codes, experience and diagnostics together have allowed a significant demonstration in the high-foot campaign. High-foot achieved the first laboratory plasma where alpha heating resulted in significant yield enhancement, but this is secondary to the fact that it demonstrated an ability (absent in NIC) to evolve a platform through control-parameter space and demonstrate a smooth response in diagnosed outputs. Such control is precisely what is required if an ignition design is to be developed, although it is also the case that physics must line up favourably. There is further cause to be positive in the latter regard, as the potential phase space is very wide when a step back is taken from the original point design. At the same time the range of options causes potential problems for the Programme if it becomes too diverse, as explained in the section on Incentives, Metrics and Shot Planning.

Strategic Governance

Regarding the charge from part I. "*evaluation of program balance among ICF approaches"* and from part III*. "effectiveness of the ICF Program's cross-platform and cross-laboratory collaboration".*

At the current time it is not readily apparent that there is a system to allow major decisions to be taken on the overall Z, Omega and NIF HED Programme. An example would be making a cost-benefit decision on whether to move NIF to a Direct Drive configuration or perhaps to switch it to green light. It is important that this governance mechanism for technical direction exists, as lack of such will lead to de facto continuation of the as-is Programmes; the only sufficiently powerful imperatives for large scale change would then be political rather than technical. The required governance does in fact exist, and has been demonstrated by the recent (January 20th) “Lab Directors’ letter”. However perception is important; governance must not only exist, but also be clearly and publicly seen to exist. Apparent ungoverned continuation of the status quo could drive decision making by those less technically equipped to make such decisions.

Recommendation: A visible forum of lab directors to steer fusion for the Stockpile Programme is essential. The “Lab Directors’ letter”, showing commitment to a single National Programme, should not be an anomalous event.

Detrimental tension between teams from different labs has previously been obvious. However it is perfectly possible to have a healthy tension with rational debate on issues. No one actually knows today which fusion solution will achieve Ignition or provide the most effective long-term input to the Stewardship Programme. It was therefore heartening to see presenters across fusion approaches and across the labs standing shoulder-to-shoulder and presenting a coherent overall picture of their work to the reviewer. The discussion will later touch on mutual support for inter-lab campaigns of experiments spanning multiple facilities, which are commended. Omega, for example, is not just about demonstrating Direct Drive, nor about proving extrapolations to NIF scale (which NIC showed us were fraught). It is about mutually closing down phase space on science and materials issues, and many of the findings are pertinent to multiple threads of the fusion Programme.

The degree to which Governance can be applied to balance the Programme is actually not great. The capital cost of the major facilities and their operational costs are dominant over the small amount of funding which delivers the beneficial outputs. A point worth making for such “entry level” situations is that a small amount more Programme can lead to significantly enhanced productivity from the facilities and this can be an opportunity. It also means that there is often not much scope for large-scale rebalancing of the Programme. The Programme balance between Ignition and non-Ignition is discussed in the next section as is the question of whether the Stewardship Programme effectively locks NIF into Indirect Drive mode.

Link from Strategy to Programme

Regarding the charge from part I. “*scientific hypotheses that guide today’s ICF program”*. Note that this discussion has commonality with the Group 2 charge.

The review considered the link between the recently published HED Strategy and the Programme. Firstly it must be stated that the links from Stockpile drivers to types of HED experiment are clear and strong; having a Strategy that lays out what is required and why is a very important step forward and is commended. However, the priorities that thereafter inform the generation of the Programme Plan (what topics are most important or urgent) have not been documented. An explicit example would be the three year NIF ICF Plan, where the link to the Strategy is clear, but the origin of the planned order of activities is not shown. As an aside, due to the Stockpile being the driver for the HED Programme, although the UK and US Strategies are similar, the priorities are different, hence differences exist between the Programmes. This serves to illustrate the importance of prioritisation.

Leaving priorities unstated could certainly generate problems. One point that clearly comes out of the Strategy is that while Ignition will lead to important new classes of Stewardship experiments, there are also large elements of the Stockpile Stewardship Programme (SSP) that do not require Ignition, and much of this work is already being pursued. These experiments include developments in code validation, methods of code application and use of grand challenges to develop designers as well as material data experiments. Furthermore some source experiments are enabled by the alpha-heating regime that has already been demonstrated on NIF. If the highest probability path towards Ignition requires a major change of configuration on one of the facilities, it is not obvious that the information is in place to make the judgement on that change. An example is whether the SSP effectively “locks” NIF into an Indirect Drive configuration.

Of course, in reality the capability to do, and therefore the feasibility of, different experiments depends on many factors such as facility capability, people capability, target deliverability, modelling and calculation maturity, radiation source availability such as Ignition and diagnostic availability. These constraints frequently dominate the planning process over and above the SSP priority. Furthermore, as described already, it may not even be possible to rebalance an entry level Programme more than slightly.

Although the Programme is dictated by reality, the expression of priorities in the Strategy would be beneficial to the enterprise. As a minimum the Strategy should indicate whether the expectation is for short or long term delivery. It would be particularly beneficial if it can be shown how different types of experiments might best be used to burn down Stockpile risks, since maximising technical risk reduction for the Stockpile will allow minimisation of the overall cost of the Programme, and makes simple any business case for HED investment.

Recommendation: It is acknowledged that Stockpile Stewardship priorities have to be combined with realism such as the timing of available capabilities in derivation of the HED Programme plan. Furthermore the plan may need to change when these realities are better understood (for example when Ignition becomes feasible). Prioritisation absolutely does occur, and there is a process. Nevertheless, it is recommended that the Strategy should state the relative priority for the activities from a Stockpile Stewardship perspective, and that when reality is subsequently factored into the planning that this is also explicitly documented.

Incentives, Metrics and Shot Planning

Regarding the charge from part I. "*evaluation of program balance among ICF approaches".*

While Strategic Governance has been discussed already, it is also important that the labs are led to maximally exploit their capital facilities. Such exploitation is incentivised by metrics, but these must be mindfully selected; it is often said "be careful what you measure". NIF, for example, is measured on the number of shots fired per year, but this will not in itself optimise the balance of shots within the Programme to deliver benefit to the Stockpile. Even within a single Programme such as Ignition, the balance between layered cryogenic shots and low power platform and diagnostic development shots would not be well served by as simple a metric as "number of shots fired".

In fact the NIF shots are now allocated via a thorough Governance model. There is indeed a drive to fire more, lower energy, shots, but this is due to the need to preserve the NIF optics rather than the "number of shots" metric. High energy shots are therefore carefully allocated between Programmes. These shots are required because scaling from small to large hohlraums is not yet possible by calculation alone, particularly for rugby hohlraums. Shots are also allocated in order to maximise benefit to the designers who are being developed (this is a major aim of the Programme) and there is only so much data analysis that each designer can do.

Recommendation: Plan with performance metrics, but ensure that they motivate the right Programme. There are many benefits to facility shots. A good metric might be "how many of the best and brightest engineers and scientists stay or are lost each year".

Of course the method of deciding which experimental campaigns to schedule is not simple. NIC experienced tension between pushing for higher yield and trying to understand more about the underlying science. Because the enabling science work has correctly stepped back from trying to achieve Ignition it has become diverse and (as noted from the previous section) lacks priorities, potentially slowing progress on each front. It was stated that the breadth of phase space available to the Ignition Programme on NIF is heartening, but a concern is that this leads to attempts to progress on too many fronts at once, in which case effort is spread quite thinly. There is a necessity to inform selection of future directions, but with tunes being played on the laser pulse length and number of shocks, the ablator material, the hohlraum gas fill or lack of it, and the hohlraum size, shape and material the progress on each front may be limited.

The number of fronts that NIF has been working has decreased recently, and this is commended as focus is required; it takes several years to play out any campaign and that cannot be allowed to stretch. To balance this, a separate activity is being undertaken to ensure that diversity of ideas is not lost; ideas are desirable even if they are quickly discarded. Ideas that survive initial analysis may lead to science on Omega before progressing to NIF, where relatively short, targeted campaigns can be used to determine feasibility. The integrated use of the three major facilities to support each other in this way is commended.

Recommendation: The number of fronts being pursued on NIF has been, arguably, too large, and the current refocus is lauded, as is the initiative to maintain diversity of ideas with the best prospects leading to limited campaigns. This approach should be sustained.

The Balance between Codes and Experiments

Regarding the charge from part II. "*experimental and computational efforts".*

It has been noted that the beneficial part of the Programme is significantly smaller than the capital and operational costs of the major facilities, and so it does not make sense to really compare the scale of these investments to the modelling effort; the former necessarily dwarfs the latter. However given that the facilities are open for business, the question can be posed whether the models are being developed with appropriate balance between experiment and calculation. On the face of things, the code development effort deployed at the three labs is meagre compared to experiment. Actually it is not as imbalanced as it seems, since much code development work is done in support of other Programmes, so the numbers that the group reviewed should not be considered in isolation. Nevertheless, it is the reviewer's opinion that more effort on code development would be beneficial, particularly for the Z Programme. Perhaps surprisingly, code development is also squeezed from the other side. Considerable funding is committed to the procurement of the latest, most powerful, High Performance Computers in the world. While such procurements are essential, these machines are themselves difficult to exploit, and drive a large amount of computer science effort in order to port codes to the new platforms. The Programmes relating to the addition of new physics models and new methodologies for modelling with them are relatively small (in other words beneficial application is being squeezed again).

Recommendation: HPC and experiments dominate costs, and the balance should be swayed (slightly) towards code development, specifically to adding new required physics to the codes and developing their applicability. In particular, it is recommended that Z is supported with more computational effort.

Although there is a need for some rebalancing, one experimental area should be protected, even enhanced if possible. That is the development of new diagnostics. History tells us that on NIF the experimental Programme was all but futile under NIC because the diagnostics were (in retrospect) insufficiently capable to discriminate between shots. Many of the advances since NIC, in particular the high-foot campaign as described earlier, have only been possible because of the improved diagnostic suite available. The current hypothesis regarding NIC, for example, is that drive asymmetry and capsule support were dominating yield degradation. New codes and diagnostics made derivation of this hypothesis possible, and further improvements will be crucial to prove or disprove it and measure the next steps forward.

Recommendation: Diagnostics are an important part of the Programme; on NIF in particular, the plan to increase diagnostic capability should be protected and given high priority even if at the expense of, for example, shot rate.

Conclusions

This review benefitted from the time and attention of a range of lab staff, and the reviewer would like to thank them for their consideration. Equally the NNSA employees who enabled the process are thanked and commended. Overall the decision-making processes from top to bottom of the fusion Programme are not unsound, but enhanced publicity and documentation will lend them more credibility. The Programme is well balanced and there is limited scope for change, but enhanced physics code development is important and diagnostic development is vital. Finally, there are two pleas. Firstly, the leadership across the Programme is outstanding, and will succeed in delivering exceptional benefit to the Stockpile Stewardship Programme if given time and scope; do not perturb the system unduly! Secondly, this Programme is closely scrutinised, and rightly so, but reviews must avoid too much duplication; it loads work on those reviewed and communicates a lack of trust. Instead reviews should use each other's findings and outputs.

Reviewer Report: Sean P. Regan

I. Assess the scientific hypotheses and the prospect for achieving ignition with existing scientific capabilities and facilities; or, if indicated, what would be required to achieve ignition and supporting analysis. Provide an evaluation of program balance among ICF approaches.

Ignition Approach: Laser-driven Indirect Drive

Findings: Research for laser-driven, indirect drive inertial confinement fusion is the most mature of the three ignition approaches. The failed ignition attempts by the low-adiabat implosions of the National Ignition Campaign (NIC) were followed by a high-adiabat campaign that demonstrated experimentally evidence of alpha heating. The maximum primary neutron yield of the high-adiabat campaign fell just short of breaking the 1×10^{16} goal. The energetics of the gas-filled hohlraum is not understood fully. The capsule mounting tent seeds an ablation-front hydrodynamic instability. A significant fraction of the laser energy (up to 200 kJ) cannot be accounted for in the energetics of gas-filled hohlraums. Multipliers (<1) are applied to the x-ray drive to match the trajectory of the imploding shell. Implosion symmetry control in the presence of cross beam energy transfer (CBET) is not deterministic. In contrast, the energetics of near vacuum hohlraums is closer to predictions, but implosion symmetry is challenged by the un-tamped motion of the laser deposition region in the converging Au plasma from the inner wall of the hohlraum. The thrust of the current research is to establish a more fundamental understanding of hohlraum energetics, implosion symmetry, and hot-spot formation. Reducing the gas fill density in the hohlraum alleviates the laser plasma instabilities and provides an opportunity to study the implosion hydrodynamics in a more straightforward manner. The overarching goals for indirect-drive ICF over the next five years of understanding the limitations on convergence ratio, implosion velocity, and stagnation and heating of the DT fuel are on the mark. The strategy of developing 1-D implosions on NIF, focused experiments, diagnostic/computational/facility capabilities should improve the understanding and modeling of ignition target behavior. The move away from the high-pressure (>1.0 mg/cm^3), gas-filled hohlraum is reasonable, especially since these gas-filled hohlraums are very complicated to understand. This research will lead to a better understanding of the hohlraum and implosion, but an ignition design based on the near vacuum hohlraum does not exist yet. What happens if the outcome of this research is that the high-pressure, gas-filled hohlraum is needed to achieve the high areal density and high yield needed for ignition? The program should have some level of hohlraum research investigating the energetics of the high-pressure, gas-filled hohlraums. The LANL program is adding to the LLNL-led, mainline research program alternate designs including double-shell implosions, wetted foam designs, and Be ablators. These innovative designs are worth exploring, but they are sorely in need of a strategy. The challenge is to define the roadmap and decision processes that streamline these innovative concepts into the mainline research within the constraints of limited resources. LANL needs to work with LLNL to define the roadmap and decision processes. LLNL has the most sophisticated 3-D simulation capability for ICF; however, it is very limited. More resources need to be dedicated to 3-D simulations. These simulations would provide insights to the residual kinetic energy in the compressed shell and hot spot.

Comments: Understanding the target physics of a few focused areas is more important than executing an exhaustive experimental campaign of many options of ablator, capsule mounting and hohlraum gas fill.

3-D simulations for every NIF implosion should become routine. Currently it is a heroic effort to complete a single 3-D NIF implosion simulation.

Diagnosing the plasma conditions in the hohlraum plasma using x-ray spectroscopy and eventually optical Thomson scattering (OTS) is critical for indirect-drive ICF.

The gas-filled hohlraum may be the only path to ignition. Energetics research on the gas-filled hohlraum should continue to develop understanding.

Develop an ignition design based on the near vacuum hohlraum and the lower gas-fill hohlraum.

It is essential to chart the progress of indirect-drive ICF using the generalized Lawson criterion for integrated, indirect-drive DT cryogenic implosion experiments on NIF.

At this point, the achievement of ignition with laser-driven, indirect-drive ICF using a 1.8 MJ driver is a stretch.

Recommendations: Continue to perform hohlraum energetics research on gas-filled hohlraums.

Increase the amount of resources dedicated to 3-D implosion simulations. The goal should be to make routine 3-D simulations for each NIF implosion.

Define a strategy for down-selecting the number of ablator materials.

Reduce the number of possible capsule supports to a tractable number for testing.

Define a roadmap and decision processes for the innovative double-shell implosions, wetted foam design, and Be ablators. A clear strategy operating within the resource constraints needs to be articulated.

Maintain a reasonable number of integrated, indirect-drive DT cryogenic implosions on NIF to chart the progress of indirect-drive ICF using the generalized Lawson criterion.

Ignition Approach: Laser-driven Direct Drive

Findings: Research for laser-driven direct drive inertial confinement fusion is conducted on the OMEGA laser system using symmetric direct drive (SDD) of layered, DT cryogenic implosions and on the National Ignition Facility using polar direct drive (PDD). Cross beam energy transfer (CBET) has been identified as a primary physical mechanism limiting the ablation pressure and consequently the hot-spot pressure. The Laser Plasma Simulation Environment (LPSE) code was developed at LLE for a predictive capability of multi-beam laser plasma interactions. The early predictive results are very promising. CBET mitigation has been proposed using beam zooming on OMEGA and wavelength detuning on NIF. Target designs that couple more energy to the hot spot have a clear advantage for achieving ignition in the laboratory. Since direct-drive ICF target designs couple more energy to the hot spot indirect-drive ICF target designs, the required hot-spot pressure and convergence ratio is lower for the direct-drive ICF target design (CR<25 vs. CR=30-40; 150 Gbar vs. 350-400 Gbar). Hydrodynamically-scaled symmetric implosions can be conducted at the 30-kJ-scale on OMEGA to test energy coupling, laser imprint, and preheat. Current implosion experiments have achieved a hot-spot pressure over 50 Gbar. Experiments are needed on NIF to test the energy coupling, laser imprint and preheat on ignition-scale coronal plasmas having longer density scale lengths and higher electron temperatures. Significant resources would be needed to change the beam layout on NIF to achieve a symmetry laser drive on a direct-drive target. In the next decade, NIF may explore the symmetric direct-drive option, but no decision has been made to convert NIF to symmetric beam geometry. Consequently, the only practical option available for laser-driven, direct-drive ICF on NIF today is PDD. LLE has designed phase plates for PDD on NIF, but resources have not been allocated to produce a full set of NIF PDD phase plates. Unlike other laser facilities that have phase plates tailored for particular experiments, NIF only has a single set of phase plates optimized for indirect-drive ICF research. Current PDD experiments use the indirect-drive phase plates in defocused beam geometry to optimize the laser uniformity on target, which works for many of the focused experiments and for low convergence ratio PDD implosions. The PDD experiments on NIF provide an experimental platform to examine preheat, laser imprint, and energy coupling for ignition-scale, coronal plasmas. The coronal plasma of the NIF PDD implosion provides an important experimental platform to test CBET mitigation schemes. Focused experiments are conducted to study preheat from the two-plasmon decay instability using planar targets; to study the early time energy coupling at the pole and equator using a multi-axis VISAR target; and to study laser imprint using cone-in-shell x-ray radiography targets. Integrated PDD implosion experiments on NIF require facility improvements to achieve high convergence ratio (CR=20) PDD implosions. The improvements include a set of intermediate PDD phase plates, multi-FM smoothing by spectral dispersion, and possibly polarization smoothing. LLE proposes to have PDD phase plates available on NIF late in FY18 to demonstrate low-mode control of high convergence ratio PDD implosions, and to decide in FY19 whether PDD or SDD is the viable path forward. The cryogenic hardware needed for direct-drive on NIF depends on this PDD vs. SDD decision. NRL researchers presented experimental results from Nike of laser imprint reduction using thin Au overcoat layers on planar targets, as well as alternative laser beam smoothing schemes. NRL is extending the Au overcoat campaign to the OMEGA laser system.

Comments: A demonstration of CBET mitigation is essential for laser-driven, direct drive ICF. The beam zooming option is being explored on OMEGA and the wavelength detuning option is being explored on NIF.

The PDD vs. SDD decision for NIF hinges on the availability of NIF PDD phase plates to test high convergence PDD implosions (CR=20).

3-D implosions for low-mode drive nonuniformities could greatly benefit the direct-drive ICF research, especially understanding the residual kinetic energy in the compressed shell and hot spot.

Ignition with laser-driven, direct-drive ICF may be more likely to be achieved than it is with indirect-drive, since direct drive can couple more energy to the hot spot.

Recommendations: Fabricate the NIF PDD phase plates to perform high-convergence ratio (CR=20) implosion on NIF.

Integrated PDD implosion experiments are proposed to diagnose CBET mitigation techniques. Explore the possibility of developing a focused experiment to test CBET mitigation techniques.

Develop laser scattering diagnostics on NIF optimized for PDD implosions.

Explore more PDD implosion designs that demonstrate alpha heating, but do not ignite. These high yield shots have HED applications on NIF.

Train scientists across the complex to use the Laser Plasma Simulation Environment (LPSE) code to study multi-beam laser plasma interactions.

Develop collaborations across the complex to develop a predictive capability for CBET, multi-beam SRS, and the two-plasmon decay instability.

A clear understanding of the effects of the thin Au overcoat layer on the laser imprint and the target adiabat should be established. The team at NRL should lead this effort.

Ignition Approach: Magnetically-driven Direct Drive

Findings: Magnetically-driven Direct Drive is the newest research program of the three ignition approaches. Magnetized Liner Inertial Fusion (MagLIF) offers the possibility of a DD equivalent of 100 kJ of DT yield in the near term on Z with modest upgrades to the existing capability. Target designs and pulsed power architecture for future Z300 machine to produce alpha-dominated plasmas and for Z800 to produce burning plasmas were presented. The magnetically-driven, direct-drive ICF presented a range of goals over the next five years covering the critical areas of research including: drive-target coupling, target pre-conditioning, magnetic implosions, stagnation and burn, modeling, simulation and scaling, and diagnostic development. SNL staff leads all of the research except for the laser preheat. SNL relies on laser plasma interaction experts at LLNL to understand the laser preheat phase. The MagLIF research at Z is challenged by the limited shot opportunities, as well as by the limitations of having only a single laser beam. The computational effort often lags the experimental effort. The laser beam can be used either for laser preheat or for x-ray backlighting. The laser-driven, MagLIF experiments conducted on OMEGA and OMEGA EP in collaboration with LLE broadens the scope of the magnetically-driven, direct-drive research approach. The MagLIF experiments planned for NIF will provide an early test of the laser preheat for an ignition-scale target.

Comments: The MagLIF research at Z is challenged by the limited shot number and the limitations of having only a single laser beam.

The laser-driven, MagLIF experiments conducted on OMEGA and OMEGA EP in collaboration with LLE greatly expand the magnetically-driven, direct-drive research approach.

The MagLIF experiments planned for NIF will provide an early test of the laser preheat for an at-scale ignition target.

Recommendations: SNL is wise to involve LLNL LPI experts in the understanding of the laser plasma interactions of the preheat beam. It is a good use of resources across the complex. However, considering that the laser preheat is an integral part of the

MagLIF research, SNL should consider hiring a post-doctoral researcher to develop in-house expertise for the laser preheat stage of the implosion.

SNL should add another laser beam to the Z machine to increase the scientific output from each shot.

SNL should dedicate more experiments to understand and optimize the power flow in the driver-target coupling.

Roadmaps and Decision Processes

Findings: The sophistication of the roadmaps and decision processes for the three approaches varied widely. The laser-driven, indirect-drive ICF showed a broad research program with few details on roadmaps and decision processes. Overarching goals to improve understanding and models of ignition target behavior to either demonstrate ignition or show what is needed in capability and understanding to ignite a target were discussed for the indirect-drive approach. The laser-driven, direct-drive ICF showed a roadmap and decision process based on goals for the hot-spot pressure and mitigation of cross beam energy transfer. The magnetically-driven, direct-drive ICF presented a range of goals over the next five years, but like the indirect-drive research program a detailed roadmap and decision process was not presented. The goals are related to drive-target coupling, target pre-conditioning, magnetic implosions, stagnation and burn, modeling, simulation and scaling, and diagnostic development.

Comments: Roadmaps and decision processes help to focus the workforce on the research priorities.

Recommendations: All three approaches should present a concise one page Gantt chart highlighting their roadmap and decision processes over the next five years.

Program Balance

Findings: The number of full time equivalent personnel working on ICF ignition provided by LLNL, LANL, SNL, LLE, NRL clearly showed that LLNL has the most FTE's in just about every category---experimentalists, designers, code developers and theorists.

Comments: The main area of concern for program balance is hydrodynamic simulations, especially 3-D simulations. Laser-driven, direct-drive ICF and Magnetically-driven, direct-drive ICF are clearly understaffed.

Recommendations: Program balance for the three approaches should be considered, especially in the area of 3-D simulations.

II. Assess the integration of experiments and codes

Diagnostics

Findings: The National Diagnostics Plan led by J. Kilkenny, G. Rochau, C. Sangster, S. Batha is divided into three categories: transformational, broad, and local. J. Kilkenny does an outstanding job leading this effort, which incorporates worldwide scientific/engineering expert input to define the diagnostic development requirements for ICF research. Diagnostic workshops are routinely held for this purpose. The structure of the plan is dynamic; it is fully capable of adapting to evolving diagnostic needs. J. Kilkenny oversees the diagnostic development at every stage from concept to scientific use. The scientific, engineering and fabrication tasks of diagnostic development are divided between LLNL, LANL, SNL, LLE, and NRL based on the efficient use of resources. The ICF program greatly benefits from the National Diagnostics Plan.

Comments: Encourage other ICF research areas to adopt a similar mode of operation to the National Diagnostics Plan.

Recommendations: There should be a succession plan for the leadership of the National Diagnostics Program. Mentoring the next leader is crucial for ICF research.

Computational Models and Predictive Capability

Findings: There is a wide range of computational models and predictive capability for the three ignition approaches. LLNL has the biggest work force and leads the way with the 3-D simulation capability. All three ignition approaches use HYDRA. LLNL developed HYDRA for laser-driven, indirect-drive ICF. SNL has adapted HYDRA for pulsed power applications and LLE has added ray tracing algorithms for direct-drive ICF applications. There seems to be a big knowledge gap or disconnect between LLE and LLNL and SNL and LLNL regarding the successful execution of 3-D simulations.

Comments: 3-D simulations could provide physical insights for many aspects of the implosion, especially for the residual kinetic energy in the shell and hot-spot. More resources should be dedicated to 3-D simulations.

Recommendations: LLNL should provide source code assess for HYDRA to collaborators at SNL, LLNL and LLE. There needs to be a national representative for code development similar to the role of J. Kilkenny in the National Diagnostics Effort. LLNL should provide more support than current level for 3-D simulations of laser-driven, direct-drive ICF implosions and for MagLIF experiments. Hydrodynamics codes need to be written to exploit fully high-speed computing hardware.

III. Assess cross-platform and cross-laboratory collaborations

Findings: An impressive list of cross-platform and cross-laboratory collaborations is outlined in one of the joint white papers (LLE White Paper #3: Cross Platform and Laboratory Collaborations) prepared for this review. The collaborations are divided into the following categories: diagnostics development and collaboration, code development and use, capability enhancements, collaborative experiments, and target development. Some of these collaborations provide support to the academic research community, which brings more students and researchers into the field of ICF.

Comments: These collaborations are crucial for the ICF research program.

Recommendations: Continue to seek and maintain further collaborations.

Reviewer Report: Robert Rosner

Roadmaps and Decision Processes

Findings:

1. There has been a remarkable change for the better in the overall ICF effort over the past 2 years, a change directly attributable to the change in leadership at LLNL. It is evident – from the top at the labs – that there is genuine interest in building a common front to the challenge of achieving ignition.
2. However, I did not see any evidence for a coordinated approach – a 'unified roadmap' across the 3 competing approaches to ignition. What seems to be missing is a definition of the milestones and "performance gates" needed to make the crucial – and hard – decisions about how to proceed: For example, at what point will we be in the position to say, "indirect drive-based ignition is on the way to success, so that the alterative efforts can be put on a back burner"? Or, at what point will we be able to say "indirect drive ignition cannot be achieved by NIF in its current configuration"? In the absence of such a coordinated plan, it is hard to see how the Labs and NNSA will confront some key decision points coming up over the next 5-10 years: What are the key criteria used to decide to modify NIF for polar direct drive? For full direct drive? For abandoning the purely laser-driven inertial approach and moving to magnetically-confined direct drive?

Recommendations: NNSA needs to push the labs to construct an overall ICF roadmap that defines critical milestones and "performance gates" for the three competing ICF approaches. This is the time to take advantage of the fact that the current leadership of the 4 NNSA labs has demonstrated that they can – and do – collaborate.

Program Balance

Findings: While the current allocation balance of funds to the 3 competing ICF approaches appears to be reasonable, it is entirely unclear how this balance will be adjusted in the future – see my comments about *Roadmaps and Decision Process* immediately above. The key elements to sensible program balance are (a) a set of performance gates applied to the three competing approaches and (b) a hard-nosed willingness to readjust program balance if performance gates are not passed.

Recommendations: NNSA needs to put in place an agreed-upon ICF program plan and roadmap – this is an essential prerequisite to any effort to define a sensible program balance.

II. Assess the integration of experiments and codes

Diagnostics

Findings: The diagnostics capabilities of the ICF program are finally getting the attention it so badly needed; and the National Diagnostics Plan presented by Kilkenny at LLNL was everything one could have hoped for. The Plan's focus on the science needs – as opposed to an approach focused parceling out funds to facilities – is to be highly commended; and the National Diagnostics Plan's timetables for instrument development and deployment seem very reasonable. The diagnostics area is a fine example of what can be achieved across the NNSA lab complex if one puts the right person in charge.

Recommendations: NNSA should ensure that the National Diagnostics Plan's timetables for instrument development and deployment can be achieved – meaning that the necessary funding should be put in place. Timely progress towards deciding between competing ICF approaches will require this, for the obvious reason that diagnostics are at the heart of a science-based approach to ignition – independent of the path ultimately chosen.

Computational Models and Predictive Capability

Findings:

1. There has been a substantially increased attention at all of the labs to validate the simulation codes against experimental data, and so substantially improving the physics capabilities – and whence the predictive capabilities – of these codes. This is a direct reflection of moving towards a more science-based roadmap towards ignition, rather than the engineering-based approach that the NIC typified.
2. In some areas, there remain substantial gaps in our understanding of the fundamental physics, resulting in the use of calibrated phenomenological models to fill these gaps. This is highly unsatisfactory because it limits the predictive

capabilities of the codes. I comment in more detail on this issue in the section of *Computational Models and Predictive Capability* immediately following.

3. In some physics areas, especially in the domain of laser-plasma interactions and mix, the existing fundamental/first principles understandings remain incomplete, and the ICF codes are not really capable of a first-principles physics-based predictive capability in such domain. The substantially greater involvement of the weapons program scientists in the ICF effort has been a boon for the ICF simulation effort, as it has enabled direct comparison of codes whose methodology and origins differ substantially. This is especially true for questions regarding turbulence and mix in converging geometries.
4. The LLNL ICF code effort is very limited in the domain of direct drive, and a real effort at a complete V&V program involving the LLNL ICF code (e.g., HYDRA) at Rochester seems to be in the planning stage, and is only now getting a real start. This has been a huge handicap for the direct drive program because Rochester has only full access to validated 2-D codes; its code building capabilities (especially for 3-D) are simply insufficient (both in terms of funds and manpower available) for a credible effort, and so depend on the HYDRA team at LLNL for support; and its use of HYDRA is compromised by the fact that they have limited access to the code development effort.
5. It is striking that in the simulation domain, there is no comparable effort to the National Diagnostics Plan (and there certainly is no National Diagnostics Program). This is a major failing, because it means that the synergies that could emerge as scientists from the 4 NNSA labs collaborate are simply missing; and it is especially amazing that no simulation plan exists across the labs because the predictive codes are an intimate and essential part of the roadmap to ignition. This failure shows up in a number of areas, three of which exemplify the problems that emerge when no real plan is put in place:
    a. It is simply unacceptable that ICF design codes are treated as "black boxes" by subsets of NNSA scientists, a situation that exists at Rochester, which does not get access to the *source* code for 3-D LLNL ICF design codes they need to run. It is difficult to understand how any good scientist can accept the results from codes to which he/she does not have full access; and given sufficient care in limiting access (say, only to scientists with sufficient security clearance), I cannot see any reasonable argument for maintaining the current "black box" regime for these codes. (I might add that what we heard about HYDRA at the LLNL and Rochester deep dives differed significantly.)
    b. The LLNL effort at building an entirely new predictive capability (based on a new framework and an Eulerian scheme), which was discussed at LLNL, appeared to be news to folks at the other labs – certainly this was news to folks at Rochester.
    c. A looming issue is the move at LLNL to a new computer architecture for the next generation leadership machine, a machine that is very unlikely to run the existing design codes efficiently – indeed, there is evidence that the wall-clock time to solution will degrade, and in the absence of sufficient funding for re-tooling the design codes (very likely), this may be a serious problem. This will affect code efforts at all of the NNSA labs because the LLNL machine will be the leading edge "capability" (as opposed to "capacity") machine for the NNSA for a number of years to come.

Recommendations:

1. It may be time for a "laser-plasma science" initiative that is separated from the roadmap to ignition, and has a separate funding line – this may be the only way to get focused attention on a fundamental physics issue that bedevils both direct and indirect drive. Similarly, it may be useful to establish a "mix" initiative – again separate from the main-line ICF effort; in this case (unlike the laser-plasma case) there would be substantial science overlap with the weapons design program.
2. There is a crying need for a "National Simulation Code Plan" – the existing regime is really not the optimal solution to building a first-rate predictive capability. As part of such a Plan, NNSA should focus on three issues in particular:
    a. Building and maintaining codes is expensive – we know this from the ASC /ASCI days. If NNSA is serious about building and maintaining ICF-capable design codes, sufficient funding needs to be made available so that the codes can evolve effectively as the dominant computer architecture evolves.
    b. At minimum, there needs to be a clearing house approach to code developments, so that scientists at all NNSA labs are aware of what is going in this domain at all the labs.
    c. NNSA should force the Labs to go to an "open source" code approach, at least within the classified domain. This would go a long way towards opening up the code efforts to effective collaboration across all of the labs.

III. Assess cross-platform and cross-laboratory collaborations

Findings: The extent of collaboration between the three weapons labs and Rochester on ICF has increased substantially over the past 2 years. This is true especially in the area of magnetically-driven direct drive, where Rochester is clearly developing a very active collaboration with Sandia. The direct drive effort at NIF remains modest, with a few LLNL scientists now getting involved in the polar direct drive design effort. Finally, LANL has substantially increased its involvement at NIF, which is highly desirable, given that LANL's approach to modeling (including its simulation design codes) is not the same as LLNL's.

Comments: A significant impediment to increased collaborative activities is the limited funding available to support such activities. In an environment in which funding is limited, scientists will tend to work on those programs that are best (and most reliably) funded. So, to no surprise, LLNL and LANL scientists will (by and large) work on indirect drive, Rochester scientists will largely work on direct drive and Sandia scientists will largely work on magnetically-driven direct drive.

Recommendations: The obvious recommendation of simply suggesting more funding is clearly not the right answer, primarily because it is so unlikely to occur. But perhaps NNSA could consider starting a competitive "ICF innovation fund", with a reasonable level of funding (perhaps in the range of $5M-$10M), that supports cross-platform and cross-laboratory work. I suspect that some of the very best scientists at the 4 labs will go after this sort of funding – and the program as a whole might then benefit well out of proportion to the amount of money actually spent.

Reviewer Report: Susan Seestrom

My comments on the program are essentially limited to program issues. I think that the program is GREATLY improved from in the past. It appears to be more open, it attempts to address a broad set of scientific issue that are relevant to achieving ignition. In the past it seemed to me that the program was always grasping at a single issue that would be the key to ignition, and the projections of performance were based on the most optimistic possible calculations, ones which many members of the community would think incredible.

That said, there still seems to be a danger of putting too much focus on the next big thing (e.g. vacuum hohlraums and/or STUD pulses) or worse, jumping onto direct drive as the best path. It is essential that the program lay out a roadmap of all the issues that must be addressed to achieve ICF predictive capability, and create a balance program to address these issues. The national diagnostic plan will be an essential part of this plan. I am also concerned about the present role of Los Alamos. It was telling that there was no deep dive held at LANL given the number of science areas that are ones of historic LANL strength (like LPI) and the fact that if NIF/ICF do not become significant tools for designers at LANL there truly is NO national program.

Finally - I have a concern regarding the LLE program. It is not clear to me whether weapons physics done with an igniting Direct Drive system would be as relevant as one provided with indirect drive. Further, since the real weapons codes are not and cannot be used model the LLE work it is difficult to see how it will provide an important test of predictive capability relevant to the weapons program.

Reviewer Report: Stephen Slutz

I. Assess the scientific hypotheses and the prospect for achieving ignition with existing scientific capabilities and facilities; or, if indicated, what would be required to achieve ignition and supporting analysis. Provide an evaluation of program balance among ICF approaches.

Ignition Approach: Laser Indirect Drive (LID)

Findings and comments: The National Ignition Campaign (NIC) was a tightly focused effort based heavily on numerical simulations to obtain fusion ignition in the laboratory. Many scientists outside of NIC believed that too much confidence was placed on numerical simulation and this is ultimately why ignition was not achieved. I believe the story is more complicated and is still unfolding. In particular scientists within the NIC recognized that numerical simulations were not accurate enough to completely determine all the parameters of the design and that experimental tuning would be required. As an example, very accurate shock timing (<100 ps) is needed to keep the fuel on the low adiabat required to reach ignition within the energy constraints of the National Ignition Facility (NIF) laser. Numerical simulations are not accurate enough due to uncertainties in physical quantities such as the equation of state (EOS). Shock timing was successfully tuned using the keyhole target configuration and low adiabat implosions were produced during the NIC. Drive symmetry is another example that requires tuning, but in this case the required symmetry has still not been achieved. In principle a sufficiently large hohlraum could provide the required radiation drive symmetry, but the larger the hohlraum the more laser energy required to reach sufficiently high radiation temperatures to drive the capsule. High-energy lasers are expensive so the hohlraums designed for the NIC were just large enough to provide adequate symmetry according to numerical simulations. Since there is not much space between the capsule and the hohlraum wall, the design included a helium gas fill to mitigate hohlraum wall motion. Early in the NIC it was discovered that the opacity and thermal conductivity models were not adequate. Modeling the hohlraum with Detailed Configuration Accounting (DCA) for opacity and either nonlocal transport or a large flux limiter for electron thermal conduction (the high flux model HFM) resulted in much better agreement with experiment. Calculations using the HFM result in somewhat colder He gas fill and consequently stronger inverse bremsstrahlung absorption and Laser Plasma Instabilities (LPI) of the laser beams. Consequently the inner cones did not deliver enough energy to the midplane of the hohlraum, which resulted in pancake shaped implosions. The solution adopted during the NIC was to adjust the wavelength difference between the laser cones to enhance Crossed Beam Energy Transport (CBET) from the outer cones to the inner cones. This approach led to more symmetric images of the stagnated fuel, but has not led to a symmetric drive over the complete duration of the implosion. NIF capsules require high convergence ratios (>30) to obtain ignition. It has been shown numerically [V.A. Thomas and R.J. Kares PRL 109, 075004 (2012)] that small asymmetries are amplified by high convergence implosions and result in turbulent flow. Such turbulent flow [Residual Kinetic Energy RKE] does not transfer to thermal energy fast enough to heat the fuel at stagnation. Consequently a high level of symmetry is required throughout the implosion. It is now recognized that it will difficult to adequately control CBET in the high gas fill (HGF) hohlraums when the inner cones need to be boosted by more than 50%. Consequently lower gas fill densities are being considered. Since this will result in more hohlraum wall motion, larger hohlraums are also being considered.

In addition to inadequate hohlraum drive symmetry; the NIC capsules were not adequately robust to instabilities such as the Rayleigh-Taylor (RT) and the Richtmyer-Meshkov (RM). This was demonstrated by the High Foot design, which obtained yields much closer to the 1D simulated yields. It has also been discovered that the thin films (tent) used to position the capsule within the hohlraum leaves scars that seed the RT instability, which grows to large amplitudes during the implosions. Due to insufficient zoning resolution, simulations performed before the experiment did not predict this scarring. The effect is more pronounced on the low adiabat NIC capsule implosions and could have degraded performance as much or more than the asymmetric drive. More recent high-resolution simulations have been performed that capture the effect of tent scarring. Several approaches have been proposed to position the capsule without detrimental scarring. These approaches will be simulated and tested experimentally.

Recommendations: A means to position the capsule without detrimental scarring must be found, but given the number of options this should not pose a fatal flaw.

The hohlraum/capsule configuration should be modified to improve symmetry without the need for CBET. This will require larger hohlraums with a reduced gas fill density. Larger hohlraums require more energy to maintain a given radiation drive temperature. Some of this energy may be obtained through reduced LPI and backscatter, but it is probable that adequate symmetry will only be achieved at lower radiation drive temperatures. Beryllium ablator capsules have higher hydro efficiencies and can thus operate at lower drive temperatures than either plastic or High Density Carbon (HDC) ablator capsules. The implosion time of a Beryllium capsule is shorter than a plastic capsule making it more compatible with hohlraums with low fill densities. Furthermore, it is relatively easy to dope Beryllium which enables the control of hard x-ray preheat. Consequently, high priority should be given to the study of Be capsules.

There is presently not enough information to know if ignition can be achieved on NIF. Hohlraum designs need to be found with low enough gas fill that LPI is minimal. These hohlraums must be large enough so that wall motion is tolerable. Power balance to the laser cones can then be used tune the low mode symmetry without the use of CBET. Diagnostics need to be developed to accurately determination of the radiation drive asymmetries on the capsule over the entire implosion. Once control of the low mode drive symmetry is obtained Big Foot and wicked foam experiments should be used to determine what convergences are possible with near 1D performance. 3D asymmetries from the finite number of lasers in each cone may limit the 1D like convergence to a value below that needed for ignition and larger hohlraums would be needed to smooth out such asymmetries. The minimum hohlraum size should be found such that ignition relevant high-convergence implosions have 1D like behavior. The laser energy needed for ignition could then be determined with confidence.

Magnetization of the gas within the hohlraum should result in higher temperatures and thus reduce absorption and LPI. Magnetization of the capsule could also reduce ignition requirements. Both of these options should be studied.

Double shell capsules have two advantages over the single shell designs, which are presently being studied. The required radiation drive temperature is lower and the wall motion will be easier to control due to the short pulse length requirement. On the negative side it is unlikely that double shells will be less susceptible to drive asymmetries due to the overall high convergence and the fabrication of double shell targets is more complex. If the study outlined above indicates that single shell designs will not lead to ignition on NIF double shells might provide 1-2 MJ yields, but there does not seem to be a high-yield option.

Ignition Approach: Laser Direct Drive (LDD)

Findings: LDD can deliver five times the energy to ICF capsules as LID. This should increase margin, but there are associated disadvantages. Increased beam smoothing is required to avoid *imprinting* because the laser beams are in direct contact with the capsule. In addition energy can transfer from a beam going toward the capsule to a beam going away from the capsule. This is referred to as cross beam energy transfer (CBET). This process reduces the fraction of the laser energy that is actually deposited in the capsule. Most of the CBET occurs from beams at impact parameters equal to or slightly larger than the capsule radius. Thus simply decreasing the size of the beam focus reduces CBET, but at the expense of increasing drive nonuniformities. Studies indicate that adequate uniformity and reduced CBET can be obtained when the beam spot size is about 80% of the capsule diameter. A conceptually simple approach to CBET modification is to keep the laser spot size roughly 80% of the capsule diameter as it implodes. Producing a time-dependent laser spot size is referred to as zooming. Plans have been made to use this approach on Omega. The zooming will be accomplished using a two-region phase plate. The central region will focus the early part of the beam to a spot size appropriate for the initial diameter of the capsule and the outer region of the phase plate will produce a smaller laser spot size appropriate later in the pulse when the capsule has begun to implode. Another approach is to shift the laser wavelength of opposing beams to keep the CBET process out of resonance. This is the approach that is being tested on NIF.

Experiments on Omega indicate that CBET and LPI can be modeled adequately. However, LDD NIF capsules will be significantly larger and LPI becomes stronger with longer plasma scale lengths. NIF experiments are planned to determine if CBET and LPI are still understood and well modeled for larger capsules.

LLE is pursuing hydro equivalence as a metric to determine the expected performance of direct drive capsules on NIF. Fuel pressures of about 120 GB will be needed for a direct drive ignition capsule on NIF. Similar pressures will need to be demonstrated on Omega. This is significantly larger than the 56 GB that has been demonstrated on Omega. The plan is to improve that number by mitigating CBET, using thicker shell capsules, and improving beam pointing (symmetry).

Comments: Symmetric direct drive SDD on the NIF will require a reconfiguration of the laser. This will be expensive and would preclude indirect drive experiments at full laser energy while the laser is in this symmetric configuration. Since laser-driven hohlraum experiments are useful for weapons physics experiments, the decision to make such a transition will be difficult.

Recommendations: In addition to achieving high pressures (>100 GB) with SSD, polar direct drive PDD should be studied on Omega. If these studies are favorable, the NIF could be converted to PDD much less expensively than to SDD. The additional smoothing of the lasers needed for PDD will only improve LID experiments. If PDD direct drive does not work the decision to reconfigure NIF to SDD will be better informed in knowing that it is necessary for direct drive.

Independent of whether PDD or SDD is recommended for NIF experiments, the method of CBET mitigation that will be used on NIF should be tested on Omega with hydro equivalent capsules that obtain the high pressure (>100 GB) needed for direct drive NIF ignition capsules.

Ignition Approach: Magnetic Direct Drive (MDD)

Findings: Magnetically driven implosions are highly efficient when compared to either laser drive options. The challenge is create fusion conditions for cylindrical implosions, which are significantly slower (~100 km/s) than is typical of conventional ICF (>300 km/s). Magnetized Liner Inertial Fusion (MagLIF) is an intriguing approach to magnetic direct drive. Initial experiments have demonstrated that the approach can create fusion conditions and sufficiently compress the magnetic field to trap α-particles, but much more research is needed to determine the ultimate potential. The process of heating the fuel with a laser is not well understood or modeled. Initial simulations of this process using Lasnex and Hydra significantly over predicted the fraction of laser energy that would penetrate the Laser Entrance Hole (LEH) foil and be deposited within the foil. This disagreement is probably due to LPI that is not modeled sufficiently well by either of these two design codes. In addition to reducing the fraction of the beam that penetrates the LEH foil, LPI probably causes the beam to filament and spray. Filaments that heat the electrodes or the liner could mix this material into the fuel and degrade the yield. This is supported by a recent experiment with beryllium electrodes that performed significantly better than a number of previous experiments that used aluminum electrodes. Note that MagLIF implosions are particularly susceptible to mix due to the long implosion times.

Collaborations have been formed between SNL, LLE and LLNL to study the laser heating process. Experience within LLE and LLNL indicates that beam smoothing is critical the controlling LPI. Z Beamlet has no beam smoothing, since it is not required for backlighting, which was its original purpose. Random phase plates are being prepared to smooth the Z Beamlet laser. Controlled laser preheating of the fuel will allow the study of MagLIF scaling with current, laser preheat, and the initial magnetic field.

Comments: Magnetically driven direct drive has the potential to be a technical surprise. Russia has plans to build a 50 MA pulsed power machine with the goal of obtaining 25 MJ yields. The Z machine can provide at most about 27 MA. China has built a scaled down version of the Z machine and is in the process of copying most of the published Z experiments. It is presently unknown when they will build a larger machine.

Simulations indicate that very large yields (~ 10 GJ) and gains (~1000) could be possible with ice burning MagLIF, albeit on machines producing much larger currents than Z. Certainly such a platform would be desirable for weapon physics experiments.

Sandia has developed future pulsed power machine designs based on LTD technology, which is significantly more efficient than Marx based pulsed power machines. Calculations indicate that approximately 9% of the capacitively stored energy could be delivered to a MagLIF liner as compared to about 5% using Z. An LTD design generating 48 MA could fit into the existing Z building. 2D Lasnex simulations of MagLIF driven by such a pulsed power machine produce 18 MJ yields with a simple gas-burning target.

Recommendations: Numerical simulations indicate that MagLIF could produce DT yields of ~ 100 kJ when driven by a peak current of 25 MA, with a fuel preheat of ~6 kJ, and an initial field of 30 Tesla. The MDD program should work toward achieving these conditions in the next 5 years on Z. Achieving such a goal may require the development of liners that are benign to mix. This could be accomplished by forming a thin (several μm) layer of DD or DT ice on the inside of the liner.

More importantly, the MDD program should work toward understand the scaling of MagLIF performance as a function of design parameters (current, fuel preheat, magnetic field, fuel density, liner aspect ratio, and liner material) over as large a range as possible on the Z facility. This scaling will be needed to determine if at some future date a next generation pulse power machine should be built to study MagLIF at higher drive currents.

Program Balance

Findings: Indirect drive on the NIF is and should be the main effort, but not to the exclusion of alternatives. The total number of FTEs working at the laser laboratories is 55.5 at LLNL, 30.6 at LLE, 19.8 at LANL, and 13 at NRL for a total of 118.9. The number of FTEs working on pulsed power is 16 at SNL. The magnetic direct drive effort is less than 12% of the total ICF effort.

Recommendations: Pulsed power is less expensive and much more efficient than lasers. MagLIF has the potential for high yields with affordable costs. A larger fraction of the effort should be directed toward this approach

II. Assess the integration of experiments and codes

Computational Models and Predictive Capability

Findings: LPI physics is not adequately integrated into the design ICF codes such as Lasnex and Hydra, even though LPI physics affects all three ignition approaches. Kinetic effects can be important in near vacuum hohlraums and may be important in MagLIF targets. Current driven systems are not well modeled. In particular vacuum regions have to be treated with artificial density and magnetic floors.

Comments: ICF is the arena for testing weapons designers, experimenters, and diagnosticians. In the attempt to achieve ignition on NIF we have learned that our codes do not predict ICF implosions adequately. Is this due to physics particular to ICF (LPI or kinetic effects) or something more basic? The physics particular to ICF experiments will have to be modeled better to answer this question.

The nation spends considerable funds developing design codes. These codes should be available to all ICF researchers with need to know and the proper clearance, both for simulation purposes and for code development. They should not be considered the property of a particular laboratory or person.

Recommendations: LPI physics needs to be improved in the ICF designs codes. This is critical to all three ignition approaches. Better modeling of kinetic effects is needed for low gas density hohlraums. Extended MHD is needed to model magnetic direct drive. This includes modeling transport such as the Nernst effect and removing density floors so that current paths can be more accurately calculated.

Within the constraints of need to know and clearance level, design codes should be shared more readily among the laboratories.

III. Assess cross-platform and cross-laboratory collaborations

Findings: Collaborations across both platforms and laboratories have been increasing. This trend should be encouraged. Such collaborations result in cross-fertilization of ideas and provide a means to balance efforts on the various approaches without actually hiring, firing, or moving people.

Comments: It is my impression that many weapons designers are content to reanalyze old experiments repetitively rather than engage in experiments on the ICF/HED facilities. Although there is certainly some value in being the custodians of historical experiments, in my opinion it is not sufficient to truly understand how difficult it is to use simulation tools to design and predict the performance of actual experiments. If at some time substantive changes to the stockpile were needed, I would hope the responsible designers have had their metal tested on real experiments.

Recommendations: Collaborations between the laboratories and platforms should be encouraged. Weapons designers should be strongly encouraged to participate in HED/ICF experiments to sharpen their skills.

B.2 Group 2 Reviewer Reports

Reviewer Report: David H. Crandall

Summary:

The Inertial Confinement Fusion (ICF) program and related High Energy Density (HED) science is following the "NNSA's Path Forward to Achieving Ignition in ICF" known as the "path forward" plan issued in 2012. In addition a number of changes have been made at the weapon laboratories (LLNL, LANL, SNL) to focus value from ICF on the weapon interests of the Stockpile Stewardship Program (SSP). The alignment of the ICF sub-program with needs and interests of science-based SSP has never been better. This conclusion is consistent with the letter from weapon laboratory Directors to NNSA Administrator Frank Klotz of January20, 2015. This is the overarching finding:

The ICF program is fulfilling its role as a primary instrument for science basis in Stockpile Stewardship.

From the beginning of the SSP in the 1990s, the vision was that the ICF program and its major facilities, the National Ignition Facility (NIF), the Omega laser and Pulsed Power (Z), would provide physics insights and data for warhead analysis and would provide nuclear burn challenge to partially replace nuclear explosive device testing. In the program planning, it was originally articulated that nuclear warhead designers would be trained and tested by designing and conducting inertial fusion experiments. That direct value to training and testing designers was not strongly followed by the US design laboratories until now. It was expected from the beginning that the grand challenge of fusion ignition was to be a primary attractor for the talent needed in SSP.

As recently as 3 years ago, this vision appeared to be in jeopardy. The ICF program was unsuccessfully seeking "ignition" (more fusion energy released by implosion of miniature deuterium-tritium filled pellets than input by lasers into the target at NIF). And, foolishly, the primary focus of this effort appeared to be getting enough energy gain to become the basis for a major Inertial Fusion Energy (IFE) program with nuclear-weapon-related interests being secondary to that IFE direction.

Now, through change in laboratory management at all of the labs, especially at LLNL, and through technical inventions at the laboratories, the program direction has completely changed. The failure to obtain ignition was a critical catalyst. A clear reminder was delivered to the SSP that codes predicting nuclear explosion cannot yet be trusted outside a tested range; a lesson of immense value. In addition, the next step in progress at the NIF, the "high foot" target implosion with notable and predictable yield, was achieved by weapon designers. Further, new concepts for using fusion yield (even at today's levels) at ICF facilities to clarify "boost physics", to observe material behavior in weapons-relevant conditions, to help interpret past nuclear tests and to provide tests of nuclear weapon effects, have been developed. The alignment of ICF program management with nuclear weapon program interests within each major lab and among all of the ICF laboratories has become by far the strongest ever. While these new program aspects are nascent and not yet fully proven, ICF research of direct value to SSP is underway, credible and highly promising.

The critical manpower to realize the potential of ICF/HED is growing now. The integration of ICF design and nuclear weapons design has improved the role that ICF/HED has in manpower development for SSP. At LANL the funding for ICF/HED is sufficiently constrained that some important ICF design work that has this symbiosis with nuclear weapon design is languishing. This can be redressed with modest funding enhancement at LANL.

The young researchers we met at the labs are potential stars for SSP and the supply of new talent from high energy density physics programs at universities is continuing with healthy competition for the brightest and best graduating students. However, there is concern that the leading professors at places like MIT, Cornell, Michigan, Washington State and other universities are near retirement without clear replacements and the intent to sustain these programs is not apparent at some of the universities. Also, the funding of these university programs is under pressure and is too lean to sustain these programs, a problem that can be redressed by relatively minor adjustments to programs in NNSA and the DOE's Office of Science.

With regard to manpower the finding is:

Current manpower, required to realize the value of ICF/HED for SSP and to develop future nuclear weapon designers, is healthy and strengthening, but near term increments for LANL and the universities are required to sustain manpower supply.

The long-term development of ICF for SSP is unclear. Since the 1980s there has been a vision of quite high laboratory fusion yields (500 to 1000 Mega Joules of fusion energy release) to address interests of the SSP in an environment without nuclear explosive device testing. However, in spite of ICF program advancement, there is inadequate technical basis today for thinking that such levels of yield from laboratory inertial fusion can be obtained. The potential value for SSP in such high yields remains valid but may be at least partially addressed at lower yields through the inventions of new approaches now occurring at the labs. Science-based SSP can progress significantly with ICF yields from 10s of Kilo Joules (KJ) to 10 Mega Joules (MJ). About 30 KJ of yield is obtained at the NIF today, but 10MJ requires ignition and gain of about 5. With continuing support and anticipated technical progress, over the next decade, experimental results and predictive computer code advancements will evolve to clarify what is possible.

Intent to support the ICF/HED endeavors for up to a decade within SSP is critical for national security and is recognized as a substantial US government commitment of several billions of dollars over that time. During that time warhead requirements may also evolve and new threats may emerge. The evolution of the US stockpile and its role in national security is likely to continue to change over this decade. In order to analyze emerging nuclear threats from antagonists against the US and to mitigate nuclear explosive device threats that may occur, the need for nuclear design expertise may grow over this decade. Given these likely changes and uncertain progress in ICF, new evaluation of ICF for SSP should occur about a decade from now with appropriate checkpoints during the decade. During this decade, the ICF program can play a critical role in sustaining and advancing nuclear design expertise essential to national security. ICF/HED has value for fundamental science and long-term fusion energy potential and that value is likely to grow during this decade if there is increasing ICF capability of current facilities and manpower. All of these factors lead to uncertainty as to the long-term value of ICF for national security interests with great dependence on the progress made in ICF during the next decade.

Thus the finding on long-term value of ICF is:

ICF can lead in development of nuclear design expertise over the next decade, retaining ignition as an important goal and applying the experimental approaches and code development underway at the laboratories. The role of ICF for SSP, and other applications, beyond the next decade remains unclear with great potential dependent on progress within ICF.

Background: This report is prepared for the National Nuclear Security Administration within the Department of Energy under contract with TechSource, Inc. as part of the “2015 IFC/HED Review” Group 2 on Programmatics. The analysis and opinions expressed are solely those of the author. The “Charge to Review Group” was supplied in May 2015 to the selected reviewers.

In conducting this review, the author participated, with others, in 3 days of presentations at NNSA headquarters from NNSA leaders and the weapons Laboratories; LLNL, LANL and SNL. An additional 5 days of “deep dive” discussions were held at the 3 laboratories and at a meeting at NNSA headquarters that included representatives from Laboratory for Laser Energetics, Rochester New York and Naval Research Laboratory, Washington, DC. Specific agendas for those meetings can be supplied by NNSA. In addition some time was spent reviewing a number of ICF and Stockpile Stewardship program planning documents and reports (classified and unclassified) and requested “white papers” from the ICF laboratories; all documents are available from NNSA. This was a highly organized and appropriate process with particularly detailed attention by Njema Frazier of NNSA, Alan Wan of LLNL, Don Haynes of LANL, Dawn Flicker of SNL and Craig Sangster of LLE.

Full Report:

Alignment of ICF with Stockpile Stewardship:

Alignment Background: Since nearly 2 years ago a process of realignment of the ICF program, particularly at LLNL, has been underway. The failure to achieve ignition at NIF was a critical catalyst. An intense program element called the "National Ignition Campaign (NIC)" was conducted during FY 2010 through FY 2012. The campaign had control of the completed NIF and benefitted from marvelous capability in target fabrication and fielding and steadily increasing diagnostic capability at the NIF. This was leading edge science in a physical regime never achieved previously in the laboratory. Arguably, the NIF with associated targets and diagnostics is the most complex engineering accomplishment ever by mankind and the science was expected to be a "grand challenge". Never the less, optimism was high that ignition (release of fusion energy exceeding the NIF laser energy supplied to the target that contained an imploding capsule of deuterium and tritium in a frozen shell with central gas) would be achieved through systematic engineering of the laser properties and target attributes. The overall ICF endeavor had been underway since 1960 with ever increasing sophistication but with unfulfilled expectations at times along the way. The detailed codes and technological capability achieved by 2010 gave observers and participants in the ICF program confidence that rapid progress might occur. However, the NIC failed; a final report of a review initiated by DOE Under Secretary for Science, Steve Koonin and completed with Koonin's reviewers, concluded that "the NIC was completed on Sept. 30, 2012 having achieved all project goals with the exception of its grand challenge scientific goal of achieving ignition". The yields achieved were about 1000 times less than predicted by simulations. The Koonin reviewers thought that the capsule did not maintain geometrical integrity for the high convergence of ~35 to 40 times used in the NIC and felt strongly that the capsule implosion cases that did match prediction must be found and experiment direction then determined in part through simulation excursions from those cases.

During the NIC, leadership at LLNL, particularly NIF Director Ed Moses, raised expectations of a rapid (10-15 year) development of LIFE (Laser Inertial Fusion Energy based on ignition at NIF). While inertial fusion energy (IFE) is a laudable goal and plausible on a longer term, the expectations raised for LIFE appeared silly, as advertised, to most knowledgeable observers. In part because of the LIFE initiative, Under Secretary Koonin established a review of IFE by the National Academy of Sciences/National Research Council. Their sanguine report in February of 2012 is detailed and lengthy and useful in the long term; it contains the statement "the Committee judges that the potential benefits of inertial fusion energy justify it as a part of the long-term US energy R&D portfolio, recognizing that the practical realization of fusion energy remains decades away". The DOE's policy on inertial fusion energy established by Secretary Watkins in 1992, following advice from the Fusion Policy Advisory Committee, remains valid; the nuclear weapons program seeks fusion ignition that can support inertial fusion energy as well, if achieved.

In the meantime, the nuclear weapons program did achieve some important results (such as data applying to "energy balance" in nuclear explosions) at NIF during the NIC but the weapons program was highly limited in operation time and was left to explain confidence in nuclear warheads in spite of the NIC failure. The weapons program leaders made that explanation patiently and convincingly, and, as well, initiated less ambitious experiments with less compression of the NIC capsules but with results that largely matched predictions and achieved an order of magnitude increase in fusion yield over that typically obtained during the NIC. Current experimental results show clearly that both hydrodynamic instability distortion of the capsule and laser plasma interactions, that interfere with the x-ray drive of the capsule during the laser pulse, caused the failure of the NIC. Current research seeks to mitigate those dynamic distortions. It would be appropriate for the LLNL/NIF web sites to have an explanation of the failure of NIF that informs the interested public of the state of knowledge about ignition experiments at NIF.

Over the past 3 years the ICF endeavors have focused appropriately on physics challenges and focused physics experiments with a mix of seasoned scientists and engineers with young and imaginative researchers. Also, during the past 2 years, increased attention has been given to development and application of computer codes to predict ICF performance (even more attention may be appropriate) and to development of new means of using the results from ICF to test specific physics of nuclear explosive devices. It is these elements of physics focus and technical progress, along with management changes for programs at all 3 weapons laboratories but particularly at LLNL, that have led to the significant change in alignment of the ICF program with nuclear weapon needs.

Management realignment:

Integrating designers: at both LLNL and LANL the ICF designers are now part of the nuclear weapons organizations and managed in common with nuclear weapon designers, breaking 40 years of traditional separation. And, it is expected that designers will participate in both ICF experiments and warhead design activities. This may be the most critical of the management realignments that have occurred. This enables the practice of training and testing of nuclear explosive device designers through ICF experiments. That practice was discussed positively at both design laboratories and is under way but specific outcome examples are yet to be developed and are needed.

Developing and testing designers: new technical leaders of the nuclear weapon design teams at both design labs have knowledge of ICF and appreciation of how it can apply to developing and testing of nuclear warhead designers. There are some healthy differences between the 2 labs in how this approach may be expanded, but a common sense of purpose in applying this approach. The technical leaders must give this approach continuing attention and assessment for its potential to be realized. It is easy to argue that this change should have been made long ago, but more important to nurture it now. This mixing of designers and technical design objects is a difficult management challenge but one that cannot be improved from outside the lab teams.

Evolving the role of ICF: during the "deep dive" discussions at LANL there was specific discussion of the value of ICF design experience versus the prototyping of modified nuclear warheads for development of nuclear weapon designers. Both can have significant value. ICF designs are not nuclear weapons so lack tests of critical attributes but nuclear weapon designs are unlikely to get developed far enough to truly test the designers. As clearly stated by Michael Bernardin at LANL, "nuclear weapons design is about energy multiplication" and ICF designs can be also through nuclear processes. ICF designers can experience the full range of design ideas completed in specific engineered hardware with outcome tested; an integrated experience necessary for confidence in nuclear weapon designers. A specific ICF design and test may require about $10M for one case while a nuclear warhead prototype design with some component testing requires more like $300M. There is no conclusion to the question of which activity has greatest value, and there may never be, but the value of ICF design activity is becoming clearer and should be applied and evaluated at LLNL and LANL.

Planning: the Directors' letter to Klotz of January 20, 2015 initiated some more integrated planning among the laboratories. They said, "We and our delegates will be meeting regularly in 2015 to ensure progress towards this integrated and coordinated National HED effort". Some meetings have occurred but not regularly and not fully coordinated. The Director intent needs to be more urgently acted upon. The willingness of the ICF laboratory community to do this appears high; the important participation of Sandia, LLE, NRL, General Atomics and Schafer is possible and those institutions do have some appreciation and understanding of the new alignment of ICF with the weapons program. Some issues like the role of the new "priority research directions (PRDs)" in ICF program execution remain unclear and need the integrated planning promised. Again, it is important that the laboratory technical leaders conduct and own this process; NNSA can observe and encourage this planning but it needs to be owned by people conducting the program.

All of these management factors of integrating designers, developing designers, evolving the role of ICF and integrating planning are critical to the new alignment of ICF with weapon program interests. Progress is nascent but needs continuing leadership attention.

New technical factors:

Use of low yield: invention of methods to use HED and ICF for weapon analysis has been critical. In the original conception of the role of ICF in Stewardship, a central theme was to provide robust ignited fusion capsules that could be degraded or caused to fail by modifications that were nuclear warhead relevant. The concept of ignition was central but its applications to weapon interests were not developed in sufficient detail. Along with the failure of NIC, some new, classified, ways to use yield at levels less than ignition, for weapon-specific results, have been developed. Useful examples were presented during the May 18-20 meeting at headquarters and at the deep dives at the laboratories in July. Some of these experiments will examine degradation of yield by specific modifications of the target; up to about 10MJ, additional experiments of this nature are possible as yield increases. At LLNL, specific experiment designs made clear that "boost-relevant" physics can be studied with yield that is now obtained at NIF. Most of the weapon physics at NIF and much of the weapon effects studies at NIF and Z are now based on yield rather than ignition per se. Experiments are being initiated using today's yield at NIF and similarly experiments using yield at a Z-like device are being conceived. These approaches were not known in detail when ICF was included in Stewardship planning, but are now the basis for ICF application for nuclear weapon interests.

Materials behavior: experiments on materials behavior when high energy density is applied are being done at NIF, Omega and Z and other facilities. The advent of short duration intense lasers and other light sources (one example, the Linear Coherent Light Source at Stanford) and the improved intensity and control of Z-pinch discharges allow laboratory study of higher pressures and higher energy density in matter than previously possible. Some of these experiments are of fundamental science interest and a number are directly for weapon applications. As stated by Rip Collins during the review, "all of the kids want to know if life can exist on other planets, controlled by the melt curve of Iron, but they are not so much interest in melting of Plutonium".   The science is similar and requires similar specialized techniques. Through these experiments ICF both contributes to fundamental science and benefits from such science. ICF facilities can both apply high energy density to materials and examine the fast time responses of the materials. Experiments at Z and NIF on Plutonium are yielding unprecedented details of the response of this unique material to high pressure and high energy density. Of course, these data are of high value in analysis of the detonation of nuclear devices. These non-ignition experiments were not known to be possible when Stockpile Stewardship was conceived but are a notable part of its success today.

Hydrodynamic behavior of plasma: measuring the evolution of plasma instabilities and dynamic plasma behavior is important fundamental science. Plasma instabilities often are combined or integrated in intricate ways, difficult to predict, but apparent in astrophysical phenomena, HED/ICF experiments and nuclear detonations. A lot can be learned from astrophysical phenomena but discussions during the review made clear that weapon designers need more than can be obtained from astrophysics. LANL scientists have developed "shock-shear" experiments on ICF facilities that give combined plasma instability behavior in a controlled manner providing data of fundamental science value and specific value for particular nuclear weapon performance questions. Other experiments by labs and university groups explore colliding plasma and various plasma hydrodynamic phenomena at the ICF facilities – fundamental science of value to the weapon applications.

Test Readiness: experiments at ICF facilities are in concert with substantial advanced diagnostic development that is critical to ICF progress. Many of these diagnostic devices can be applied in any future nuclear device tests. And, these activities develop the people that would be required for obtaining data of scientific value from any nuclear tests. Along with subcritical tests in Nevada, these ICF activities make it possible to gain scientific value from testing of nuclear explosive devices should that ever become necessary in the future.

Improving the computer codes: during the review, a definition of Stockpile Stewardship was offered by Frank Graziani, "In the absence of testing, define and test computational procedure that is accurate enough to do the job". An important element in doing that and in having confidence that it is being done is HED/ICF. Both open and classified computer codes are applied to results of experiments in HED/ICF. Some codes are developed specifically for ICF ignition others deal with various HED experiments, and others deal with specifics of nuclear explosions. Code development is both learning to apply faster computational capability and including specific improved physics models and material data. More effort on codes is needed now and is made meaningful by having new data from HED/ICF. The more integrated planning of ICF should have value in planning of code development. This is another area where technically knowledgeable leadership at the labs is critical.

The challenge of ignition: while it is ICF yield more than ignition per se that is applied for weapon issues, the challenge of ignition stimulates the program and attracts the needed people. This is as true now as it has been over the first 20 years of Stockpile Stewardship. In addition yields in the 1-10MJ range would enable more advanced weapon applications of ICF and such yields are only achievable in the near term from ignition at the NIF. Ignition is itself weapon physics. Thus the drive for nuclear ignition and burn in the laboratory remains central to Stockpile Stewardship and critical to future value of ICF for Stewardship.

Weapon Effects: experiments at a number of NNSA's pulsed power facilities and at NIF provide x-rays for valuable weapon effects testing on electronics and even re-entry bodies. Valuable x-ray and gamma-ray tests continue using ICF facilities and expertise. The end of use of the Sandia Pulsed Reactor (SPR) meant that fewer means of testing effects of neutrons are available. Yield at NIF, and potentially at Z, can provide sufficient neutrons for some weapon effects tests. Invention of ways to use these neutrons in a more physics-based way, testing codes, would be valuable in weapon effects assessments.   Yields in the few MJ range would be quite useful, particularly if greater volume of neutron flux is also achieved. At LANL, Michael Bernardin commented that one of the most significant challenges that he sees coming is qualifying the function of our new and modified nuclear warheads in radiation environments; all rebuilt nuclear warheads will inevitably have changed electronic controls with potentially changed sensitivity to radiation. He highly values what could be achieved within ICF for this application. The convincing and more complete tests of weapon effects could be achieved if the full vision of 500 to 1000 MJ yield were achieved through ICF approaches. However, such high yields remain highly uncertain, requiring ignition success, significant new facilities and at least decades of progress and construction; this cannot be a sensible part of concrete Stewardship planning now. So the near term development of means to use yield at NIF and possibly Z should be the focus of effort for weapon effects interests.

Overall: *Technical invention of means to conduct experiments using ICF facilities has enabled a new range of weapons physics and effects and meaningful tests of advancing computer codes. These ICF-related technical advances are critical to science-based Stockpile Stewardship.*

The combination of management changes aligning ICF with Stewardship and technical advances allowing a wider range of experiments, especially including those with lower yields, leads to the overarching conclusion:

The ICF program is fulfilling its role as a primary instrument for science basis in Stockpile Stewardship.

Planning and development of manpower: In order to continue and strengthen the remarkable realignment of ICF with nuclear weapon interests there are important considerations for NNSA and the weapon laboratories. Nothing could be of greater value than technical progress toward ignition and yield above 1MJ. No great increase in funding for ICF is needed now but some funding adjustments are needed and future progress could change the funding needs. It is critical that NNSA have the intent to support HED/ICF at about the current level for a decade.

The integrated planning of the ICF community for the 3 approaches to inertial fusion burn (indirect laser drive, direct laser drive, and magnetic compression at Z) is critical. The facilities, particularly NIF, have become more efficient at providing shots and experiment time which enables more results and better planning. The laboratories need to lead in making the integrated planning more meaningful and this is a matter of some urgency.

As technical progress occurs in ICF critical new questions may arise such as investing in more laser energy at NIF or reconfiguring NIF for symmetric drive or investing in a more advanced Z facility. None of these new investments are justified today, but technical progress is difficult to predict and could happen rapidly or slowly. The ICF program is poised to pursue the needed progress intently and in a coordinated manner.

White papers about manpower were supplied to the group by the weapons labs, LLE and NRL. Each of these papers documents the trajectory of 10s of young researchers in recent years at each laboratory. These papers show that ICF/HED has been a clear attractor of talent for the Stewardship program. That remains true today and new and talented young researchers continue to be recruited to the labs from university-based HED programs today. There is not a manpower crisis today.

Some near-term funding adjustments are critical for long-term manpower development and supply. At LANL the integrated approach to design in ICF and nuclear weapons needs increased support. Work on topics like double shell ICF and Beryllium ablators for NIF capsules is languishing for lack of a few million dollars. This work is important to the use of ICF design for training and testing weapon designers. A modest increment (a few million $) in HED/ICF funding at LANL is important.

NRL has had a long-term role in the ICF program developing techniques and concepts for laser direct drive and supplying specific science expertise and data valuable to Stewardship. NRL needs modest funding increase (a few million $) to sustain this role. It is clear that NRL has played a useful role in the program.

At the universities the HED science that develops new talent for Stewardship faces financial hardship and change. The science is of high quality and attractive to capable young scientists and engineers as indicated by their participation and papers at technical conferences. Both the Office of Science and NNSA have interest in nurturing this area; the science and the interest of young scientists should influence program planning in both parts of DOE. Additional funding (about $20M increase) for basic high energy density science at universities could pay great dividends, both in new quality science relevant to materials behavior, fundamental plasma physics, astrophysics and nuclear science, and in retaining the interest of young scientists and stimulating intent of universities to continue their programs. The NNSA and the DOE Office of Science should strengthen their university programs in HED in a joint manner for the benefit of fundamental science and national security. The potential retirement of so many leading professors in the field makes this somewhat urgent.

The manpower finding is:

Current manpower, required to realize the value of ICF/HED for SSP and to develop future nuclear weapon designers, is healthy and strengthening, but near term funding increments for LANL and universities are required to sustain manpower supply.

Long-term value of ICF for SSP: Yogi Bera might have said, "planning is difficult when you don't know the answer". The question of "high yield" (500 to 1000 MJ in a laboratory) does not have an answer now. Even the question of ignition and modest gain at NIF (1 to 10 MJ yield) does not now have an answer. This makes the long-term answer to the question of value of ICF for the SSP unclear. In discussions at the laboratories, specific weapon-relevant experiments to use gains up to 10MJ were presented. There were no current clear examples of experiments or needs for yields in the range 10 to 100 MJ. Above 100 MJ yields (beyond the potential capability at any existing ICF facilities) there were clear concepts for weapons physics and weapon effects experiments that opened. These concepts are classified but clearly supported utility to SSP of high yield, which has been a long-term goal for ICF. High yield can be retained as a goal for ICF but it has no useful meaning if considerable progress toward ignition is not achieved. Within about 10 years the answers, that could give meaning to planning for high yield, may be obtained. In the intervening 10 years it is clear that ICF will have significant value for Stockpile Stewardship in training and testing designers and in providing physics insights (for example boost physics) and specific data (for example Plutonium response to high pressure) for Stewardship and other science and national security interests, and in challenging and attracting bright scientists and engineers. Strong support for ICF within Stewardship is easily given now, but the long-term is not clear.

ICF can lead in development of nuclear design expertise over the next decade, retaining ignition as an important goal and applying the experimental approaches and code development underway at the laboratories. The role of ICF for SSP, and other applications, beyond the next decade remains unclear with great potential dependent on progress within ICF.

Closing comment: The alignment of ICF with SSP has never been better in more than 50 years of ICF history. The diverse ICF program participants have never been more unified in their pursuit of the scientific goal of ICF ignition and burn in the laboratory. These changes are quite remarkable and are to the credit of a great number of people; technically astute leadership at the weapon laboratories and willing integration of talents is key. Management attention at the labs is critical to retaining this new environment and realizing its value. New technical inventions are allowing better use of ICF capabilities for weapon interests. However, new technical progress in the quest for ICF ignition and burn is also required to maintain this momentum. That progress is not assured and is difficult to predict. It will likely take as long as a decade to understand and assess this quest. Along the way checkpoints are needed; some type of technical review of ICF should occur every 2 or 3 years. Early progress, anytime during the decade, on the fusion front could call the question of choosing some form of upgrade or modification at NIF or Z in order to sustain progress; there is insufficient basis for such upgrades or modifications now. The SSP and the ICF subprogram are strong and dynamic; sustaining that is the challenge.

Reviewer Report: Jill Dahlburg

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28 August 2015

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Dear Dr. Frazier,

The 2015 Review of the ICF / HED (High Energy Density) Physics for Stockpile Stewardship first convened on 18-20 May 2015 at DOE NNSA Headquarters [HQ]. Group -2 of this Review, which addressed "Non-ignition HED and Long-term Goals," convened as part of this 18-20 May event, and also convened on two other occasions: 14-17 July 2015, for deep dives at the three NNSA Laboratories; and, for a deep dive, to discuss the Naval Research Laboratory [NRL] ICF program and the University of Rochester Laboratory for Laser Energetics [LLE] program, at DOE NNSA HQ on 23 July 2015. With the exception of the deep-dive day at Los Alamos National Lab [LANL] on 16 July 2015, and the deep dive day at Sandia National Lab [SNL] on 17 July 2015, I personally participated in all of the Group -2 convenings, i.e.: the 18-20 May event, the days 1 & 2 of the 14-17 July event, at Lawrence Livermore National Laboratory [LLNL]; and, the entirety of the day at NNSA HQ on 23 July. Note: For the NRL portion of the 23rd, because of my 1985-1999 affiliation with the NRL ICF program within NRL's Plasma Physics Division [PPD], and my ongoing employment at NRL as Superintendent of the NRL Space Science Division [SSD], which is a sister division to the PPD and in another directorate at NRL, I offered to recuse myself. This offer was deemed unnecessary by the Group -2 members and by the DOE personnel present, so I participated in this portion of 23 July's event also.

This letter summarizes my independent review, from a Group-2 perspective. At the start of the review, on 18 May, Keith Le Chien, Director, ICF, NNSA, introduced the review topics and purpose. This review was a promise made by NNSA to Congress, to conduct a comprehensive review in 2015 on the "progress towards ignition" and the broader HED physics program that ICF facilities enable within the NNSA Stockpile Stewardship Program [SSP]. Specific Group-2 duties were to: (I) assess the alignment of the ICF / HED program with SSP and the broader Nuclear Weapons Program [NWP], with emphasis on workforce development and program management; (II) assess the long-term direction planning associated with the tri-Lab Directors' Letter; and, (III) assess the need for "high-yield" for the SSP. My assessments, following, include four (4) findings and also four (4) actionable recommendations.

(I) Assessment of the alignment of the ICF / HED program with SSP and the broader NWP in the near, medium, and long term, with emphasis on workforce development and program management.

During the Cold War era, the U.S. effectively provided a validated NWP strategic deterrent, designing, building, testing, and deploying numerous nuclear warheads of various designs. This deterrent was in the product -- the stockpile -- and its delivery vehicles. And, the deterrent was also in the development and production Complex which encompassed: the staff; the RDT&E programs; and, the facilities, including the Labs and the factories.

The moratorium of nuclear testing that began in 1992 ushered in a new era for the deterrent. The product passed into a mode primarily of maintenance, which had immense implications for the development and production complex. Now nearly a quarter of a century into the moratorium, and nearly three quarters of a century from when the deterrent was first realized, no active researcher directly remembers a time before the advent of the deterrent, and few have experience from the active phase of the deterrent -- the Cold War era.

The primary function of the Complex is refurbishment of existing warheads and associated, with objective to guarantee deterrent effectiveness in the event that it is ever needed.

Maintaining a state of peak but static readiness is very difficult, in part because this goal is seen as achieved and abiding by all constituents of the Complex. The central core of the Complex, around which all else could be rebuilt should that need ever arise, is the knowledgeable and experienced staff. In the context of a fixed definition of readiness, static can become passive, leading to analysts and analyzers where -- for a reliable deterrent into its next century -- designers and experimentalists are needed instead. It is in this area that the ICF / HED program can and should provide significant mitigation to a central risk in readiness, which is that our nuclear weapons will fail to perform as expected, due to staff misapprehensions arising from complacency as result of insufficient experience with empirical, real-world effects, AKA the unpredictability of "Mother Nature."

(F-1) Finding: The designers at the NNSA NWP Labs have demonstrated that lack of sufficient experimental, empirical information can lead to unexpected and profound errors, as evidenced by the lack of success of the National Ignition Campaign [NIC] to achieve ignition.

(R-1) Recommendation: The NNSA should ensure that the design and experimentation skills of the NWP staff are regularly exercised, including by innovative exploitation of ICF and HED facilities available to the staff. For example: high-Z metal targets with deuterium-tritium gas fill may offer promising routes to laboratory ignition and burn, including on the National Ignition Facility [NIF], relative to the conventional plastic capsules of the NIC and immediate follows-on. Additions of imposed, compressed magnetic fields could enhance the ignition margin, and should also be studied. These designs would challenge staff in key areas of modeling and simulation, materials and target fabrication, and diagnostics, and should be experimentally explored in the near- and mid-term, and if promising also in the far-term.

In addition to stagnation is the danger of staff insularity. As early as the KD-0 decision point for NIF, circa 1992-1993, informed ICF researchers external to the tri-Lab (LLNL, LANL, SNL) complex recognized, promulgated, and documented all of the major hydro failure modes of what became the NIC baseline capsule [REF: "Shell Implosion Modeling (U)," Defense Research Review (1994), Jill Dahlburg,* Stephen Bodner,* John Gardner,* Andrew Schmitt.* *(NRL)].

This (REF) work was presented to the DOE NNSA Inertial Confinement Fusion Advisory Committee [ICFAC] during the KD-0 decision time period. Had this information been properly evaluated by the tri-Lab ICF staff, at the time, many years of more effective research could possibly have been achieved in the intervening nearly quarter century.

During the years from 1992 to the present, both of the non tri-Lab ICF organizations -- NRL and LLE -- have contributed significantly to the Complex. These include myriad successful experimental campaigns on Omega and on Nike, new concepts and capabilities both realized and proposed, and key staff transitions to the tri-Lab cadre, among them Dr. Charles Verdon from LLE to LLNL and Dr. David Meyerhofer from LLE to LANL. In addition, the contractor ICF organizations General Atomics and Schaffer Corporation have provided tremendous target fabrication and related ICF technology support as well as beneficial staff transitions.

(F-2) Finding: The tri-Lab NWP has benefited by inclusion in the ICF / HED cadre of organizations outside of the tri-Lab institutions, in substantive and documented ways.

(R-2) Recommendation: The NNSA should reliably cultivate appropriately broad partnerships with the ICF / HED community beyond the tri-Lab cadre, including with Laboratories in other Agencies, Universities, and private Corporations. For example: the NNSA could define and promulgate an ICF / HED experimental challenge intended for participation by the tri-Labs and also by other ICF/ HED organizations. The challenge should be scoped so that it could be executed on these (and possibly also other) facilities: Z at SNL; Trident at LANL; NIF at LLNL; Nike at NRL; and Omega & Omega-EP at LLE.

(II) Assessment of long-term direction planning associated with the tri-Lab Directors' Letter.

From a Group-2 perspective, the Letter is a good start. The above-discussed issues of staff possible stagnation and insularity are also the purview of the tri-Lab NWP management as represented by this Letter, and their subordinates. It is a sadly often-recognized fact that truly good management is not appreciated until it is gone. The NNSA NWP Complex at present is extremely fortunate with excellent tri-Lab Directors and also with outstanding -- by any measure -- NWP / ICF/HED management: at SNL (Dr. Keith Matzen); world-leading overall management at LANL (Dr. Charles McMillan); and, perceptive and thoughtful, outstanding NWP management at LLNL (Dr. Charles Verdon). In particular, Dr. Verdon's recent reorganization of A-, B- and ICF areas of LLNL is to be applauded.

(F-3) Finding: Suitable management is in place at all three of the NNSA Labs to enable next beneficial steps for the advancement of the NWP cadre, as intimated by the tri-Lab Directors' Letter.

(R-3) Recommendation: NNSA should formally recognize this NWP / ICF/HED management (McMillan, Verdon, and Matzen) excellence, and request this management to build from the posture described in the Letter, and work together to develop a roadmap for NWP designers' and experimentalists' career development, across the tri-Lab Complex. The roadmap: should included detailed description of a tri-Lab vision that describes what the community of designers and experimentalists should look like at both 5-year and 15-year points in the future; should delineate the necessary attributes of this designers and experimentalists community; and, should include definitive discussion of how ICF/HED experimental research will help to cultivate and hone these attributes, at all three Labs.

Consequence of no near-term action: The time during which all three of these personally knowledgeable and managerially outstanding leaders currently available to the NNSA will pass, and a rare window of opportunity to utilize their combined expertise for tri-Lab joint workforce advancement will disappear.

(III) Assessment of the need for "high-yield" for the SSP.

The near-total lack of ability to test the reliability of a nuclear warhead's components in a relevant environment presents a risk that cannot be understated. No current capability enables broad assurance of refurbished components in already-known threat environments, and as new threats evolve, the risks increase. High yield under controlled conditions would enable retirement of many of the known and envisioned risks. This is much needed.

Further, designing for high-yield exercises an NWP designer's innovation skills in ways that few other non-proscribed technical activities achieve.

(F-4) Finding: High yield is needed for the SSP, technically, and it is also beneficial for workforce development across the NNSA NWP Complex.

(R-4) Recommendation: The NNSA should continue to encourage development of high yield designs. In consideration of the fact that ignition is not yet achieved on NIF, and also that high yield would not be supportable by the NIF Facility, these designs should be encouraged to be as innovative as reasonable, so as to stretch the design capabilities across equation of state, radiation transport, hydrodynamics and magnetohydrodynamics, thermal conduction, and turbulence modeling. At the same time, the designs should be arguably feasible to fabricate. For more promising designs that may require novel geometric configurations or properties (e.g. metal foam), a modest amount of target fabrication collaborative research could be very beneficial, to keep the modeled design grounded in reality.

I welcome requests from your office to elaborate or clarify.

Sincerely,

Jill Dahlburg

Jill Dahlburg
Naval Research Laboratory

Reviewer Report: John R. Harvey

Introduction

The primary focus for NNSA's HED facilities (NIF, Omega, Z) must be to advance nuclear weapons stockpile stewardship.[1] In this regard, their role is two-fold:

These facilities are unique in enabling experiments to test the validity of (and thereby provide means to improve) existing nuclear weapons codes in the temperature, pressure and density regimes in which nuclear weapons operate.

Probably more so than for other modern experimental facilities in NNSA's weapons complex, they provide primary means to replace nuclear testing as a training tool for a new generation of nuclear weapons designers and engineers.

Both objectives are essential for the future success of stockpile stewardship. How well these three facilities, and the scientists and engineers who operate them and carry out complex experiments, meet these objectives is the focus of more detailed remarks that follow.

I. Alignment of ICF/HED program with SSP and broader weapons program

NNSA's HED facilities have made essential contributions to the nuclear weapons program over the past five years. Non-ignition HED experiments have made important contributions to our understanding of how nuclear weapons operate in the areas of energy balance (a major breakthrough), boost, secondary performance, and warhead radiation output and associated weapons effects (relevant to qualification of non-nuclear components of the W76 and B61-12 LEPs). Achieving ignition has important implications for weapons work as well as for providing long-term energy solutions for our nation. Very importantly, ignition experiments confirm that our computer codes do not model with sufficient precision ICF capsule implosions designed to achieve the temperature, pressure and density regimes for sustained thermonuclear burn.[2] That said, the conclusion of the ignition campaign has permitted a desired increased focus on important weapons physics that can be addressed in non-ignition experiments. Other key findings regarding HED workforce development and overall program management follow.

Workforce Development – *It's the people, stupid!*

For two decades officials have wrestled with the problem of how best to ensure that the next generation of nuclear weapons designers and engineers is ready to take over from those who honed their skills during nearly five decades of nuclear testing. As time passes, loss of knowledge resulting from the departure of the older generation, and the need to transfer critical skills based on that knowledge, heighten the urgency of this effort. Lab directors have expressed concerns about the "shifting to the right" of the peak of the age distribution of working-level weapons scientists and engineers. This relates not just to the ability to develop a modern warhead or field a new or different military capability if required in the future, but to the judgment to ensure that the existing stockpile remains safe and reliable. Bringing highly-capable young scientists and engineers to the laboratories has been and will continue to be driven by access to world-class scientific facilities producing path-breaking research, and the ability to work on complex technical problems involving the security of the nation whose importance is communicated clearly by its leaders in both words and actions.

Importance of HED Experiments with Advanced Diagnostics

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[1] These notes reflect my perspective on the state of NNSA's ICF/HED physics program after participating in a "deep dive" review organized by NNSA's NA-10 organization. Although a physicist, I have not been immersed in this program. My remarks however reflect 35 years experience working nuclear weapons and national security issues, first at Lawrence Livermore National Laboratory, then at Stanford University's Center for International Security and Arms Control and in senior government positions in the Departments of Defense (twice) and Energy. I carried out this work for NNSA under a consulting agreement already in place with the Los Alamos National Laboratory. As stated, these notes reflect my own views and are not necessarily those of Los Alamos or the NNSA.

[2] Indeed, the high convergence implosions characteristic of HED capsule experiments stress the weapons codes in ways that modeling a generic nuclear explosion do not.

The challenge of training weapons designers and engineers is evolving due to the absence of nuclear testing and availability of new, extraordinarily powerful computing capabilities. More so than their predecessors, young designers rely heavily on computer simulation, modeling and calculations, tending towards overconfidence in the quality of the weapon physics embedded in the codes. One senior designer noted that “the codes always lie” and the job of the designer is figure out where they can be wrong and when.

Predicting the results of an experiment ahead of time, whether a nuclear test in Nevada or an HED physics experiment, has been and remains an important learning experience for a young designer. Finding out why the codes do not work in certain instances creates knowledge that builds judgment. In addition to greatly advancing our understanding of how nuclear weapons work, the NIF, Omega and Z facilities, coupled with advanced experimental diagnostics capabilities, provide opportunities for young weapons designers to build skills and judgment through the understanding gained when the results of their calculations are—or are not—confirmed by Mother Nature.

To achieve these training objectives require some adjustments in ongoing programs. Funding shortfalls and other priorities prevent young scientists from fully exploiting these facilities. For example, because LANL does not have a resident large-scale HED facility, LANL secondary designers, in order to access temperature and pressure regimes unique to secondaries, must conduct experiments at a remote facility. All three facilities are open to outside users, but LANL designers tend to rely less on HED experiments in honing skills than do those at LLNL. Indeed, LANL’s HED program is relatively modest by comparison.[3] LANL’s leadership recognizes the problem and is working to address it, among other things, by shifting the culture at the lab to reinforce the idea that HED physics is an integral part of stockpile stewardship. LANL’s HED program, unlike LLNL’s which has a strong ignition component, is totally focused on non-ignition weapons physics. Significant work, and training, can therefore be carried out at Omega where it is cheaper to operate. Still, with the conclusion of the ignition campaign, more high-value NIF weapons shots can be allocated to LANL, which is steadily ramping up its work at NIF. To fully exploit opportunities at NIF, however, will require a corresponding increase in associated LANL funding.

Program Management

Program management should seek to promote tight coupling between HED experimental work and the needs of nuclear weapons design and modeling efforts in support of upcoming LEPs. This is a work in progress with some noteworthy disconnects. For example, there are concerns that the scientific knowledge gained from HED experiments is not being exploited rapidly enough in updating nuclear weapons design codes. Indeed, in one case at least, this had led to different codes being used for ICF capsules and for weapons. There may be good reasons for two sets of codes (e.g., the high convergence implosions characteristic of ICF capsules referred to earlier), or for delay in updating weapons codes (e.g., concerns about data quality), but ideally it seems desirable to strive to use the same codes to model the same physics.

In the past year or so, to promote greater linkages between weapons and HED physics work, LLNL has integrated primary and secondary design teams, and the ICF capsule designers, into one weapons design division. Notwithstanding a speed bump or two in the ensuing culture clash, this is the right approach. LANL, which had already integrated primary and secondary design teams and the computational physics folks into one division, should explore the benefits of further integration with the HED physics team (aka Experimental Plasma Physics division).

Recommendations

- More fully utilize existing HED facilities. Tight budgets are a fact of life; we must get “more bang for the experimental buck” by operating more efficiently and by managing safety risks rather than seeking, fruitlessly and at high cost, to eliminate them. Such efficiencies can augment other resources to bolster LANL’s HED physics program.

[3] Only 10% of LANL’s secondary designers are actively involved in HED experiments. LANL’s share of the total FY15 ICF/HED budget for experimental work related to stockpile support and ignition is also about 10%. An LLNL white paper notes that training in HED science is essential for all of its designers and suggests that a much higher fraction are involved in associated experiments.

- Provide young LANL designers with stronger incentives to carry out experiments at NIF, Omega and Z facilities in their training and later design work. To promote this, strive to make NIF more “user friendly” to outside users. Among other things, clarify the process through which work funded by LDRD can compete with other users for NIF shots.
- Continue to challenge young weapons scientists at all the labs to brief and document their predictions on the results of experiments before they are carried out. This process provides opportunities to fail and, thus, is a vehicle for building judgment.
- All labs should strive to strengthen integration of HED programs and weapons design teams or, in the case of Sandia, with those working nuclear effects. LANL should explore integrating its HED physics team with its single nuclear design division.
- Implement timely updates to weapons codes based on validated physics results from relevant HED experiments. As a goal, each nuclear design lab should maintain a single set of codes (independently developed to facilitate peer review assessments of the other’s work) to model both nuclear explosions and HED experiments.

II. Planning associated with Tri-Lab Directors’ letter

The January 2015 letter sent to NNSA Administrator Frank Klotz by the three weapons lab directors highlights the importance of the ICF/HED program for stockpile stewardship.[4] It states:

The overwhelming majority of the yield of the Nation’s nuclear weapons is generated when the conditions within the nuclear explosive package are in the high energy density (HED) state. This requires that proficiency in HED science remains a core technical competency for the Nation’s Stockpile Stewardship Program (SSP) for the foreseeable future.

It calls for a review of the overall effort on ignition:

. . . the HED program in the United States is at an important juncture where we must assess the appropriate balance between pursuit of ignition and the other uses of HED research in stewardship.

. . . the pursuit of fusion yield in the laboratory, is critical for the long term health of the stockpile stewardship program.

It identifies specific goals for the ICF and HED scientific programs:

In the absence of new nuclear tests and the attrition of nuclear test experience, looking forward the nuclear weapons laboratories will need the ability to (1) test nuclear designers in high energy density (HED) experimental design, (2) access material pressure and density regimes that are presently inaccessible to other experimental techniques, (3) generate and utilize thermonuclear burning plasmas, (4) develop commensurate high-fidelity diagnostics and experimental platforms that help to assure our weapons are safe, secure, and effective, and ultimately, (5) create and apply multi-mega joule fusion yields to enable enduring stockpile stewardship.

Finally, the letter calls for “regular meetings in 2015 to ensure progress towards this integrated and coordinated HED effort . . .”

Ignition

With the conclusion, in 2012, of the scientific campaign to produce ignition, the trend has been to increase the proportion of NIF shots devoted to non-ignition weapons physics experiments. This is an important development because much can be learned about weapons physics at NIF absent ignition. It also provides an opportunity to increase the number of shots that can be allocated to training young designers. At the same time, pursuit of ignition remains important to stockpile stewardship for several reasons:

- If we don’t achieve ignition conditions in capsule implosions, we need to understand why so we can adjust the weapons codes accordingly.
- If ignition is achieved, our understanding of weapons physics (e.g. secondary performance) will be further advanced by the study of how variations in capsule design and initial drive conditions affect performance. Hardening/vulnerability

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[4] Letter to Frank Klotz, NNSA Administrator, from Charles McMillan (LANL), William Goldstein (LLNL), and Paul Hommert (Sandia), 20 January 2015.

assessments of military hardware will benefit from the intense neutron environments produced from burning DT plasmas.
- Achieving ignition in the laboratory—arguably one of the preeminent scientific challenges of our time—would represent an extraordinary demonstration of U.S. excellence in science and technology related to nuclear weapons. As such, it would augment an important component of our overall capabilities to assure allies and deter potential adversaries.
- Some of the excellent young scientists and engineers who are drawn to state-of-the-art HED facilities to work on ignition will at some point in their careers move to nuclear weapons design work.

All this argues for continued strong effort on ignition but at what level? In recent years, the number of NIF shots devoted to ignition on an annual basis dropped from about 90% to about 50% of total shots. Weapons-related non-ignition shots have taken up the slack. This overall trend is desirable and, indeed, somewhat overdue. The details of shot allocation, however, can best be addressed by the technical community with clear guidance from NNSA that the needs and priorities of stockpile stewardship are to be the principal driver in shot allocation.

Long-Term Direction

NIF weapons physics experiments, supplemented with related work carried out at Omega and Z, have contributed to resolving some of the scientific puzzles (e.g. energy balance) discovered, but not explained, during the days of underground nuclear testing. There is reasonably wide agreement within the weapons community that the next “grand challenge” for HED physics research, and one that does not require ignition to achieve significantly increased understanding, is in boost physics. Remaining challenges involve more precise understanding of plutonium EOS, secondary performance and weapons output. These can be addressed in parallel with lower priority.

Although there has been significant progress in Sandia’s work at Z, another key focus for future work must be on source development for exposing military hardware to intense X-ray (both hot and cold) and neutron environments.

The Laboratory Directors’ letter rightfully calls attention to “avoiding technological surprise” as a key mission driver for NNSA. Indeed, we must ensure that the U.S. nuclear weapons enterprise, and the security it provides, is resilient to unforeseen adverse contingencies whether geopolitical or technical in nature. In our “deep dive” meetings at the laboratories, we (certainly I) did not hear a clear articulation of how a balanced HED program contributes specifically to avoiding surprise. Of course, maintaining the capabilities of scientific and technical personnel, the experimental tools, and manufacturing infrastructure—if directed by the President—to develop and field modern warheads or warheads with new or different military capabilities is an important component of hedging against surprise. But it would benefit this program, by broadening support for it in the DoD and Congress, to develop the story for how the results from HED experiments specifically contributes to avoiding surprise.

Establish Coordinated National Roadmap/Decisions Making/Metrics for Progress

Finally, the letter calls for further meetings in 2015 to scope out a balanced national HED program. When several of us asked for what had been done on this (i.e. as of July 2015), the answer in essence was “not much so far.” A ramp up in this activity seems needed.

Important Role for Omega and NRL

Work at the University of Rochester (Omega) and the NRL provide important contributions to the nation’s ICF/HED program. These facilities are smaller and cheaper to operate and therefore provide efficient means to develop experimental platforms, associated diagnostics, and target fabrication to support the work of larger facilities. Training benefits because graduate students at the U of R have opportunities to take a greater role than they could at NIF, for example, in developing and fielding experiments. Both labs are involved with innovative approaches (e.g., direct drive, hybrid drive) that could hedge against failure of other approaches. The NRL, as a source of expertise within the DoD, can help convey the importance of HED physics for stockpile stewardship to other parts of DoD—often a harder sell for the DOE folks. Finally, these two centers of HED expertise provide an important means for independent peer review.

Recommendations:

- The pursuit of ignition is important for weapons. We must therefore (as I sometimes do!) avoid characterizing HED experiments as either "ignition" or "weapons-related non-ignition." To best advance weapons program interests, the focus of future ignition work (as it seemingly was not during the ignition campaign) must be to understand why the fundamental physics embedded in the codes has so far not aligned with Mother Nature.
- A "national HED effort" must focus on the needs and priorities of stockpile stewardship. It can best be achieved with an inclusive process in which weapons program leaders from all three NNSA facilities and the NRL create a program that leverages each other's work and identifies a proposed associated resource allocation within an overall total budget level provided by NNSA.
- In line with the Lab Directors' letter, ramp up activities/meetings in the second half of 2015 in advancing a national HED program.
- Articulate (in a monograph. or equivalent. written for the layman) how a balanced HED program and associated experiments can provide a hedge against technological and geopolitical surprise.
- In advancing a national program at the NNSA facilities and NRL, invest in a broad range of ideas involving both direct and indirect drive as well as innovative capsule designs (e.g., metal capsules, double-shell designs, etc.)

III. High Yield (multi-Mega joule) for Stockpile Stewardship

Previous discussion has addressed the benefits for stockpile stewardship of achieving ignition in the laboratory (e.g., gains in the range of 1-10) and the relative balance between ignition and non-ignition weapons-related experiments. The question here is the relative degree to which ignition experiments and associated platforms that achieve very high gain (hundreds of MJ) are essential, desirable but not essential, or not that important at all for stewardship. Of course, achieving ignition and burn at these levels has important implications for the nation's long-term energy future, for example, or in the national security arena in regard understanding the potential for advanced new types of nuclear weapons. In regard to today's stockpile stewardship needs, the principal benefit would seem to be in providing an intense source of radiation with an appropriate spectrum to support precise assessments of the hardness and vulnerability of military hardware as well as to validate nuclear effects codes. It is unclear to me the degree to which very high-gain experiments would provide a more complete understanding of the grand challenges of boost physics and secondary performance. In any case, once ignition is achieved work should continue to reach high gain. Increased funding for such experiments, outside the weapons program, might well follow. Until ignition is achieved, however, there does not seem to be much justification for allocating substantial funding to plan for such experiments.

Recommendation: Maintain planning for high-gain experiments, and associated platforms, at a relatively modest level pending achievement of ignition.

Reviewer Report: Jeffrey P. Quintenz

The primary charge is an assessment of the alignment of the ICF/HED program with stockpile stewardship program and the broader nuclear weapons program. Assess the contribution to stockpile stewardship in the non-ignition HED sciences in the near, medium, and long term.

Given that the overwhelming majority of the yield of a nuclear weapon is generated when the conditions within the nuclear explosive package are in the High Energy Density (HED) state[5], HED science and understanding are unquestionably critical elements of the nuclear weapons program. From its inception, the Inertial Confinement Fusion (ICF) program has advanced our understanding of nuclear weapons; indirectly at first and more directly in recent years. ICF research places significant challenges to our understanding of fundamental physics of relevance to a nuclear weapon. For example, data obtained on ICF facilities have measurably improved tables for opacity and equation of state in relevant materials and conditions. In my opinion, the alignment of the ICF program with the broader weapons program has always been there but has markedly improved since the completion of the National Ignition Campaign (NIC). The balance between dedicated experiments primarily exploring specific issues related to ignition and experiments more broadly addressing weapons physics and engineering has shifted with roughly 50% weapon centric at the National Ignition Facility (NIF) and a greater percentage at Z. In my opinion this is about the right mix, though as the need for higher yield becomes more apparent, I believe that more effort should be placed on exploring the limits of fusion yield using magnetic drive and below I have included a recommendation to that effect. The Z effort on ignition and yield seems suboptimal though the split in utilization (ignition/ICF vs. direct weapons work) is appropriate. More ICF effort on Z would likely require more operational funding or offloading some materials work as discussed later in this report.

In the past, ICF facilities, and data obtained thereon, have contributed to a better understanding of nuclear weapons. Specific examples include the energy balance question and properties of relevant materials at extreme conditions of temperature, density, and pressure. In the near term, data from these experiments will be used to further improve understanding of opacity, equation of state, various coupled radiation/hydrodynamic instabilities, and other properties of matter under conditions relevant to nuclear weapon operation. These data and improved understanding are being incorporated in numerical simulation codes and provide a basis for improved confidence in their predictions. In the longer term, ICF should produce a burning plasma allowing for the first time the study of these conditions in a laboratory environment with precision diagnostics and providing further insight into boost physics. A high yield facility would allow new capability that has long been recognized as opening unprecedented opportunity to study aspects of weapon physics not available short of resumption of underground nuclear testing. More about this history and the requirements for high yield are referenced in the classified appendix to this report.

I. *In their January 20, 2015 letter, the laboratory director's described several specific multi-decade goals for the ICF/HED program within the context of the broader stockpile stewardship program. Assess both the scientific and programmatic progress – and plans – in today's ICF program to meet those goals.*

The laboratory directors expressed their strong support for an enduring program to maintain proficiency in HED science, "a core technical competency for the Nation's Stockpile Stewardship Program (SSP)". The letter is short on specifics regarding multi-decade goals but in broad brush terms they expressed the needed ability to test nuclear designers in HED experimental design, access material pressure and density regimes that are presently inaccessible to other experimental techniques, generate and utilize thermonuclear burning plasmas, develop diagnostics and platforms, and create and apply multi-megajoule fusion yields. Progress has been made in each of these areas but significant challenges remain ahead. For example, we have yet to achieve the alpha-heating goal on the NIF (though we are very close to the 10^{16} neutron goal). A careful facility and laboratory inclusive study identified several transformational diagnostics that were needed to continue to advance our understanding of the HED environments produced on these facilities. We are short of ignition, the predecessor to robust burning plasma platform, and no current facility can credibly be predicted to reach the multi-megajoule yields as indicated in the director's letter.

The directors expressed their "view that the U.S. must continue to strive to be the first nation to demonstrate ignition and high yield in the laboratory" to both support the SSP "but also to send a strong signal to others regarding our Nation's scientific and technical capabilities". In my opinion, the U.S. remains far ahead of the rest of the world in creating and utilizing HED platforms. It is unlikely that any nation will soon duplicate the capabilities of NIF or Z. Even the French, with their significant investment in Laser Mègajoule (LMJ), are several years away from full design operation. The Chinese and Russians have announced plans to build

[5] Laboratory Director's Letter dated January 20, 2015

substantial HED facilities but, in particular for the Russians, these plans have been around for more than a decade with little evident progress. The Chinese are reported to be building a Z-like machine based largely upon published Z material. I am not aware of any recent results from the Chinese but close attention to their progress would be prudent as I expect they could move faster than the Russians in this area even though Russian scientists have been quite innovative in fast pulsed power research and design.

In commenting on the progress toward the goals laid out in the director's letter I would first acknowledge the work being done by Group 1 and Group 3. Progress made toward ignition and go forward plans in continuing to "strive to be the first nation to demonstrate ignition and high yield in the laboratory" are the purview of Group 1. Group 3 will likely include some discussion of present diagnostic capabilities and needed improvements in that area. For HED science as required by and applied to the SSP more directly, I suggest that progress has been substantial and has recently accelerated. Sandia has been increasing the utilization of Z fraction from ICF to more directly relevant weapons work for many years to good effect. After the end of the National Ignition Campaign (NIC), the effort on NIF has also shifted with approximately 50% of the experimental time devoted to HED Council defined experiments. The HED Council has been a welcome influence on the direction of research on the ICF/HED facilities. The Council has expanded participation in experiment planning and prioritization and has made a concerted effort to direct experiments to the most appropriate facilities without the past institutional biases.

Funding for the ICF program has come almost entirely from the weapons program. From its inception, the quest for ignition and high yield has been justified by applications to weapons science. This justification has been repeatedly documented in classified and unclassified venues. ICF presents a significant scientific challenge with many aspects directly related to understanding needed by weapons designers. The significant investments made to construct the world leading HED facilities are paying dividends on multiple fronts. Conditions of pressure, temperature and density heretofore unavailable in the laboratory are now routinely produced and exquisitely diagnosed. New data on opacity, EOS, radiation flow, etc. are being used to challenge our understanding and weapons code predictions. There are many examples that can be found in the references at the end of this report. In large part due to the available facilities and challenging work, the ICF program has been a main attractor to the laboratories of new stewards. This program provides the weapon stewards with new tools to test their ideas. The laboratories did express concern that the pipeline of university students was at risk due to the loss, or potential loss, of several professors who have a good track record of supplying students with the desired background for work on nuclear weapons. They also stated that with the smaller pool of graduating students, there is at times unhealthy competition for these students between the laboratories. NNSA should address this concern soon given the time required to educate a future steward [see recommendation below].

In short, many of the promises of a sustained investment in ICF/HED have already been realized even without the achievement of ignition and gain.

II. *The formal title of the ICF Program is "Inertial Confinement Fusion and High Yield." "High-yield" at laboratory scale is more than a decade away, but having this ultimate goal shapes program decisions many years in advance of their perceived need. Assess the need of "high-yield" capabilities at laboratory scale as a long term goal of the ICF/HED program given evolving nuclear threats, and the overarching boundary condition of no additional nuclear testing*.

High yield has been an ICF objective from the very beginning of the program. Even while underground nuclear testing was ongoing, nuclear explosive package and non-nuclear weapon component designers alike recognized the utility of a laboratory facility that could produce significant fusion yield. "High" is a relative term and there are likely differences of opinion about what constitutes high yield. A report written in the early 1990s and reinforced during this review suggested that several 10s of Megajoule fusion yield were required for some interesting applications[6]. Designers have proposed experiments that would be enabled at successive increases in yield. All proposed experiments would provide data that could be used to further understanding, improve codes, and test designer's skills and judgments. The two NEP design laboratories each have listed underground nuclear tests (UGT) they would propose should that testing be allowed. It is highly unlikely that any UGT will be forthcoming, so the question to be answered is how is the current HED/ICF program effort being directed to improve understanding, reduce uncertainty, and increase confidence in our deterrent? The program of work as defined by the HED Council and coordinated with the ICF program leadership is, in my opinion, doing a very good job of prioritizing the work to be done. The Council helps to identify and prioritize research needed to support the NSE objectives and in large part is the source of input to define the SSMP goals and milestones. There is much work that can be done on existing facilities and that work could realistically be expected to last for many years. Designers are clever and

[6] See classified appendix to this report

will propose many different and important experiments to be done on these facilities. The HED Council has a formal process to sort through the proposals and define the plan to execute. I am encouraged that the HED Council and the newly formed ICF Council are coordinating efforts on these shared facilities. By identifying the opportunities for improved understanding, determining priorities and appropriate timeframes, and setting realistic goals, the Council adds significant value to NNSA program management. When asked what yield is needed, the designers will almost always answer that nothing they don't already have access to is needed to assess the current stockpile. They will also say, however, that more yield is better.

It is important to recognize that even after ignition is achieved, yield of a few megajoules will be needed to provide the robust ignition platform required to perform desired experiments in this low yield regime. Robust in this context implies yield sufficiently beyond the "ignition cliff" to distinguish between the physics of interest and the physics of ignition. Near the cliff, capsule performance can vary widely as small changes or accumulated errors can dud an implosion or confuse the desired physics data. Robust also implies reproducibility. It is likely that more yield will also require more margin. More margin will likely result in more predictable yields and better reproducibility.

I. Alignment of ICF/HED program with SSP and broader weapons program

Workforce Development

Findings: The laboratories expressed concern that the pipeline of university students was at risk due to the loss, or potential loss, of several professors who have a good track record of supplying students with the desired background for work on nuclear weapons. They also stated that with the smaller pool of graduating students there is, at times, unhealthy competition for these students between the laboratories.

Comments: Given the time required to educate a future steward (7-10 years post graduation), there is the potential for an unfavorable change in demographics at the laboratories if not corrected soon. In the past, due to the vagaries of hiring freezes and bad publicity, reductions in the number of staff with certain years of experience have been created and these "holes" in the experience distribution tend to propagate over the course of time. While budgets and bad publicity can't be reliability predicted, it is certain that a lack of qualified students in relevant fields will create future "holes" if not addressed.

Recommendations: NNSA should address this concern soon by establishing metrics to quantify and track the problem and by incentivizing new faculty and students to enter the relevant fields of study. This may require rebalancing funding within HED and ICF programs. bv

Findings: The challenge presented by ICF attracts and helps to retain highly competent staff who can and often do contribute directly to stockpile stewardship. Potential loss of future talent is a threat to the entire NSE and demands action.

Recommendations: NNSA, not solely NA-11, should identify additional funds for university research and target those funds toward developing new professors and their students. Funding decisions must be based upon data. NA-11 should lead in the development and tracking of these metrics.

Findings: The HED facilities provided by the ICF program are critical instruments used in the training and testing of new stewards in the SSP. All three NNSA laboratories recognized the importance of ICF/HED experiments in training new staff and keeping more mature staff current.

Comments: LLNL and SNL were more explicit in their statements about training using ICF facilities but LANL talked to the value of testing designer judgment using these complex experiments and having to "think in an integrated way" with emphasis on interfaces throughout the process. In May, the LANL representative was asked how much HED instruction was included in the TITANS program and he answered "probably not enough". When pressed later during the LANL deep dive session, the answer was refined to state that there is no specific HED module in the TITANS program and, given that it is more aimed at broader issues, it would not be appropriate. For example, there is no module dedicated to DARHT experimental capability either.

Recommendations: Since the DPAC is focusing its first study on Workforce and several other recent studies external to NNSA are making recommendations in this area, I would only add that the ICF/HED programs should take seriously the recommendations from these studies. NNSA should compile a list of recommendations from the various recent studies, evaluate its performance, and implement changes as warranted. NA-112 and NA-113 could jointly develop a report that highlights the workforce development attributes of ICF/HED. This report could include a description of the various university and mentorship programs sponsored in whole or in part by NA-11.

Findings: All three NNSA laboratories commented upon emerging threats and the need to be responsive.

Comments: No detail was presented but the need for highly trained, competent and experienced scientists and engineers to quickly address an emergent threat cannot be overstated. HED/ICF is, and will continue to be, a key asset in this area.

Recommendations: It is recognized by most that attracting and retaining skilled talent in the weapons program is an imperative to our National deterrent. Staff within the HED/ICF programs are well equipped to address many of the questions that arise as new technologies are developed or new threats arise. These programs should retain the flexibility to assign personnel to joint projects with other NNSA organizations as the need arises. The flexibility could include rebalancing program budgets and schedules. These efforts to prevent technological surprise and address emerging threats have proven to offer excellent straining opportunities for weapon stewards

Findings: Concern was expressed that we are turning engineers/scientists into analysts.

Comments: Analyzing past UGT data will eventually run its course. Engineers and scientists need the challenge of "white sheet" design to practice innovation and to exercise the myriad of interfaces between design, prediction, manufacture, execution, analysis,… The inherent complexity of experiments conducted on ICF facilities requires exercising many aspects of "white sheet" design. Hypotheses are developed, pre-shot simulations performed, targets manufactured, diagnostics employed, and data analyzed to support or refute the hypotheses. With the exception of hydrodynamic experiments, these HED/ICF experiments may be the closest thing we have available to exercising and testing designer Skills, Knowledge, and Abilities (SKA).

Recommendations: The ICF/HED programs use of the ICF facilities is, for the most part, appropriately balanced between pursuits of ignition/fusion and non-fusion applications. The main exception in my opinion is that more fusion directed work should be done on Z. A suggestion about how to accomplish this is found in a later recommendation. Given that the balance is about right, I would not recommend a change to the process that has successfully arrived at the balance. The HED and ICF Councils together with NNSA and laboratory leaderships have found a management approach that seems to work. The non-fusion activities advance understanding in weapons relevant areas and/or contribute to fundamental science that can be an attractor for future stewards. All of the above activities help to maintain or advance designer SKA. In the days of UGT, there was significant pre-work done above ground before the experiment was ready to go down hole. Peer review, diagnostics development, calibration, experimental planning, etc. An experiment on NIF or Z has many of the same elements. A good example is a cryogenic capsule experiment on NIF. In order to maximize the opportunities for innovation and "white sheet" design, NNSA must continue to support breadth in the program and encourage the exploration of new ideas.

Findings: LLE provides a unique opportunity for steward development.

Comments: Because of its university affiliation and the Omega facility, LLE is able to attract and help train current and future stewards in the weapons program. LLE has a long and proven record of educating students who have later gone on to become valuable contributors to the SSP. Omega also provides a cost effective test bed for proposed experiments and diagnostics on NIF and Z. Through the laboratory basic science program, LLE provides the nuclear weapons laboratories scientists time to do non-programmatic research which in turn acts as an attractor to and helps to retain these staff at the NNSA laboratories.

Recommendations: Given the stress on other university participation in developing future stewards, NNSA should continue a strong relationship with LLE and encourage laboratory staff engagement in experimental research on Omega. Sabbaticals might make sense to strengthen the ties.

Program Management

Findings: The HED Council is the model for laboratory direction setting and prioritization in this field. It is referenced as the model to be followed by the ICF Council and the Subcritical Experiments Council. The HED Council, together with NNSA, have constructed a robust program management system that aligns program activities with the larger goals and objectives of the SSMP.

Comments: The HED Council is working well. The ICF Council is in definition and its value is yet to be realized. The Councils, working with NNSA, have balanced the experimental efforts on the ICF facilities and at present that balance appears to be appropriate to make reasonable progress on the various SSMP objectives.

Recommendations: NNSA should assess the evolution of the ICF Council in comparison to the model of the HED Council. Due to the facility centric nature of the three ICF approaches, the ICF Council will likely have much less impact upon program direction than the HED Council has had on HED science. In my opinion, to this point in time, the ICF Council has not worked at the required level of detail to make recommendations for or reach consensus on research paths that go beyond very generic questions. For example, LLE is unlikely to advise SNL on the next best MagLIF experiment to conduct on Z. I do not recommend forcing the ICF program into the mold of the HED program. They have different organizing principles. An exception to this observation is the good work done to arrive at consensus on the National Diagnostic Plan. As long as the balance between and within the ICF and HED efforts remains consistent with NNSA overall program priorities, and I believe that they are today, NNSA should assume a concurrence role in reference to Council recommendations. The required balance changes with time and judgement must be regularly applied. It is difficult to be quantitative about the right balance but it was very clear that during the waning days of the NIC, the balance of activities on NIF was too heavily toward ignition with little room for non-ignition SSP experiments. (You will know it when you see it.) NNSA ultimately has the accountability for overall program performance and has the authority to non-concur should that be needed.

Findings: There is no common view about the ICF Council role nor the advisability of combining the ICF and HED Councils.

Comments: The ICF program is organized along three approaches to fusion and these are highly facility (NIF, Omega, Z) specific. The HED program in contrast is organized along PRDs and is largely facility agnostic. Both programs make heavy use of the ICF facilities so there is some discussion about combining the two Councils. Some are of the opinion that the ICF Council adds no value when it comes to prioritizing experiments. If there is common understanding between the laboratories and NNSA about the respective roles of each Council, it was not apparent during the review. (The answer to a similar question about the need to, or value of, combining NA-112 and NA-113 to better coordinate efforts was unanimous that there was no need to combine the organizations and, in fact, it could be detrimental to the overall effort.)

Recommendations: NNSA should work with the laboratories to revisit the ICF Council charter and to gain a common understanding of its role and value. Decide if the HED and ICF Councils are duplicative efforts and, if so, combine. If not, then more clarity in the ICF Council charter, roles and responsibilities is warranted.

Findings: Tremendous value was derived from the May 2012 review of the ignition program at NIF, the June 2014 HED workshop, and the more focused review of NIF experiments and plans in August of 2014. The progression toward more community involvement and openness was evident with each new event.

Comments: The value of these events was enhanced by the organizers who invited a wide range of expertise, set high goals for the events, made a record of the conclusions and discussion and reported out the results afterwards.

Recommendations: NNSA should, as currently planned, periodically sponsor similar events. A workshop on progress toward ignition that addresses all three ICF approaches is in planning. A workshop on approaches to high yield would make sense soon as well.

Findings: Progress in evaluating the magnetic drive approach to ICF has been slowed due to the high demand for materials work on Z and the lowered ICF budgets at Sandia.

Comments: Sandia has suggested that much of the materials work could be offloaded from Z to a dedicated and specifically designed accelerator and that would allow a doubling of the ICF effort on Z in addition to reducing the risk of damage to the Z accelerator. This seems to be a good suggestion and I expect a machine of this type would be valuable to our allies as well. In order to help plan for a high-yield future, the magnetic drive approach needs to be evaluated at a faster pace.

Recommendations: NNSA should establish an independent review of Sandia's proposal. If the review substantiates Sandia's expectations for the new accelerator and the claim that the fusion effort could be doubled, then NNSA should fund this investment. A major application of this facility would be driven by the NA-80 Counterterrorism and Counterproliferation program. NA-11 should explore the possibility of a partnership with NA-80 and possibly AWE. For example, shared development and operating costs and possibly the construction of two accelerators (one for SNL and one for AWE).

Findings: The cost of change of station is prohibitive and presents a barrier to program integration.

Comments: Encouraging broad cross-laboratory participation in experiments on ICF facilities is in the best interest of the ICF/HED programs. These are complex facilities and considerable time is invested in successfully executing each experimental series. LANL described the issue of utilizing NIF effectively. They believe that a change of station, especially an on-site engineer, is very helpful but the rules for those assignments make them unattractive. It seems that the rule makers are being penny wise and pound foolish given the goals of the program and the expensive facilities being utilized. Encouraging is the fact that LANL has grown its NIF experimental effort through the HED Council from 6 to 40 shots recently in spite of this difficulty.

Recommendations: NNSA should revisit this issue in light of the barriers this rule poses to collaboration and utilization of these facilities. If the rules originate within NNSA, they should be rewritten. If the rules are driven by forces outside NNSA, NNSA must be the advocate for change.

Findings: The budget level in HED/ICF is impeding the rate of progress.

Comments: Budget issues were common throughout this review. LANL suggested that their budget was insufficient to sustain HED but followed with a suggestion that ASC should fund some of their HED code development work that is presently funded by the ICF/HED program. Sandia would like more ICF experimental time on Z and suggested that it could be accomplished by offloading materials work to a new accelerator (cost TBD). NRL said their budget was below their requirements case but that requirement has been reduced from a 2011 level of $12M to $9M today. To my recollection, LLNL and LLE were silent on budget issues.

Recommendations: NNSA should continue to enhance coordination between the HED/ICF efforts and the ASC program. A major objective of the SSMP is the improvement and validation of nuclear weapon simulation codes. Several components required in this code development and validation effort (HED, ICF and ASC) fall within the purview of NA-11. NA-11 has the ability to address the LANL recommendation if needed and should take an active role in determining the appropriate balance. As NA-80 requests more time on ICF facilities, NNSA should look to that program to help support the operational costs.

II. Planning associated with tri-lab Director's letter

Long-Term Direction

Findings: The laboratory director's letter is a strong statement of the need to sustain world leadership in HED science. Further, the letter reaffirms the need for continued development of the science of HED/ICF and the pursuit of ignition and high yield.

Comments: The letter is long on vision and short on specifics but that is to be expected. It is encouraging that the three NNSA laboratories found common ground and pledged to work together towards an "integrated and coordinated National HED effort". The HED Council has gone a long way to defining that effort where HED science is concerned. The ICF effort is not as far along and needs some emphasis at this point. It is not clear to me that the ICF Council can provide that emphasis alone and NNSA may need a stronger hand at this point.

Recommendations: NNSA should establish a regular forum to report on and monitor progress toward the "integrated and coordinated "effort promised in the director's letter. NNSA should drive the ICF Council to establish roles, responsibilities, authorities and accountabilities that are acceptable to all elements of the program or should decide that this Council does not fulfil a need and put its efforts elsewhere.

Findings: HED/ICF experiments are advancing the Nation's test readiness posture.

Comments: There is significant overlap in the skills associated with conducting complex, highly integrated, and expensive experiments on the ICF facilities and those needed to conduct a nuclear test. Specifics include diagnostic development and fielding and managing many different interfaces through design, fielding and analysis. As long as maintaining the capability to test remains a National imperative, these HED/ICF experiments will help to demonstrate that capability.

Recommendations: NNSA should direct its laboratories to develop a report to document the skills overlaps between ICF/HED experimentation and UGT. This does not need to be a long report but would be useful to have in response to inquiries on this topic.

III. High Yield for SSP

Applications of Yield

Findings: The value of a laboratory high yield capability was recognized very early on in the ICF program history and has repeatedly been affirmed as a worthy goal that would be put to very good use should it be available for weapons experiments.

Comments: The difficulty of maintaining Congressional support during the construction of NIF and continuing through the ignition effort, caused some to lose track of the initial goal for the program to provide high yield (several 10s of Megajoule). The unstated argument was that defining a capability need beyond what NIF could reasonably be expected to provide would be fatal to the project. Why build NIF when we need something bigger? NIF and ignition were initially recognized to be a necessary step toward high yield but, in part because of the difficulty of achieving ignition, the high yield goal suffered from diminished visibility. One senior program leader even stated that "there never was a requirement for high yield". I am encouraged that NNSA is once again seriously considering the ultimate goal of its ICF program. Certainly high yield is decades away and necessarily follows demonstration of ignition but there is no existing facility (or approach, short of a nuclear weapon) that can credibly promise high yield today.

Recommendations: NNSA and its laboratories should acknowledge the ultimate ICF goal of high yield in the laboratory and the necessary first step of ignition. In my opinion, serious consideration should be given toward determining a conceptual design for the next ICF facility and what physics and engineering uncertainties need to be addressed before any decision to begin the process of justification or design. The goal would be to establish a research path beyond the existing capabilities and create the next big vision for the NNSA RD&T HED experimental program.

B.3 Group 3 Reviewer Reports

Reviewer Report: Sean M. Finnegan

I. Underlying physics understanding and integration.

EOS

Findings: High pressure equation of state (EOS) microphysics is a frontier scientific endeavor. Experimental tools like the NIF are producing conditions within materials that only naturally exist at the cores of astrophysical objects and thus have never before been studied in the laboratory. Similarly, facilities such as the LCLS and APS are providing the opportunity to diagnose the evolution of materials as they transition through phases with a level of precision capable of distinguishing between our most advanced theoretical models. Researchers are taking full advantage of the emergence of these capabilities to generate experimental data capable of constraining EOS models for use in ICF codes.

It is clear that the development of high quality, phase-aware, EOS which is self-consistent with both structure and strength and its implementation into global models such that phase transitions including solid-solid transitions and refreezing are accurately captured will challenge researchers for decades to come.

While it was not necessarily discussed explicitly during the review, high pressure EOS studies are also relevant to researchers studying the formation of planetary cores. This broader application beyond ICF will generate opportunities to engage with researchers outside of the field of ICF which could pay significant dividends in terms of broadening the number of program stake holders.

Comments: The physics underlying high pressure material equation of states (EOS) is fundamental to the field of HEDP and its application to ICF. Like every aspect of HED and ICF science, resolved measurements of microphysics, capable of distinguishing between theoretical models, will advance the field toward the development of a truly predictive capability.

Recommendations: The NNSA HED/ICF program is encouraged to:

- Continue to utilize all available experimental platforms (including cross-platform comparisons), and potentially develop new complimentary platforms and diagnostics, to directly validate micro-physics.
- Develop and field diagnostics capable of directly measuring temperature.
- Continue to develop and validate new techniques.

Opacity and transport

Findings: Regarding opacity research, there appears to be a clear understanding within the research community as to where the significant challenges/opportunities exist for progress in advancing our understanding of underlying physics in the coming decade. This seems to be true for both LTE and non-LTE environments. Additionally, researchers are making excellent use of multiple experimental platforms both within the NNSA portfolio (e.g. Z, NIF, OMEGA) and more broadly in the DOE portfolio (e.g. LCLS) to test theoretical models and predictive capability. Excitingly, experimental data from the Z machine at Sandia are both demonstrating excellent agreement with models of some materials (e.g. Ni) and striking disagreement for other materials (e.g. Fe). Such discrepancies between observations and theoretical models are clear opportunities to advance our understanding.

Perhaps the single biggest concern in the area of opacity is the diminishing availability of scientists trained in the field of high temperature (high energy density) atomic physics and spectroscopy. If left unaddressed, loss of such expertise stands to eventually erode the NNSA's core competency in this area.

As with opacity research, scientists studying transport (electric, thermal, radiation, particle, etc.) in HED/ICF systems are making excellent use of a wide variety of experimental facilities both within the NNSA portfolio and more broadly (e.g. LCLS, ALS, etc). Quit surprisingly though, there is limited experimental data in ICF relevant regimes, though this appears to finally be changing. It will be exciting to see how precision measurements on world-class facilities challenge the state of the art micro-physics modeling (e.g. DFT-MD, Purgatorio, etc.).

Comments: Like EOS, both opacity and transport studies are foundational to the field of high energy density physics particularly as they underpin our ability to model and make predictions about ICF system performance. The availability of facilities like NIF, Z, LCLS, etc. along with the continued development of high performance computing platforms are enabling scientists to challenge our understanding of micro-physics and atomic processes in extreme states of matter with unprecedented precision and resolution.

Recommendations: The NNSA HED/ICF program is encouraged to:

- Evaluate the developing workforce situation concerning the availability of scientists trained in HED atomic physics and spectroscopy. If it is determined that the situation is at a tipping point, the NNSA should consider creating a center of excellence in HED atomic physics that engages in HED research as it pertains to both ICF and astrophysical systems. This could serve as a natural way of tapping into the much larger field of observational astronomy/astrophysics. Such a coupling could significantly improve workforce availability in this area and introduce new innovations into the program.
- Continue to utilize all available experimental platforms (including cross-platform comparisons), and potentially develop new platforms, to resolve outstanding problems in both opacity and transport studies.

Hydro and burn physics

Findings: It is difficult for me to determine if uncertainties in predicting the growth of hydrodynamic instabilities (RT, RM, or KH at low or high mode number) in ICF experiments stems from gaps in our underlying understanding of these instabilities or rather from gaps in our fundamental knowledge of the particular material EOS, transport, or simply the inability to accurately assess the full impact of the driver in altering the initial conditions on (and in) the target (e.g. CBET, hot electron pre-heat, etc.). In ICF, it seems to me to be driven more by the latter rather than the former and as such those opportunities for improvement are discussed in other sections.

There does, however, appear to be significant opportunity to advance our understanding of the evolution of these instabilities to a turbulent state and the resulting mixing of materials in the target. In recent years there is a growing body of data suggesting the importance of kinetic processes, enabled largely by the nuclear diagnostics developed by the MIT group. Developing a complete understating of such processes will challenge all three phases of research: experimental platform development, diagnostics development, and the development of multi-scale modeling capabilities. In fact, the integration of kinetic or micro-physics effects into the modeling of integrated systems in a self-consistent way is a grand challenge for the program and will push the frontiers of high performance computing.

The importance of accurately capturing the complete hydrodynamic and kinetic behavior at all scales (including 3D flows and viscosity) in converging targets is highlighted by the fact that outputs from numerical simulations are routinely used to infer the properties of imploded targets (e.g. hot spot temperature). In the absence of appropriate diagnostics, numerical modeling is our next best option to understanding behavior of these systems and as such the multi-scale problem needs to be met head-on.

Comments: It is near impossible to understate the importance of hydrodynamic and burn physics to the HED/ICF program. It is therefore no surprise that the portfolio of research supported by the HED/ICF program has no equals and is unmatched in breadth, depth, and standing. Many of the problems faced by the HED/ICF program in this area are also challenges for other communities (albeit in different geometries e.g. divergent as opposed to convergent systems) which may present an opportunity to bring in new innovative ideas.

Recommendations: The NNSA HED/ICF program is encouraged to:

- Explore introducing a “common” rad-hydro code, openly accessibly to the broader research community that will server to reduce the barrier to entry for collaborating on research projects and designing experiments for the NIF. There would be significant benefit to making the source code available for the community to develop and introduce new packages for their own purposes. In this way the code would become a living tool capable of growing and expanding with the research needs of the community in real-time.
- Specifically, seek to engage the university community in the validation of physics packages in integrated codes, through experimentation and diagnostic development
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Global and driver physics

Findings: In many, if not most, HED/ICF experiments in the laboratory, the driver (often laser light) imprints itself on the response of the target, in ways both intentional and unintentional, and as such the nature of that interaction often needs to be fully characterized before the focus of the experiment can be completely understood. While this statement is seemingly obvious, the disparate temporal and spatial scales associated with characterizing and understanding Laser Matter Interactions (LMI), particularly when compared to hydrodynamic scales, makes this problem largely intractable. As a result this aspect of HED/ICF physics is often oversimplified if not ignored entirely.

This seems to be particularly true for performance driven pursuits like the National Ignition Campaign. Rather than emphasizing study and understanding of laser matter interactions (a process critical to the deposition and distribution of energy in the hohlraum), a strategy of avoidance and tolerance of deleterious instabilities was adopted. Again, this is understandable, to a degree, given the complexity of the problem. However, laser-plasma interactions are so fundamental to laser-driven ICF that this challenging problem needs to be addressed head-on in a comprehensive way (i.e. dedicated experiments, new modeling tools, and theory).

Lastly, it appears that the number of U.S.-trained research scientists working in the area laser-plasma interactions continues to diminish. This reality could present a problem in the future as it is unlikely that laser drivers will be replaced any time soon.

Comments: I specifically chose to focus my comments on laser-plasma interactions as it is potentially the most relevant and important element of driver physics with opportunities to improve our underlying understanding and to improve our physics modeling. The reason is simple, most HED/ICF platforms supported by the HED/ICF program are laser driven and all three mainline efforts pursuing the achievement of ignition in the laboratory involve laser-plasma interactions. The subject is quite simply unavoidable and tolerance may be unacceptable.

Recommendations: The NNSA HED/ICF program is encouraged to:

- Support research to understand the statistical, self-organizing nature of LPI at multiple scales in laser driven ICF systems to place realistic bounds on mitigation strategies. This includes the development of new multi-scale kinetic modeling tools and dedicated experiments.
- Engage the broader academic community. The absence of a diverse research community working on laser-plasma interaction problems means that there is no community to peer-review the efforts of the national laboratories in developing strategies for mitigation or exploitation.

II. Partnerships with external entities

Community: codes

Findings: A potentially significant barrier to collaboration between researchers at the NNSA laboratories and scientists at universities, both domestic and international, is access to a common set of “codes” which are used to model laboratory experiments. This is particularly true for radiation hydrodynamics codes. The rad-hydro modeling code of choice by researchers at the NNSA laboratories has increasingly become HYDRA. Export control however limits access to this tool, effectively marginalizing researchers outside of the NNSA laboratories. Furthermore, pre-shot simulations are becoming increasingly important in experiment design on the major facilities, particularly the NIF. As such, researchers who do not have access to codes which are implicitly endorsed by the facilities are at a significant disadvantage in terms of experimental design and ultimately being awarded shot time through a competitive proposal and peer review process.

Furthermore, integrated codes, such as HYDRA, are built upon sets of interconnected physics packages, the accuracy and validity of which often needs to be verified with well diagnosed single-physics (or as close to single-physics as possible) experiments. Where possible, I believe that the university community is best positioned to deliver on this need, as the academic environment incentivizes basic science.

Comments: With the increasing availability of memory and high performance computing, numerical modeling is playing an ever increasing role in designing and interpreting experiments, particularly where diagnostic data is not available. Researchers must exercise caution when simulations are used to infer statistical properties of matter (e.g. temperature of the hot spot of an imploded ICF capsule, temperature in hohlraum plasma) in experiments when no physical measurement is actually made. I fear that this may lead to a false sense of confidence in our understanding of highly integrated systems and distract from the fact that there is no substitute for actual measurements.

Lastly I would like to comment on the danger of running ever bigger calculations on increasingly more processors for the sake of "big science". While I appreciate the public relations value that such calculations can yield as well as the potential to capture the full 3-dimensional spatial nature of a problem, they also cost both significant time and money and the overabundance of information which is generated can be immensely difficult to interpret or even provide a false sense of understanding. I therefore encourage researchers to work "smarter" and not necessarily look toward the crutch of size, speed, and power. Bigger is not always better.

<u>Recommendations:</u> The NNSA HED/ICF program is encouraged to:

- Explore introducing a "common" rad-hydro code, openly accessibly to the broader research community that will server to reduce the barrier to entry for collaborating on research projects and designing experiments for the NIF. There would be significant benefit to making the source code available for the community to develop and introduce new packages for their own purposes. In this way the code would become a living tool capable of growing and expanding with the research needs of the community in real-time.
- Specifically, seek to engage the university community in the validation of physics packages in integrated codes, through experimentation and diagnostic development.

Community: Experiments

<u>Findings:</u> While the leadership-class machines like the NIF, Z, Omega, and Omega-EP are well supported and maintained, the enabling facilities like Trident and JLF appear to be eroding in their capabilities due to lack of budget priority. Facilities such as these play an important role in the experimental "ecosystem" as the relative cost per shot and shot rates afford the opportunity to test and vet high risk – high reward ideas and diagnostics before being fielded on the leader-ship class systems.

Support for a healthy "ecosystem" or hierarchy of machines is important to cost effectively maximizing scientific yield from the premier facilities. At present the HED/ICF program supports a complete set of facilities which utilize lasers as the principle target driver, with the National Ignition Facility as the crown jewel. However, the set of pulsed power machines is far from complete. Unlike with laser systems, the hierarchy of pulsed power machines is incomplete as there is no intermediate scale machine to aid in the development of ideas before they are fielded on the Z machine.

The health and vitality of the HED/ICF field is enabled by, and depends critically on, the programs experimental capabilities. More importantly, the existence of a university community and their ability to meaningfully collaborate with and contribute to HED/ICF research, including workforce development, depends critically on open access to the facilities supported by NNSA.

<u>Comments:</u> The HED/ICF program maintains a world-leading set of experimental facilities at multiple scales ranging from modest (e.g. Trident, Jupiter Laser Facility, Nevada Terawatt Facility, etc.) to massive (National Ignition Facility, Z machine). These facilities continue to collectively push and expanded the frontier of discovery in HED and ICF science. Moreover, these facilities attract many of the best and brightest researchers into the field as they offer the opportunity to study matter in the laboratory in a state which has often never before been created on Earth and only naturally exists in astrophysical systems. Providing open experimental access through a competitive peer review process is critical to capturing the full potential for discovery on these facilities. While only a limited amount of time is made available on these machines for open discovery-class science (i.e. non-program), each and every experiment represents tremendous value for the researcher, student, or partnering agency.

<u>Recommendations:</u> The NNSA HED/ICF program is encouraged to:

- Continue to maintain and provide access to their world-class facilities at all scales through competitive, peer reviewed selection processes.
- Regularly engage the user groups of NNSA facilities to solicit feedback regarding operations procedures, facility needs, and future diagnostics.

- Thoroughly evaluate the portfolio of laser facilities at the bottom of the "ecosystem" and consider possible reductions or closures in order to support the most impactful facilities fully.
- Consider developing an intermediate scale pulsed power system.

In addition to the above, the NNSA should explore partnering with other funding agencies to jointly develop or maintain facilities at the small and intermediate scale where it is of significant mutual benefit. It is not clear that the NNSA can continue to bear this burden alone.

Community: Collaborations

Findings: It is clear that the NNSA recognizes the value of collaborative partnerships to both problem solving through innovation and scientific discovery, as well as workforce development. The level of collaboration between the three NNSA laboratories appears to have increased in recent years (since 2012), particularly in research areas of importance to the ICF program. While there is still plenty of room for improvement, this is a significantly positive step forward for the program. In the absence of the laboratories actively competing with each other to solve identical sets of problems, the best way to innovate and make progress is for them to work collaboratively.

In addition to cross-laboratory collaboration, the HED/ICF program also supports collaborations between the national laboratories and university researchers through grants programs, graduate student fellowships, and providing access to the world-class experimental facilities operated by the program. Scientifically, the existing set of collaborations has been particularly successful in developing new and innovative diagnostic tools which have proven critical to gaining deeper insights into the kinetic physics behavior in ICF targets. Perhaps the greatest benefit to the HED/ICF program from collaborations between the national laboratories and universities has been workforce development.

Regarding workforce development, the HED/ICF program continues to successfully use partnerships between the National Laboratories and universities, both domestically and internationally, as a "pipeline" to train and recruit the next generation of HED scientists. As stated previously, this is accomplished through maintaining grants programs, graduate student fellowships, and providing access to NNSA operated experimental facilities. All three of these elements are essential to meeting the workforce needs of the program, and should be maintained.

Lastly, it is important to maintain a vibrant community of researchers external to the national laboratories to serve not only as a pool of potential collaborators, but also as a scientific system of checks and balances. The value of such an external community was not capitalized upon during the National Ignition Campaign, and provided important insights after the fact through their participation in the Science on Fusion Ignition Workshop (San Ramon, May 2012).

Comments: Diversity in collaborative partnerships encourages and fosters innovation and the development of creative solutions to both near-term and long-term problems. This is true at all scales (micro: individual researcher, meso: institutional, and macro: federal agencies) and across scientific sub-fields. I see tremendous opportunity for the HED/ICF program to leverage their world-leading computational and experimental capabilities to not only maintain existing partnerships but to also establish new value-added relationships at all levels to deliver on its mission. Without continued collaborative engagement with a diverse set of partners, the program is likely to see stagnation in innovation, discovery, and an inability to recruit and retain a workforce with the talent necessary to accomplish the mission.

Recommendations: The NNSA HED/ICF program is encouraged to:

- Maintain existing competitive grants programs, graduate fellowships, and access to experimental facilities by researchers external to the NNSA national laboratories.
- Promote diversity in the number of institutions (university, national laboratories, private companies) collaborating with NNSA laboratory researchers on solving fundamental HED and ICF science and technology (i.e. diagnostics, drivers, etc.) problems.
- Reward scientists at the laboratories for developing and fostering successful collaborations with researchers at universities and private companies by directly supporting their time an effort to engage in those activities. Positive career development incentives will ensure that researchers actively seek out opportunities to collaborate.

Reviewer Report: Yogendra M. Gupta

This report is organized in the following four parts: Overall Comments; HED Science; the ICF Program; and External Partnerships. Please note that I am making a distinction between HED Science and the ICF program. To me, the former is a scientific endeavor while the latter has well defined goals that utilize the knowledge gained from the various HED scientific activities.

I. Overall Comments

A. Observations

1. The three NNSA Laboratories are interacting in a positive and synergistic manner. Despite their different approaches, they recognize that achieving ignition is an enormous scientific challenge. Healthy, competitive/collaborative dialogues and interactions were evident.
2. Although a significant amount of information was provided by the Laboratories about what they have been doing and are doing (in various write ups and in the presentations), I would like to have seen a succinct listing of the important and foundational science needs/challenges that should be addressed to achieve ignition in the Laboratory. Perhaps, the enormity of the highly coupled, nonlinear problem does not lend itself to succinct definitions of the different challenges.
3. What states of matter does High Energy Density refer to? A better definition of HED states would be very useful in discussing the relevant scientific issues and questions that need to be addressed.
4. The linkage between fundamental science activities and the ICF Program needs was not obvious.
5. The highly coupled (or integrated) nature of the physical phenomena "on the path to ignition" necessitates the use of complex codes and raises the following question: how feasible is it to "untangle" the physical phenomena of interest and examine them through a less integrated approach?
6. The scientists involved in the ICF Program are extremely talented, aware of the enormous S&T challenge that they have embarked on, and their passion and commitment toward the "ignition" goal came through.

B. Comments/Recommendations

1. NNSA (HQ) and the Laboratory Directors should do everything reasonable and possible, even in times of tight budgets, to encourage and ensure that the healthy competitive/collaborative spirit is maintained well into the future.
2. Laboratory Ignition is truly a scientific "Grand Challenge"! The United States – through the DOE/NNSA – must remain committed to this extremely ambitious goal and develop a long-term investment strategy that is both appropriate and sustainable.
3. Despite the highly coupled nature of the problem, it would be useful for the experts to link the ICF goal to foundational scientific challenges. Such a link would help establish more effective ties between the academic community and the Lab. scientists, and will greatly benefit the ICF program in the long run.
4. Regarding the definition of HED states, I offer some suggestions in part II.
5. The foundational scientific challenges that constitute the hurdles to achieving ignition in the laboratory need to be articulated and prioritized as much as possible.
6. It would be useful (though likely difficult) to define some fundamental science experiments that examine and evaluate different parts of the simulations being used to design and analyze experiments.
7. The leadership at NNSA (HQ) and the leadership at the Labs need to chart out a sustainable path to encourage and foster a sense of intellectual inquisitiveness and excitement among the scientists working in the ICF program.

II. HED Science

A. Definition of HED states

Not having a clear definition of HED states makes it difficult to discuss the relevant scientific phenomena and issues in a meaningful manner. Some of the definitions in current usage are: pressures above 1 Mbar or experiments carried out at HED facilities (Omega, Z, NIF) or experiments carried out using high intensity lasers. None of these definitions are satisfactory. Clearly, 1 Mbar in tantalum is not the same as 1 Mbar in hydrogen. Perhaps, the First Law of Thermodynamics can be used to define HED states

$$dE = dW + dQ$$

HED means a large internal energy increase in a small volume. Since mechanical work is primarily due to volume compression, it would be better to define HED states in terms of volume compression and temperature. The latter could be defined in terms of the Fermi Temperature. I recognize that the appropriate definition of HED states is not a new issue (http://science.energy.gov/~/media/fes/pdf/workshop-reports/Hedlp_brn_workshop_report_oct_2010.pdf).

In short, HED states need to be defined in terms of volume compression and temperature, and a lower bound needs to be agreed upon for defining HED states.

B. EOS

Often the term EOS is used (incorrectly) to denote a P-V relation. The development of an accurate EOS (not just an isotherm or a Hugoniot) for a material is a central need because so much of the rest of the material response depends on the accuracy of the EOS. Although there is a broad agreement within the HED community regarding this need, experimental data to provide fundamental insights and to discriminate between different EOS models are lacking. *Accurate temperature measurements over the broad range of compressions and temperatures relevant to HED states remain an important need.* This long-standing need for accurate temperature measurements simply has not received the concerted and sustained investment needed to ensure reliable determination of temperature. Temperature measurements are important discriminants in evaluating different EOS models.

Over the past 20 – 25 years, there has been a very large increase in publications that report EOS developments using increasingly sophisticated theoretical methods. In contrast to the calculations, accurate experimental data (even P-V data), outside of the Hugoniot data, are quite sparse. Deuterium, central to fusion, offers an interesting example. A recent paper by Sandia researchers (published in Science at the end of June) reported metallization of deuterium by sampling P-T space through ramp (or shockless) compression preceded by shock compression. The experimentally determined phase boundary for this insulator-metal transition was very different from the many published theoretical calculations. The differences among the various theoretical calculations were also extremely large. The obvious question is: why the very large spread in the theoretical calculations and why are they so different from the experimental calculations?

Because an accurate EOS (spanning a broad range of density-temperature space) is central to all other aspects of HED science, the following measurements need to be pursued in a sustained and meaningful manner.

- Accurate P-V measurements spanning a large region of density- temperature space
- Accurate T measurements over a large region of density-temperature space
- Measurements that more directly examine the microscopic nature of the HED states

It is important that multiple platforms (e.g. pulsed power, lasers) and different diagnostic capabilities (for temperature measurement) be used to ensure consistency and accuracy of these measurements.

C. Material Strength

Material strength models are commonly used to represent time-independent, inelastic deformation of solids. Though convenient, time-independent, “strength models” may not be the correct way to represent the actual phenomenon of interest – Resistance to Deformation (RTD) under dynamic loading.

Irrespective of the above comments, it is not clear why material strength – used to describe inelastic deformation in solids – is important at HED conditions. Of course, this statement depends on the definition of HED states. Although the study of inelastic deformation under dynamic loading is an important need for materials related issues (Campaign 2), its relevance to the ICF program or at HED conditions is not obvious.

If inelastic deformation is indeed important for HED states of matter, then the determination of “material strength” or RTD needs to go beyond time-independent, phenomenological approaches. Without going into a lot of details, let me state that understanding RTD or developing accurate strength models that are applicable for a wide variety of load paths remains a significant challenge and an important need. Plane shock wave or ramp compression data are not sufficient to discriminate between different “strength” models, which themselves depend on the assumptions being made (in the models) and the EOS being used.

III. ICF Program

Because of a lack of scientific expertise in this area, I don't feel qualified to offer any substantive comments.

A general comment/question: is there a way to break the very integrated approach into smaller elements so that the same can be examined using simpler or fundamental science experiments?

IV. External Partnerships

The discussion period with external constituencies (almost exclusively academic faculty members) was very helpful in understanding the relevant issues related to academic involvement at the HED facilities.

The Sandia/UT (Austin) collaboration on the astrophysics work at Z is a great success story. Alan Wooten's strong interest and role in making this happen deserves special mention.

It is clear that Z and NIF are not typical user facilities. Both at Z and NIF, there have to be strongly engaged Lab scientists who are genuinely excited about the science outcome and have a good understanding (including the requisite savvy) of how to get experiments completed in a timely manner. In short, the Lab person has to have the strong scientific interest, time, and stature (within the Lab) to help the academic partner.

Several issues that have hindered stronger academic engagement were brought up in the discussions and the same are listed below.

1. Export control: Academic partners not being able to access codes to design potential experiments at the HED facilities was viewed as a major hindrance by all academic attendees. According to some individuals, the export control restriction was unnecessary.
2. The academic community needs to be certain that multi-year (and not just one year) access to the facilities will be available when their project is selected to receive time at the facility. This is needed for graduate students to do their work and, evidently, is not the norm at all facilities.
3. The time that the Lab scientists can spend on University/Laboratory partnerships was brought up as a challenge for the Lab scientists involved in such partnerships.
4. Facility time by itself is not sufficient. There needs to be funding for the academic partners to cover other costs.
5. The national diagnostic plan needs to be broadly disseminated and discussed with the academic community.

Summary/Recommendation (External Partnerships)

Although one meeting is not sufficient to really understand all the relevant issues and to develop a path forward, the above comments need to be discussed between the appropriate individuals at NNSA (HQ) and at the Labs to develop a clear and transparent path forward to engage the academic community in a meaningful manner.

I am not advocating more meetings or forming more committees. Instead, each NNSA Lab that wants to engage the academic community at their HED facility should inform the interested users about the ground rules for being involved: selection criteria for projects; proposal template; how will the non-facility costs be covered; will multi-year access be guaranteed for selected projects; Lab PI for each project, etc. Also, the Lab has to interact with NNSA (HQ) to ensure that sufficient resources can be committed to academic partnerships.

At the end of the day, the Lab management and scientists (with NNSA's support) have to be very clear in their own minds about the strong commitment to meaningful academic partnerships. They have to be realistic about these commitments in the face of various programmatic demands and budget uncertainties.

As a university faculty member, my final comments regarding this matter are as follows. In establishing meaningful Lab/University partnerships, expectations for both sides need to be carefully defined and agreed upon at the beginning. Subsequently, the expectations need to be carefully managed through the completion of the research project. The academic institutions have a lot to offer to this field. But the disparate scientific cultures between the National Labs and the Universities will require care on both sides to ensure a successful outcome.

Reviewer Report: Stephanie Hansen

Executive summary:

We are poised at the brink of a golden age for fundamental High-Energy-Density (HED) science in the United States. With extraordinary experiments at world-class facilities creating extreme states of HED matter, unprecedented simulation and computing capabilities, advancing instrumentation enabling ever-more stringent tests of those capabilities, and the highly visible failure of a scientific approach that is over-reliant on numerical simulations, we have both the tools and motivation to strengthen the foundations of HED science. Progress will be most rapid in a research environment that encourages deep interaction between simulation, theory, and experiment, values curiosity-driven investigations informed by programmatic goals over results-driven "engineering" tasks, and offers a stable funding outlook to facilitate challenging long-term projects and substantive university interactions. Without this environment, we risk squandering the present opportunities for progress in basic HED science and more deeply embedding a false confidence in our understanding of complex HED systems.

The state of HED science

Like any scientific endeavor, fundamental high-energy density science progresses by bringing its models of the world into harmony with the voice of nature, which is expressed through experiments and mediated by measurements. Applications such as credible ignition and weapon designs are built on a foundation of scientific understanding, the integrity of which requires well-diagnosed, repeatable experiments and well-founded, falsifiable models. Here the field of HED science faces unique challenges: HED experiments are complex, with profound transience and gradients that make measurements difficult to perform and interpret. Frontier HED experiments are expensive, making repeat experiments and cross-platform comparisons rare. The simulations that help design and interpret these experiments are extraordinarily intricate, tracking the flow and interaction of material and energy over enormous variations in length scales. And both simulations and diagnostics are deeply intertwined with theoretical models of material properties that remain largely untested at the extreme pressures and temperatures that characterize HED material.

In the face of these challenges, the nation's recent investments in HED facilities, diagnostics, and computing capabilities have set the stage for an era of discovery in HED science. Experimental facilities like the NIF at LLNL, Omega at LLE, the Z machine at SNL, Trident at LANL, NIKE at NRL, and the LCLS x-ray free-electron laser at SLAC concentrate energy in space and time to produce states of matter at extreme conditions never before studied on earth. While such "exotic" states of matter compose most of the visible universe, active laboratory interrogation is much more informative than passive observation of distant astrophysical objects. Laboratory plasmas are diagnosed with ingeniously designed instruments that can resolve spatial, temporal, and energy dimensions rich with information. These measurements help inform both fundamental HED theory and simulations, which are both enabled by advanced computing power. Fundamental HED theory is moving towards first-principles quantum modeling of the atomic- and nano-scale properties of material at extreme conditions, providing essential information to the massively multi-scale simulation tools that are used to inform our understanding of plasmas on laboratory and even astrophysical scales. With sufficient understanding of foundational HED science, these simulation tools could be used much like the simulation tools of aerodynamics or other engineering disciplines to design and control complex systems with practical applications. Unlike engineering tools, they will also provide insight into the nature and evolution of the visible universe. But unlike engineering tools, HED simulations are not yet securely coupled to established scientific foundations.

There is a necessary element of bootstrapping in any emergent field of scientific investigation by which informed predictions are made, *tested*, and refined. Large steps away from well-established ground – such as that taken in going from Omega's 40 kJ to NIF's 2 MJ – are predictably uncertain. While the failure to achieve ignition on NIF was painful for the HED community, it was probably the best possible outcome for the health of fundamental HED science in the United States. This is not primarily because NIF provides a marvelous platform for HED science (although it does) or because NIF is now operating with ever-more efficiency as integrated ignition experiments cede limited ground to focused, well-diagnosed HED science experiments (although it is), but because the failure to achieve ignition on NIF has reminded us that we are a community of scientists, not engineers. Our tools are not yet the tools of engineers. In addition to the failure of our integrated simulations to even now explain the results from the original NIF target designs, we are finding disturbing disagreement between high-precision experimental data and our basic theoretical models of x-ray absorption, transport, and the pressure response of materials in extreme conditions. As we increase our experimental control and refine our diagnostic techniques, the shortcomings of our theories and simulations are becoming increasingly clear. With this acknowledgment and the extraordinary scientific infrastructure now at hand, we are better positioned to make real progress in fundamental HED science than we have been since the advent of the science-based stockpile stewardship program in 1995.

And this progress appears to be well underway: A survey of recent technical publications in the high-profile journals *Science* and *Nature* indicates a vibrant and growing field, with the publication rate of HED-relevant topics more than doubling from about 4 per year in 2010-2011 to more than 10 per year in 2012-2015. (For comparison, there are 1-2 MFE-related publications per year in these journals.) About 30% of the high-profile HED publications report on results obtained at facilities outside the U.S., ~20% each are from Omega and LCLS, and ~7% each are from NIF, Z, Trident, and a combination of several smaller U.S. laser facilities. Excluding the significant contributions from international facilities and LCLS to focus only on publications from NNSA-funded facilities, we find that roughly 40% of the high-profile publications originate from work at Omega and 15% each originate from all other facilities. Although NIF, Z and Trident also conduct classified research not represented in the open literature, the scientific output from NNSA facilities measured in this way does not track closely with NNSA funding allocations (roughly 65% for NIF, 15% each for Omega and Z, and 2-3% for Trident, Nike, and Academic Alliance programs). While the topical areas of the surveyed publications are broad, ranging from high-pressure materials science to laser-matter interaction and atomic physics, the drivers are dominated by optical lasers. Strong connections to astrophysics and condensed matter physics are evident. The affiliations on the surveyed publications indicate reasonably healthy collaborations among the U.S. facilities, with LLNL-LLE and MIT-LLE connections being particularly strong and LCLS collaborations particularly ecumenical, reflecting the science models in place at the various facilities. While collaborations among the NNSA laboratories are increasing, there are only a handful of strong University interactions. And while there is energetic and rigorous interaction between experiments and theory (particularly Quantum Molecular Dynamics), simulations often appear less connected to experiments; either being treated as quasi-experiments themselves or serving merely as rough guides to help interpret experimental data.

The Group 3 activities in summer 2015 presented a similar impression of the overall health of HED science: Intense interest and a deep engagement between theory and experiment was evident in discussions of focused, fundamental physics topics such as equation of state, transport, non-equilibrium physics, and (to a lesser extent) opacities. For integrated fusion experiments, too, there was a sense of excitement and collaboration among designers, experimentalists, and diagnosticians. But on topics of integrating fundamental data into simulation codes, extending simulation capabilities (e.g. by introducing native laser-plasma interaction, magnetohydrodynamics, or kinetic effects), or even rigorous testing of particular physics packages (e.g. mix models, diffusion) by comparing simulation predictions to data from tailored experiments, there appeared to be much less general enthusiasm. While the development of predictive simulation tools for complex HED systems is an extraordinarily complex task, it cannot become isolated from the experimental and theoretical facets of the HED community without losing credibility – a credibility that transfers to U.S. science-based stockpile stewardship. Only when we achieve consistent, detailed, and rigorous agreement between our modeling and our measurements can we be confident that we understand the physical processes that drive the hearts of stars, the costs of viable fusion reactors, and the operating margins of thermonuclear weapons.

Findings and actionable recommendations

1. In comparison with other scientific communities, HED has an extended design-experiment-analysis cycle of many months to years. In a corporatized research environment driven by demands for bottom-line improvements (e.g. neutron or x-ray output), the analysis part of that cycle is easily short-circuited: rather than analyze data from failures, designers begin looking for the next tweak that might enhance performance. This approach can work in an engineering context with reliable simulation tools, short experimental cycles, and cheap data, but it leads to *enormous waste* in a scientific environment where simulation tools offer limited guidance and data is dear. In frontier science, a research environment closer to the oft-derided "sandbox" model that encourages pulling the threads on curious or anomalous data and deeply analyzing failures is *much more productive* than a forced march towards a prescribed goal.

   <u>Recommendations</u>: Recognize that funding stability is essential for the long research cycles of HED science. Because target fabrication is one of the major causes of long experimental cycles, encourage innovation in target materials and flexibility in target design. Explore flexible scheduling options at large facilities to permit follow-up studies on compressed timescales for at least simple targets. Encourage and explicitly fund time for data analysis.

2. In comparison with other scientific communities, HED has developed a deep reliance on simulations to both design and interpret experiments. In a field where experiments are complex, expensive, difficult to diagnose, and not often repeated, it is tempting to use simulations to fill in the gaps in our understanding. But it is all too easy for a flawed but familiar tool to become oracular, and this carries a significant risk of misleading or even retarding progress in real scientific understanding.

   <u>Recommendations:</u> Reduce code-use and source-code restrictions (at least among the laboratories) to increase scrutiny of code components and enhance opportunities for contributions from outside core developer teams. Support tri-lab code comparison workshops and outreach to external communities with less general but more locally rigorous tools (e.g. particle-in-cell methods, line-by-line radiative transfer, extended MHD). Encourage and reward engagement between code developers and users (designers) *throughout the laboratories*. Encourage and reward engagement of code developers and designers with theoretical and experimental scientists through focused experiments that test particular aspects of the simulation codes, particularly driver physics, non-hydrodynamic transport, and mix. Encourage repeated experiments to ensure meaningful code verification metrics.

3. In comparison with other scientific fields, HED science rests on an extraordinary interdependence among experiments, diagnostics, simulation, and theory. While there are a limited number of focused-science experiments that produce relatively uniform and quiescent HED samples, most HED plasmas have large spatial and temporal gradients that require simulations to provide least qualitative guidance for data analysis. Simulations are of course informed by atomic-scale theory through constitutive models, but the diagnostic signals from even uniform HED plasmas are also intimately connected to theory: imaging depends on detailed atomic-scale emission, absorption, production rates, and stopping powers; energy-resolved measurements sample line shapes and scattering processes. Very little HED theory has been independently benchmarked – indeed, as we better control experiments and refine our diagnostics, we find few experiments that do *not* surprise us. Worse, there is a persistent circularity in even the most sophisticated benchmarking experiments (e.g. line shapes from simple ions are used to characterize the conditions that produce complicated opacity signatures from recent experiments on the Z machine; X-ray Thomson scattering is used to characterize the conditions of samples on Omega to provide benchmark stopping powers – but both scattering and transport use a common electronic structure model).

   <u>Recommendations:</u> Continue to support basic HED science experiments on platforms that emphasize sample uniformity and extensive diagnostics for cross-validation. In particular, because the disagreement between Z experiments and theoretical iron opacities has significant implications for atomic-scale theory, cross-platform experiments on NIF *must* be supported along with continuing opacity experiments on Z. Continue support of the national diagnostics plan to provide higher spatial, temporal and energy resolution for increasingly stringent tests of theory and simulations. Explicitly fund theoretical development of quantum molecular dynamics and other approached for equation of state, transport, radiation (particularly line shapes) and non-LTE physics, emphasizing experimental observables that can be used to post-process simulations for direct and detailed comparisons with data.

4. Platform diversity is essential to the health of HED science for validation of basic science at the frontiers of HED, risk mitigation through flexibility in the mode and efficiency of energy delivery, and the diverse research opportunities afforded by various facility scales and access models. The importance of cross-platform validation for fundamental HED science was highlighted in the early 2000s by a controversy over the deuterium equation of state as measured on Z and NOVA, and NIF measurements will be essential to resolve the iron opacity problem raised by recent experiments on Z. The fact that Z and NIF are natural partners for cross-platform validation underscores the importance of diverse drivers: Because HED experiments are deeply connected to the method of energy delivery, each driver carries its own idiosyncrasies in energy delivery, native efficiency, and diagnostic challenges. Optical laser drivers deliver energy cleanly, but their wall-plug efficiency is inherently low and laser-plasma interaction can further limit and distort the intended energy delivery. Pulsed-power drivers provide a harsh environment for diagnostics but are highly efficient, with energy delivery limited by breakdowns in the current feed and the induction of the target itself. Platform diversity may be a key for progress in ICF as well as basic HED science: because energy delivered to the target appears to be the critical factor in achieving ignition, the efficiency of pulsed power drivers makes them an appealing option for a future facility. Today's

20 MA Z machine delivers ~0.5 MJ of energy to its targets – similar to NIF's ~2 MJ – and can credibly produce similar numbers of thermonuclear neutrons. Looking beyond NNSA facilities, X-ray lasers like LCLS offer unique options for creating and probing HED material, the value of which is reflected in the extraordinary scientific output of the OFES-supported Matter at Extreme Conditions (MEC) endstation at LCLS. Like LCLS, Omega is a user facility that is also highly productive in basic HED science, supporting the importance of maintaining a diversity of facility access models. It is notable that while there are a plethora of small- and mid-scale laser facilities at U.S. universities that operate at intensities similar to NIF, there are only a handful of small-scale (~1 MA) pulsed-power drivers and no mid-scale or user-based pulsed-power facilities. Thus the most efficient HED driver operates at a significant disadvantage, with a much smaller workforce pipeline and very limited opportunities for scaling studies.

Recommendations: Maintain driver diversity in large-scale facilities and encourage cross-platform basic HED science. Explore options for a mid-scale (~10 MA) pulsed power facility to enhance the user base, workforce pipeline, and scientific contributions of pulsed power science; such a facility would also help verify scaling arguments for a potential high-yield pulsed power facility. Engage OFES and other agencies to leverage NNSA-supported ICF and basic HED science.

5. HED has natural resonances with other scientific communities, particularly those of magnetic fusion, condensed matter, atomic physics, and astrophysics. All of these fields have a broad university base and thus could each contribute to the HED pipeline, especially if given increased access to major HED facilities and codes. While the magnetic fusion community has served as a training ground for many leaders of today's HED community, the unique physics associated with high densities often makes the transition from classical plasmas to HED difficult. Connections with the university-based condensed-matter community appear to be growing in HED. This is valuable for two reasons: First, increasing the temperature of natively dense systems offers a critically independent test of traditional plasma-based approaches that increase the density of classical plasmas. This is particularly important in the regime of warm dense matter, which is one of the most fundamentally difficult regimes of HED – and one that is amenable to rigorous study on university-scale facilities. Second, the condensed matter community is natively uncomfortable with the large uncertainties that are often accepted as a matter of course in HED science and will push for higher precision. The atomic physics community would also seem a natural partner to HED, but its focus in the U.S. has largely shifted towards cold systems. This has led to a very limited number of university interactions in atomic physics and an atrophying of the HED-relevant atomic physics capabilities that inform both diagnostics and simulations. Critical capabilities like opacity and atomic structure codes, line shape modeling, and atomic physics databases are currently sustained by a small handful of scientists, with about half of them near retirement. The astrophysical community includes scientists with experience in multi-scale modeling and non-local effects like beams, fields, and radiation transport in regimes of high temperature and density. However, the HED community has, in general, developed more sophisticated models than the astrophysical community and interactions are currently quite limited. Increased interactions would broaden the applicability of HED science, provide critical data for the astrophysical community, and widen the HED pipeline, but their immediate impact on fundamental understanding of HED science and the fidelity of HED simulations is likely to be modest.

   Recommendations: increase university funding opportunities for focused, high-precision experimental studies in warm dense matter and for theoretical model development, particularly in atomic physics. Critical topical areas for ICF/HED science are transport physics (particularly thermal conductivities) in warm dense matter and radiation physics (particularly line shapes and non-LTE kinetics) in hot plasma; both play major roles in energy transport and instability development. Explore ways to increase university access to large facilities. Sandia's "Z Astrophysical Plasma Properties" program is an exemplary model that engages both university researchers (including promising students) and the astrophysical community. Explore ways to broaden the user base of lab codes.

Reviewer Report: Richard W. Lee

TO:	Keith LeChien,	ICF Director
	Lois Buitano,	Group 1 HQ Lead
	Njema Frazier,	Group 2 HQ Lead
	Kirk Levedahl,	Group 3 HQ Lead

I. Underlying physics understanding and integration.

EOS

Findings: The discussion of the equation of state modeling indicated that this field is, with respect to the wider scientific community, being developed in an efficient manner. The codes that have been and are being developed seem to provide reasonably accurate results in agreement with the experiments carried out at several facilities in the NNSA complex. The work that is largely dominated by matter shocked along the Hugoniot is well in hand.

Comments: My view of the status would be that it is slightly subtler than the Findings overview. High pressure is not actually that well known: you have a choice - static experiments, for example Diamond Anvil Cells (DAC), which are limited in pressure, along an isotherm, or dynamic, which gives data along the Hugoniot. Nature allows you to do that by responding appropriately. It could be said that because we do not have *independent* temperature measurements yet, one does not actually have benchmark high pressure data except along those two particular paths. To put this in perspective one would have to agree that this situation is much better than, EOS's counterpart, i.e., Opacity data, but only because nature is helping you. The big challenge will come when one attempts to go off-Hugoniot and find a method to measure the temperature,

Recommendations: I would strongly recommend that the EOS effort be encouraged to continue pretty much without interference. As the editor of the special topic journal "High Energy Density Physics" I get to see contribution from the NNSA complex and all the other laboratories doing work in this area. It is quite clear that at this time the EOS effort made here are amongst the leading efforts. Further, the level of collaboration in both theory and experiment (including instrumentation, data analysis, etc.) is impressive. This openness should be encouraged – possibly by leaving these researchers to do their job.

Opacity and transport

Findings: The situation for Opacity and Transport is definitely distinct from that of EOS. I note that the scientific issue is Opacity, which once known, would be used in radiative transfer codes to provide a description of the radiation field intensity. So, I deal here with opacity. In current practice, from my understanding in the discussions, the last thing the code developers want to put into their codes is an accurate model of non-LTE population kinetics, which I am pretty sure dominates the ionization in virtually all laser-plasma experiments. This is understandable as the number of levels can quickly become intractable for high- Z plasmas for any current computer. On the other hand, this does not excuse the lack of effort put into this area when it comes to experiments. [Okay, I admit that this is the field I have worked in roughly forever.] The trouble has been that there are only a few benchmarks in the field of Opacity.

Comments: I don't think that radiative properties have been hard to benchmark because the processes involved are on the microscopic –or atomic– scale but because there have been no methods of creating an appropriately uniform volumetrically heated sample, and one has the complication for opacity of requiring a separate source and target laser. In contrast for the isotherm for high pressure DACs you don't heat, and for the Hugoniot, nature conveniently does it for you, lapping up that entropy you are producing, and only by definition digests exactly the right amount to reach the 'correct' state. For radiative properties, i.e., line shapes, population kinetics, collision physics, etc., there have been several attempts over the last 35 years to create bench mark data for laser-plasmas and the difficulties arising from the plasma gradient structure have thwarted these efforts. That obtaining a benchmark is central to the advancement of the field can be evidenced by the continued use of the plasma benchmark we have from W.L. Weise, D.E. Kelleher, D.R. Paquette, (Phys. Rev. A6, 1132

(1972)) which was a long time ago. Indeed, using this data it has been recently determined in a comparison of line shape theories, that one of them is incorrect. Without a benchmark the true nature of the problem would remain uncertain.

The second benchmark comes from the use of the X-ray Free Electron Laser at SLAC's LCLS. This data was taken in an x-ray heated aluminum sample which has virtually no thermal gradients. The data was used to determine the incorrectness of the standard Ionization Potential Depression, which has been used since the 1960's and was chosen for the APS Division of Plasma Physics Dawson prize this year. This effort was led by an international team from Oxford, LLNL, LCLS, European XFEL, DESY, STFC (UK), UCB, LBNL.

Recommendations: I believe that the efforts in the area of Opacity have been extremely well served by the work being performed at Sandia. The group there arguably has the best assemblage of researchers in HED plasma spectroscopy in the world and has provided a new impetus for the field by making measurements that are broadly interesting to the scientific community. This effort should be maintained and further encouraged.

Further, I think the X-ray FELs are unique and can be important as they allow researchers, for the first time, to decouple volumetric heating from the probing of the plasma. That is to say one could argue along the lines of the x-ray FEL providing the perfect probe. All the success of the data on Al, Si, Mg etc. has arisen because the x-ray FEL is producing core holes, to allow emission at wavelengths that are not emitted thermally, even though the system is hot. While this gives one a huge parameter space to explore, we assume researchers will need to rethink the X-ray FEL experimental designs once they attempt to interrogate a system at a wavelength where it is emitting thermally, and not just owing to the core holes generated. X-ray FELs will provide the possibility of data taking on the 10fs time scale with a probe tunable to greater than 10 keV.

Hydro and burn physics

Findings: The performance of the hydro codes associated with the ICF complex has been continuously impressive. The reported results though when compared to the results of experiments is less accurate than one would have hoped for. This is exceedingly troubling. Further, again probably due to my lack of detailed knowledge of what was being done in support of ICF, the reliance on 1-D models without burn etc. makes no sense, as the addition of burn seems to have worsened the agree between the experiments and the simulations. This is difficult to grasp, much less understand.

Comments: The number of efforts being pursued to understand the complex behavior of the HED plasma generated in the ICF experiments seems to me to be an indication that the pursuit of indirect laser-driven fusion is not scientifically ripe at this time and will require high quality experimental results that can be used to benchmark the codes. The codes that need to be developed would include all the disciplines that have been discussed in the NNSA Laboratories' reports. The list is long and provides an outline of all the areas of physics that require concerted effort before one could hope to have a complete simulation capability that can be relied upon to obtain valid prediction.

Recommendations: I believe that the best one can do is to take the lead by enlisting as many high quality researchers as possible to take on the challenges of the ICF program. Room may be in the budget to develop a set of Academic Centers of Excellence tin HED Science to aggressively develop both experimental and theoretical approaches to find the solutions to the outstanding issues.

Global and driver physics: (I assume the Global physics addresses the experimental-scale performance of the codes, including integration of the atomic- and micro-scale physics

Findings: It is clear the there are many gaps in the necessary knowledge base needed to pursue the calculation of the physical processes required to understand ICF/HED.

Comments: The concepts that require addressing are numerous and were well outlined in the discussions and white papers provided by the laboratories. However, there are many researchers worldwide working on the same or similar problems. The global ensemble is large but the inability of the group to effect improvements in the simulations could arise from the need to adhere to the program plan, which in hindsight was well off the mark.

Recommendations: Figure out how to redirect some of the funds appropriated for laser-driven fusion to Centers of Excellence in HED science which could be focused on the larger view of HED science including aspects of astrophysics, planetary physics, the microphysics that can be accessed by the x-ray FELs. This would provide a substantial increase in collaborations and manpower working on the central problems related to ICF/HEDS.

II. Partnerships with external entities

Community: Codes

Findings: The codes developed within the NNSA complex have been well received scientifically and remain at the center of the ICF/HED field. The work previously performed to incorporate improvements in the codes seems responsive to the programmatic needs. On the other hand, it is clear that servicing those needs was not sufficient to improve the predictive capability of the simulations

Comments: At the risk of being repetitive, I do not have a way to explain the many areas where the codes need improvement, but were still used to support the program plan. This is unfathomable to me.

Recommendations: The best one can do in a situation of this kind is to re-evaluate the influences the program planner had on those developing the codes and thus on those performing the experiments. A better organizational structure should be created to address the fact that this large scale project must have had tell-tale signs of unsatisfactory results. Yet there was no indication that corrective actions were taken given the continuous evolution of the ICF program. I apologize if my perception is incorrect due to my distance from the program's oversight.

Community: Experiments

Findings: The small facilities broadly speaking allow more rapid progress due to the obvious advantages of being less expensive to run, provide an ease of genuine continuity for training students, who through eagerness and novel views can provide much benefit to smaller scale research teams. The larger the facility, the more expensive the effort, the slower the progress. On the other hand, the larger facilities can access much wider ranges in the HED phase space than smaller facilities– the NIF clearly dominates the the largest range of the HED space and will do so for many years. Careful coordination amongst the facilities in a national program may be able to optimize the opportunities, but this form of coordination requires much effort, intelligent management, and zeal.

Comments: In the end the number and type of facilities need to be responsive to the program needs. I assume here that one needs to accommodate Indirect Drive, Direct Drive, and Magnetically driven implosions in the plan. The first two are of a kind, while the third is sufficiently different to provide a test bed to test the creation of high density, high temperature environments. In this regard there are a reasonable set of facilities to achieve this goal.

Given my sense that direct and indirect drive are quite similar I would say that there is much exchange of information and a similarity of techniques. On the other hand, magnetically-driven systems would be difficult to compare with the Laser-based ICF methods, ***unless*** these are fundamental studies where a particular quantity is extracted from the experiment – at that point the comparisons again become straightforward.

Recommendations: A coordinated effort to provide redundancy in a particular measurement has never been actively pursued. When is the last time that you remember an experiment being performed on a different platform to compare results ***because*** the different facilities have differing diagnostics and approaches toward achieving the conditions of interest? This is rarely done on mid- and large-scale facilities due the nature of competition and the resistance of management to attempt duplicative experiments. This, again, would require directions from above. Wherever that may fall in the organization.

To clarify these points: I believe that the diversity of experimental approaches is reasonable but the cross-checking is close to zero. Also, the exchange of results happens within DoE NNSA as it does outside of NNSA, i.e., largely through the literature and professional meetings. I am obviously not including in this comment those topics that are not communicated in open forums.

The "User Facility" model should not be expanded within the NNSA community, as there are several highly functioning facilities However, there should be a substantial effortto incorporate other facilities aggressively into the HEDP arena. This would be a cost-effective way to benefit the larger international community, as HEDP has numerous hard-to-solve problems and could become a focus of a larger scale efforts. The newer capabilities, e.g., petawatt lasers generating relativistic electrons and other extreme conditions, high energy Swift Heavy Ion sources, and sub-picosecond, intense hard x-ray free electron lasers, which are being built in various places around the world can make an important contribution to the longer term success of ICF through a deeper understanding of HED science. These facilities have nascent HEDP efforts, which would be supported and could be excellent partners with the ability to recruit high-quality students and provide benchmark data.

Following on from the above, the construction of numerous facilities is ongoing around the world, tapping into this vast resource base would be a far more effective way to resolve the potential problem of facility redundancy, provide for an enhanced interest in HEDP together with a much expanded work force.

Community: Collaboration

Findings: The health of the laboratories' scientific efforts is enhanced by the numerous collaborations that have been developed over many years. These collaborations are important as they allow researchers to interact with a wider set of their peers. These collaborations also make it possible to perform experiments on other facilities around the world, which requires a great deal of interaction, coordination and much data.

Comments: The extension of the collaborations with researchers outside of the NNSA complex should be encouraged. It is a cost effective way to expand the data being generated and has the advantage of bringing novel techniques, new researchers and their idea into the system.

Recommendations: Allow collaborations to expand within the constraints of the governmental oversight. The development of new facilities around the world will provide an extended source of information for NNSA. The facilities, like LCLS at SLAC operate on the basis of the peer group proposal process which mean that the "best" proposals get beam time. This means that the cost to NNSA would be the researchers and their equipment. Thus the "User Facility" model should not be expanded within the NNSA community, but there should be a substantial effort to incorporate other facilities aggressively into the HEDP arena. This would be a cost effective way to benefit the community, as HEDP has numerous hard-to-solve problems and could become source of a new focal point for HED science. The newer capabilities, e.g., petawatt lasers generating relativistic electrons and other extreme conditions, high energy Swift heavy ion sources, and sub-picosecond, intense hard x-ray Free electron lasers, which are being built in various places around the world. These facilities have nascent HEDP efforts, which should be supported and could be excellent partners with the ability to recruit high-quality students and provide benchmark data.

Reviewer Report: John Sarrao

General Comments

In general I found the review to be quite stimulating. There is an energy and a spirit of collaboration within the ICF/HED community that is much more evident than it was just a few years ago. This is dramatically positive. At the highest level, my advice is keep doing what we're doing. I don't get the sense that ignition or some other field-changing breakthrough is at the tips of our fingers, and therefore a near-term, budget increase in order to 'surge' to the finish line seems premature. On the other hand, it would seem to be a significant mistake to dial back the effort now, given the current momentum and future high potential for impact. As discussed specifically above, there was some variability by subfields in the focus/excitement of the respective communities. In general, those areas that were focused on particular microphysics topics were most energetic, especially when a significant fraction of the leadership was now from beyond LLNL. This is not to say that Livermore scientists are not doing great science, rather it's just the opposite – success would not be coming if not through LLNL leadership; however, a broader community is key for the competition of ideas that drives good science. On the other hand, the sessions that focused more on integrated codes and performance were less stimulating and rather more LLNL dominated, presumably due to e.g., the large code base that is closely held by LLNL. Continuing to grow and diversity the community and to move the intellectual center of mass beyond Livermore remains an important opportunity and challenge.

It was quite valuable to spend a day at SLAC. The cultural differences between SLAC and LLNL were obvious, even when LCLS has a relatively low 'shot' rate and its campaign approach is relatively similar to NIF. Again, keeping the shot rate up at NIF and ensuring beam time availability at LCLS will do wonders in stimulating the community for future success. Further, the geographic proximity of SLAC and LLNL provides an opportunity to enhance Bay Area leadership in HED science, spanning SC and DP and including broader elements of the community, including at Berkeley.

I. Underlying physics understanding and integration.

EOS

Findings: The community is benefiting from a new generation of experimental tools, both in terms of what quantities are measured and the extremes in which these measurements occur. The state of theory and computing feels solid and is pushing itself into new regimes. This is an emerging area of science in which we know we know less than we did 5 years ago, which is a positive sign for future breakthroughs that will not only advance the frontiers of science but also directly impact NNSA mission needs.

Comments: A key opportunity is cross-platform and cross-diagnostic comparisons. The community is now acquiring enough data that discriminating tests in regions of interest become possible, in contrast to single, isolated measurements. More data will surely lead to more internal inconsistencies; resolving these apparent conflicts will impact positively our predictive capability. An important area of focus will be bridging atomistic micro-physics and integrated hydrodynamics.

Recommendations: Continue to invest in and pursue the articulated diverse science strategy, including synergistic opportunities enabled by LCLS as well as NIF.

Opacity and transport

Findings: While this has been and continues to be an important area of science, the community seems to have less 'vision' than other communities. The staffing pipeline seems challenging here because there are not a lot of 'near neighbor' disciplines with which to engage.

Comments: The community would benefit from some 'fresh blood' leadership and a forward-leaning strategic plan.

Recommendations: Working to grow the number of university partners and new contributors to the field should be a community priority.

Hydro and burn physics

Findings: This is an important area of science, but the community seemed overly focused on hydro growth and its impact on implosion/ignition, and not the underlying general coupled multi-physics problem. New diagnostics coupled with more sophisticated models create an opportunity to pursue previously unresolved fundamental questions.

Comments: To the non-expert, it's not clear if there is a systematic approach to enhanced predictive understanding in the community or if it's limited to an approach along the lines of pick your favorite candidate uncertainty and work to reduce it.

Recommendations: Ensure that the community embraces a vision of not only better thermonuclear burn but also a broader understanding of instability physics.

Global and driver physics

Findings: The academic community interested in LPI seems to have shrunk to a level that may be unsustainable.

Recommendations: As the community moves beyond on a singular focus on ignition, the opportunity to pursue 'crazy' ideas should be enhanced in order to challenge understanding and to test/train designers.

II. Partnerships with external entities

Community: codes

Findings: Barriers continue to exist, especially for integrated codes, in providing the best available codes to the academic community. The arrival of exascale computing will present both opportunities and challenges for this community.

Comments: Integrated codes are likely to remain the domain of the labs in general and LLNL in particular, but further growth in university-led micro-physics codes would be valuable.

Recommendations: Work to find ways to leverage the PSAAP centers in diversifying and expanding the suite of integrated and single-physics codes

Community: Experiments

Findings: Opportunities exist to bridge the "LCLS culture" and the "NIF culture." Success will enhance the visibility and foster the growth of the ICF and HED communities.

Comments: Target fabrication remains a challenge for the academic community which needs to be addressed to ensure the most effective scientific agenda.

Recommendations: As the LCLS- and NIF-based communities continue to grow, direct funding support is needed to foster the emergence of university groups until they reach a state of maturity similar to the materials-synchrotron community in which other funding modalities are possible. It is also important to continue support for staff scientists at the facilities to foster and grow new members of the community and to facilitate their initial success.

Community: Collaborations

Findings: HED science would seem to be a frontier for the interface between Office of Science and NNSA, and synergistic partnerships should be actively pursued.

Comments: A number of collaboration and funding models are currently being used, e.g., by Omega, Z, and NIF, as well as LCLS. At the moment, maintaining this diversity (rather than picking a particular answer) is probably healthy for the community

Recommendations: Given the significant change that has come to the HED community in the last several years with the emergence of LCLS and the dramatic changes in NIF shot rate, one should be patient in not forming judgments about the community too quickly.

Reviewer Report: George Zimmerman

I. Underlying physics understanding and integration.

EOS

Findings: Besides thermodynamic quantities as a function of density and temperature, HED experiments and modeling should explore the time variable (phase transitions) and also effects on EOS due to departures from thermal equilibrium such as unequal electron and ion temperatures.

Comments: Most current modeling assumes that pressure and energy can be specified as a function of temperature, density and composition in equilibrium. A multiphase EOS has the potential to include time dependent phase information but needs to be supplemented with the appropriate transition rate data. Strength models are usually inconsistent with the EOS and do not provide for a time dependent loss in strength. HED requires the EOS of materials with unequal electron and ion temperatures, yet we assume that the pressure is Pe(ρ,Te)+Pi(ρ,Ti) without considering, for example, the screening effects of the ion temperature on the electron pressure. Non-LTE radiative models required to simulated hohlraums and doped fuels provide only very crude EOS quantities which do not agree with equilibrium models when they should. Dependence on simplistic concepts, such as degree of ionization, should be reduced in favor of a more fundamental modeling approach.

Recommendations: Small scale experiments (with high data rates) and related modeling efforts should be encouraged to extend EOS models into these non-equilibrium situations. This is an opportunity to get academic engagement and workforce development. Goals should be experimentally validated non-equilibrium EOS models which agree with the equilibrium EOS when appropriate.

Opacity and transport

Findings: LTE opacity models agree with each other much better than they agree with experiment. There is a severe shortage of young talent in opacity modeling coming into the labs. Plasma transport coefficients and non-LTE atomic physics are essentially not measured at all.

Comments: The recent SNL measurement of the opacity of Fe is ~2x that calculated by our best opacity models. There may be something wrong with this experiment, but if not, we will need to seriously rethink these models. Measuring non-LTE opacity (both emission and absorption) and plasma transport coefficients (thermal conductivity, electron-ion coupling, and stopping power) in a uniform plasma of known temperature are very difficult experiments. They may always be integrated experiments, better suited to validating theoretical models then providing fundamentally independent data. Non-LTE opacity generation in hydro code simulations is already expensive and will become much more so before the non-LTE modeling will reduce to the best LTE opacities when they should.

Recommendations: Repeat the SNL Fe opacity measurement on NIF varying parameters to capture trends with T, ρ and Z if possible. Extend simulations of plasma transport coefficients to include mixtures of elements. Research methods to approximately tabulate non-LTE effects as well as develop non-LTE algorithms on future GPU architectures. Strive to establish consistency in the modeling for EOS, opacity and transport coefficients.

Hydro and burn physics

Findings: In layered implosion simulations all unstable wavelengths can be resolved on the mesh, but LES and RANS models still need to be in HED codes to simulate other capsules such as the CDMixCap series. These models do not yet get all three DD, TT and DT reaction totals correct. Most capsule simulations are done in 2D using diffusive energy transport and without self-generated magnetic fields. Experiments focused on hydro instability growth are well matched by simulations.

Comments: Magnetic fields affect electron thermal conduction which affects plasma density gradients which affect hydrodynamic instability growth rates. We may know all the equations, but routinely running 3D simulations at adequate resolution including magnetic fields is a tall order. Toss into that the possible need for transport instead of flux-limited diffusion and you have a truly grand challenge problem. The validity of a single fluid hydrodynamic model is questionable in low density hohlraums and exploding pusher targets.

Recommendations: Incorporate magnetic fields in simulations as soon as they can be done robustly. Perform a small number of 3D “kitchen sink” simulations in which all known asymmetries are included. Look into slightly extending hydrodynamic (and MHD) simulations toward longer ion mean-free-paths by incorporating ion species diffusion/separation, multiple fluids, multiple ion temperatures and/or pressure tensor evolution, benchmarking against Fokker-Planck and LSP simulations.

Global and driver physics

Findings: LPI is a classic multiscale problem requiring subzone models for backscatter and cross beam energy transfer. If we cannot get around LPI then it may be important in all three: direct drive, indirect drive and MagLIF. Measuring the plasma conditions, rather than relying on hydro simulations, might help clarify the role of LPI. Large circuit models for Z do not yet model the load current well. Experimentalists spend 90% of their time setting up shots and 10% understanding results.

Comments: If simple circuit models for Z can model the load current, then large (many element) models should be able to as well. Perhaps we can understand and fix this problem during the MHD modeling workshop August 24-26, 2015 at LLNL. We must understand expensive experiments at better than the 10% level. Getting more students involvement might be an answer.

Recommendations: Continue to develop Thomson scattering measurements of hohlraum plasma conditions. If possible measure backscatter with <10 ps time resolution to provide additional information to LPI modelers.

Appendix C Supporting Documents

C.1 Directors' Letter

Lawrence Livermore National Laboratory   Los Alamos NATIONAL LABORATORY   Sandia National Laboratories

January 20, 2015

The Honorable Frank G. Klotz
Under Secretary
National Nuclear Security Administration
United States Department of Energy
Forrestal Building 7A-049
1000 Independence Avenue SW
Washington, DC 20585-3430

Dear Under Secretary Klotz:

The overwhelming majority of the yield of the Nation's nuclear weapons is generated when the conditions within the nuclear explosive package are in the high energy density (HED) state. This requires that proficiency in HED science remains a core technical competency for the Nation's Stockpile Stewardship Program (SSP) for the foreseeable future. As we enter the third decade since the cessation of nuclear testing, the HED program in the United States is at an important juncture where we must reassess the appropriate balance between pursuit of ignition and the other uses of HED research in stewardship. In response, we are developing a more coordinated approach across the major national HED efforts to ensure the long term viability of the SSP.

The NNSA has developed three cutting edge experimental capabilities as part of the Inertial Confinement Fusion (ICF) Program to enable access to HED regimes relevant to nuclear weapons: the Omega and Omega EP lasers at the University of Rochester Laboratory for Laser Energetics, the Z pulsed power facility at Sandia National Laboratories, and the National Ignition Facility (NIF) at the Lawrence Livermore National Laboratory (LLNL). These investments have already enabled critical contributions to the sustainment of the nation's nuclear stockpile. A more coordinated national HED effort will enhance our ability to sustain our nuclear stockpile, attract a new generation of stockpile stewards, and protect the U.S. undisputed world leadership in HED science.

With these objectives as guiding principles, we met with key technical leaders in early December to discuss the future coordination of our national ICF/HED efforts. A clear consensus emerged at this meeting that HED science, and more specifically the pursuit of fusion yield in the laboratory, is critical for the long-term health of the stockpile stewardship program. At this December meeting, we committed to work together to bring forward an integrated National program plan that will enable the long-term sustainment of this essential HED capability.

The U.S. nuclear weapons laboratories are fully committed to building and maintaining HED capabilities that support key NNSA mission drivers:

Recycle
7600-81230

- Sustaining and modernizing the stockpile
- Qualifying systems and components for new threat environments
- Avoiding technological surprise
- Recruiting, training, testing, and retaining technical staff
- Assessing nuclear designs without a return to nuclear testing

In the absence of new nuclear tests and with the attrition of nuclear test experience, looking forward the nuclear weapons laboratories will need the ability to (1) test nuclear designers in high energy density (HED) experimental design, (2) access material pressure and density regimes that are presently inaccessible to other experimental techniques, (3) generate and utilize thermonuclear burning plasmas, (4) develop commensurate high-fidelity diagnostics and experimental platforms that help to assure our weapons are safe, secure, and effective, and ultimately, (5) create and apply multi-megajoule fusion yields to enable enduring stockpile stewardship.

It is our view that the U.S. must continue to strive to be the first nation to demonstrate ignition and high yield in the laboratory. This goal is important not only because of its support for the SSP but also to send a strong signal to others regarding our Nation's scientific and technical capabilities. NNSA presently has three credible research approaches to demonstrating laboratory ignition and high fusion yield and addressing the technical challenges outlined above: x-ray drive, direct laser drive, and magnetic drive. Continued leadership in HED science is and will continue to be an essential component of a coordinated and balanced sustainable national nuclear security enterprise. We and our delegates will be meeting regularly in 2015 to ensure progress towards this integrated and coordinated National HED effort, and we look forward to a continuing dialogue with NNSA leadership on the importance of this effort.

Sincerely,

Dr. William H. Goldstein
Director, Lawrence Livermore National Laboratory

Dr. Charles F. McMillan
Director, Los Alamos National Laboratory

Dr. Paul J. Hommert
Director, Sandia National Laboratories

C.2 Summary of Previous Reviews

It was not an objective of this study to evaluate the importance or the necessity of an ICF/HED program, although this has been extensively studied over the past 30 years. Below is a list of recent studies for reference:

1986 – Review of the Department of Energy's Inertial Confinement Fusion Program, National Academy of Sciences.

1989 – Laboratory Microfusion Capability Study Phase I, Department of Energy (DOE/DP-0069).

1990 – Second Review of the Department of Energy's Inertial Confinement Fusion Program, National Academy of Sciences.

1990 – Performance of Participants in DOE's Inertial Confinement Fusion Program, Government Accountability Office (GAO/RCED-90-113BR).

1990 – The Nike Laser Program at the Naval Research Laboratory, Department of Energy.

1990 – Fusion Policy Advisory Committee, Department of Energy.

1992 – The Nike and Mercury Programs, Department of Energy.

1993 – Laboratory Microfusion Capability Study Phase II, Department of Energy (DOE/DP-00 1 7).

1993 – NIF Justification of Mission Need, Department of Energy.

1994 – Review of Science Based Stockpile Stewardship, JASON Committee (JSR-94-345).

1994 – Independent Cost Estimate – The National Ignition Facility Conceptual Design, Foster Wheeler USA (DOE Contract No. DE-ACO 1-94PR I 00 1 6).

1994 – Approval of Key Decision One for the NIF, Department of Energy.

1995 – The NIF and the Issue of Nonproliferation, Department of Energy.

1996 – Inertial Confinement Fusion Review, JASON Committee (JSR-96-300).

1996 – NIF Title I Design Review, Department of Energy.

1996 – Programmatic Environmental Impact Review for Stockpile Stewardship and Management, Department of Energy (DOE/EIS-0236).

1997 – Review of the Department of Energy's Inertial Confinement Fusion Program – The National Ignition Facility, National Academy of Sciences.

1997 – Independent Cost Estimate – The National Ignition Facility Title I Design, Foster Wheeler USA

2000 – National Ignition Facility: Management and Oversight Failures Caused Major Cost Overruns and Schedule Delays, Government Accountability Office (GAO/RCED-00-271).

2000 – Final Report of the NIF Laser System Task Force, Secretary of Energy Advisory Board.

2001 – High-Energy-Density Physics Study Report, National Nuclear Security Administration.

2003 – Frontiers in High Energy Density Physics, National Academy of Sciences.

2005 – Preliminary Results of Review of Campaigns to Provide Scientific Support for the Stockpile Stewardship Program, Government Accountability Office (GAO-05-636R).

2005 – Assessment of the plan and prospects for achieving ICF ignition at the NIF by 2010, JASON Committee (JSR-05-340).

2005 – Preliminary Results of Review of Campaigns to Provide Scientific Support for the Stockpile Stewardship Program

2009 – Addendum: Policy and Scientific Issues for Consideration by Expert Advisory Panels, JASON Committee (JSR-09-330).

2010 – Actions Needed to Address Scientific and Technical Challenges and Management Weaknesses at the National Ignition Facility, Government Accountability Office (GAO-10-488).

2010 – 2012 – Quarterly Reviews of the National Ignition Campaign (The Koonin reviews).

2012 – Basic Research Directions for User Science at the National Ignition Facility, Office of Science.

2012 – Science of Fusion Ignition on NIF (San Ramon Report), National Nuclear Security Administration.

C.3 Milestones from the 2012 Path Forward

	Ignition Platform	Suggested milestone	Completion Criteria	Status/ Update
1	All	For all fusion approaches, define the plan and specific goals for scientific and technological activities to be performed in preparation for the FY 2015 review.	For all approaches, identify and document the detailed experimental, computational, technology development, and other activities required to be performed in preparation for the FY 2015 review. For PDD, this will include an assessment in FY 2013 from both the target physics and technology perspective. Based on this assessment, NNSA, LLE and LLNL will define a set of PDD tasks consistent with planned budgets and priorities.	Completed
2	All	Review results of all three ignition approaches (LID, PDD, MDD).	Review progress of all fusion approaches with respect to the program plan defined at end of FY 2013 and out-year plans for ICF and high yield platforms.	Completed
3	LID	Conduct experiments designed to examine scientific and implosion performance issues identified during the NIC campaign.	This milestone will include a campaign of experiments to look at symmetry and mix issues and will include high-adiabat, cryogenic implosions to compare code predictions and performance.	Completed
4	LID	Review alternate x-ray drive implosion concepts including technology feasibility.	Review alternate x-ray drive implosion concepts from both a scientific and technology perspective.	Completed

5	LID	Conduct physics and integrated DT implosion experiments to examine experimental and computational understanding of capsule drive.	Assess experiments conducted to determine the level of experimental and computational understanding of capsule drive and hydrodynamic performance.	Completed
6	LID	Conduct an experimental campaign and assess agreement between models and simulation of implosion compression and pressure.	Develop and execute cryogenic gas-filled and layered DT implosions with convergence ratio > 20 as an integrated test of experiment and code performance. Measure fraction of yield due to alpha heating and report other performance parameters including DSR, Ti, velocity, and fuel shape.	Completed
7	PDD	Complete an assessment of the predicted implosion performance using the measured imprint efficiency with multi-FM smoothing by spectral dispersion.	Assess the predicted implosion performance on the NIF using the measured imprint efficiency with 1D multi-FM SSD on OMEGA EP and compare with measured and simulated implosion performance using the current laser smoothing levels on OMEGA.	Completed
8	PDD	Perform integrated PDD implosions on the NIF to investigate symmetry control and LPI mitigation.	Using current NIF capabilities, conduct PDD implosions on the NIF and compare predicted symmetry and laser energy coupling performance against simulations and OMEGA experimental results.	Completed
9	PDD	Conduct integrated cryogenic DT implosions on OMEGA to establish the predictive basis for NIF-equivalent hydro performance.	Compare computational predictions of cryogenic DT implosion performance on OMEGA against a broad spectrum of design parameters and investigate discrepancies in the computational models.	Completed

10	PDD	Conduct in-depth external review of PDD point design to assess go-forward program and readiness for CD-1	Includes completion of CD-0. Reference definitions of CD-0 and CD-1. Scope and commitments need to be clear and consistent with budgets.	Completed
11	MDD	Demonstrate initial capability for magnetized and pre-heated fusion experiments.	Commission initial capability at Z to simultaneously magnetize and preheat cylindrical fusion targets on Z with requirements of initial B = 7-10 Tesla and initial laser pre-heat energy = 1.5–2 kJ. Determine the impact of the magnetic field on current coupling to the target. Conduct experiments with magnetization and pre-heat separately and together.	Completed
12	MDD	Conduct initial integrated fusion target experiments and compare results to simulations.	Determine fusion plasma parameters at initial levels of pre-heat, magnetic fields, and drive currents. Apply initial methods to measure the efficacy of flux compressions by the imploding liner. Compare results to simulations.	Completed
13	MDD	Evaluate fusion performance and stagnation plasma parameters at enhanced drive conditions and compare results with simulations.	Increase magnetic field, laser pre-heat, and drive current capability. Requirements are B > 20 T, laser pre-heat > 4 kJ, drive current > 22 MA. Conduct experiments to measure the stagnation plasma parameters and fusion target performance for all platforms. Compare results to simulations and quantify agreement.	Completed
14	Diagnostics	Demonstrate operation of Advanced Radiographic Capability (ARC) at NIF using one NIF beam.	Complete installation of ARC equipment for one NIF beam and demonstrate ARC is operational.	Completed

C.4 Major Accomplishments in the ICF/HED Science Portfolio from 2012 – 2015

FY 2012

- A NIF layered cryogenic target implosion produced a record neutron yield of $7\text{x}10^{14}$ with a 3.7 keV ion temperature.
- The Omega laser facility performed its 20,000th target shot.
- Proof of principal experiments for future NIF platforms to study radiation transport were demonstrated on the Z Facility.
- The Argon gas puff was developed and fielded on the Z Facility enabling a new class of sources for radiation effects to assess nuclear survivability of components.
- High pressure tantalum strength experiments on Z measured higher yield strength than predicted by most existing theoretical models, with some experiments indicating higher sensitivity to microstructure at Z strain rates than was expected.
- LANL conducted a set of colliding shock/shear experiments at Omega to study turbulence and mix.

FY 2013

- Completed operational qualification of the first set of ignition regime-relevant diagnostics and installed capabilities to support cryogenic target implosions on the National Ignition Facility (NIF).
- A record peak pressure exceeding 50 Mbar was demonstrated in an isentropic compression of carbon experiment on the NIF.
- Completed first set of NIF experiments to tune and control the shape, implosion velocity, compressed fuel density, and mix of implosions and demonstrated layered tritium-hydrogen-deuterium (THD) and deuterium-tritium (DT) cryogenic layered implosions.
- A record yield of $2\text{x}10^{13}$ was obtained from a direct drive cryogenic implosion on Omega using new polar drive phase plates.
- Demonstrated improved hydrodynamic efficiency in direct-drive implosions on Omega using beryllium ablators and quantitatively showed the effects of A/Z (atomic mass/charge) on the conversion of absorbed laser power into shell kinetic energy.
- A load current record of 26.4 Mega-Amperes (MA) was achieved on Z, and routine operation at 85 Kilovolts (kV) Marx charge was demonstrated, allowing higher energy densities to be obtained routinely on Z.
- Experiments on Z provided comprehensive characterization of materials relevant to development of multi-point-safety options for the stockpile.
- After completing Omega experiments to develop he platform, the LANL shock-shear platform was moved to NIF to study mix.

- NIF experiments were performed in the Pleiades collaboration between AWE and LANL for measuring radiation transport through a silicon aerogel foam. Spectral data of the burn-through was obtained.
- Experiments were performed on Omega EP to develop an HED platform to infer equations of state by measuring target release.
- SNL completed a comprehensive study of foam under shock compression. Foams are widely used to protect components in high impact applications.

FY 2014

- The NIF high-foot campaign delivered a record neutron yield of $9.5x10^{15}$.
- A new experimental platform validated the improved hydrodynamic stability of the high foot design relative to a NIC target, dispelling a hypothesis that 5X larger than predicted hydrodynamic instability growth rates were responsible for capsule degradation during the NIC.
- NIF experiments on the ramp compression of diamond are the cover article for the July 18 edition of Nature.
- The first beryllium capsule experiment was successfully completed on August 30 on the NIF.
- First highly resolved full sphere 3D ICF capsule simulation capability was demonstrated that enabled detailed post-shot modeling, eventually leading to calculations that reproduced some aspects of the capsule degradation observed during the NIC.
- LLE validated implosion energetics modeling using backlit radiography and ablation front shadowgraphy using Polar Direct-Drive implosions on the NIF.
- SNL, in collaboration with LANL, executed the 14th plutonium (Pu) experiment on Z, utilizing an innovative load design to reach 80% higher pressures in shockless compression then previously attained.
- Joint Sandia/LANL opacity experiments on Z, with multiple materials and two temperature/density conditions, revealed significant discrepancies between most atomic models and the data. Tests to understand the experimental platform over several years have not yet revealed any flaws in the experimental methodology.
- Magnetized Liner Inertial Fusion (MagLIF) first demonstrated thermonuclear neutron production and fuel magnetization, opening a broad space of potential ICF designs.
- A NIF experiment in the complex hydrodynamics campaign was completed measuring symmetry of the shock wave produced in a copper foam ablator in a vacuum hohlraum.
- A LLNL, SNL, AWE collaboration performed shots to measure system generated electromagnetic pulses (SGEMP) produced by a laser-generated x-ray source on NIF.

FY 2015

- Observed the lattice structure of plutonium under extreme pressure and temperature conditions on the NIF.
- Safely executed the 17th plutonium experiment on Z to study shockless loading at low pressure in order to span the full equation-of-state phase space.
- Increased the number of shots on NIF from 191 in FY2014 to 356 in FY2015 with fixed funding.
- Achieved the 25,000 shot on Omega, a seminal achievement in the facility's 45 year history.
- The detailed analysis of the first fully-integrated Magnetized Liner Inertial Fusion (MagLIF) experiments on Z were published and the initial results were reproduced.
- LLE made the first direct measurements of the conduction zone length and mass ablation rate in direct-drive implosions.
- Achieved record 50-gigabar pressures in capsule implosions on the Omega Laser. These capsules are designed to inform future decisions for facility investments at the NIF.
- Identified the two major causes that have limited performance of NIF implosions; capsule drive asymmetry and capsule support engineered features.
- Executed the first experiments for a campaign on NIF to characterize the measured radiation transport in SSP relevant regimes. Obtained excellent results for validating ASC models.
- NIF completed a four-shot radiation transport campaign to study Marshak wave propagation in complex configurations.
- LLE performed absolute EOS measurements of foam targets using OMEGA EP x-ray radiography.
- Executed Z shots to develop neutron sources and study neutron radiation effects in semiconductors.

C.5 Summary of First National Implosion Stagnation Physics Working Group (NISP) Meeting

A summary of the first NISP workshop, Oct 27-28, 2015

1. Agenda:

Tuesday, October 27, 2015, B481, Room 2005

8:00am	Gathering and refreshments	
8:30am	Welcome and opening remarks	K. LeChien
8:40am	Charter and deliverables	J. Frenje/S. Regan
9:00am	MDI	K. Peterson (discussion lead)
12:30pm	Lunch (will be provided)	
13:30pm	DDI	R. Bahukutumbi (discussion lead)
16.00pm	Day 1 wrap-up discussions	All

Wednesday, October 28, 2015, B481, Room 2005

8:00am	Gathering and refreshments	
8:30am	IDI	P. Patel (discussion lead)
12:00pm	Lunch (will be provided)	
13:00pm	Workshop wrap-up discussions	All
15:00pm	Generate report out	All

Attendees:

A. Schmitt, S. Velicovich, R. Bahukutumbi, J. Knauer, V. Goncharov (web), A. Simakov, T. Murphy, M. Gomez, P. Knapp, K. Peterson, B. Spears, P. Patel, T. Ma, A. Pak, I. Nobuhiko, D. Casey, C. Yeamans, R. Mancini, B. Appelbe, J. Chittenden, M. Gatu-Johnson, W. Hsing, J. Kilkenny, J. Edwards, B. Sims, K. Levedahl, K. LeChien, S. Regan and J. Frenje.

Summary of the Magnetic-Drive Session

Sandia's highest priority with the MagLIF Program is to develop a well understood and repeatable preheating platform. Currently, the initial conditions of the preheated fuel and the observed conditions at burn and stagnation are not well understood. At peak burn, an electron temperature of 2-4 keV, a density of ~0.1 to 0.5g/cc, a burn duration of 1-2 ns, a height of 5-10 mm, a width of 50-100 um, a B-field of 5-15 kT (at stagnation), a peak liner velocity of 70-100 km/s are observed, which correspond to an inferred stagnation pressure of ~1 GBar, whereas clean 2D simulated values predict values of 2.2 GBar assuming optimal laser energy coupling to the fuel.

Magnetic Drive: Hypotheses for the Observations

Several hypotheses explaining the observations and discrepancies to modeling were discussed. These are:

- Low laser energy is coupled to the fuel, with little to no mix of the liner/endcap/window. The implosion is also essentially 1D (accounting for end losses) in nature at stagnation.

Simulations of the early MagLIF experiments explain the observables assuming only ~200 J (10%) of laser energy couples to the fuel. This hypothesis may explain the initial integrated experiments that used relatively thick (3 μm) LEH windows.

- A moderate amount of laser energy (~50%) is coupled to the fuel, while a few percent of the liner/endcap/window material is mixed into the fuel. The observables can be described by near-1D (accounting for end losses) modeling of the implosions at stagnation. This may explain integrated MagLIF experiments that used thinner (1.5 μm) LEH windows where the expected laser energy transmission increased significantly, but neutron yields dropped 10×. Recent experiments have shown that integrated performance is significantly degraded with Al endcaps compared to Be, implying endcap mix is important in experiments with improved laser coupling.
- In contrast to hypotheses #1 and #2, the observed helical structure at stagnation represents a significant departure from a 1D stagnation description and results in inefficient conversion of kinetic energy, inadequate confinement, and the presence of residual flows. This hypothesis is based on 3D simulations, which can also describe experimental observables with a moderate amount of laser energy (~50%) coupled to the fuel with minimal amounts of liner/endcap/window mix.

These hypotheses were formulated with one or more of the following assumptions:

- The stagnation column is contiguous and the axial magnetic field lines follow perturbation contours at stagnation.
- Non-symmetric laser heating generates vorticity, enhanced scrape mix from liner walls and limits compression.
- Hard X-ray diagnostics are adequate surrogates for neutron burn history.
- Sufficient magneto thermal insulation has been obtained. Transport models are reasonably accurate and thermal conduction losses are not significantly degrading implosion performance.
- Axial mass/flux losses are not degrading performance more than expected from simulations and analytic estimates.

Summary of the Direct-Drive Session

A major goal with the direct-drive program at LLE is to demonstrate a well-understood and well-modeled cryogenic implosion reaching a pressure in excess of 100 Gbar. To date, the highest hot-spot pressure inferred from x-ray and nuclear diagnostics is 56±7 Gbar for an $\alpha \sim 3.3$ implosion of a layered DT target, which should be compared to the 1-D simulated value of ~90 GBar. The 1-D simulation includes cross beam energy transfer, which reduces the target absorption and resulting ablation pressure of direct-drive ICF targets. Relative to 1-D simulations. 3-D simulations suggest that low-mode distortion of the hot spot seeded by laser-drive non-uniformity and target-positioning error truncates the neutron rate and reduces the hot-spot pressure. This trend is consistent with the measured neutron rate and the hot-spot pressure. When burn truncation is taken into account the 1-D simulations for implosions with a convergence ratio CR≤17 and $\alpha \geq 3.5$ are in closer agreement with the experimental values of the hot-spot pressure and the compressed areal density. In addition, ion temperatures (Ti) measured with neutron-time-of-flight detectors, positioned in different locations around the implosion, indicate significant variations in cryogenic implosions (generally displaying ~10-14% rms, but up to 50% variations are occasionally observed), which should be compared to room-temperature implosions with a Ti variation of ~2.8% rms. In cryogenic implosions, the observed burn rate generally tracks the 1-D simulated rates but then deviates from experiment (burn truncation) prior to the 1-D bang time. To effectively address these issues, better diagnostic information (time resolved and viewed from different directions) about the compressed core including the hot spot and cold shell is required.

Direct Drive: Hypotheses for the Observations

Reasons for reduced hot-spot pressure in the cryogenic implosions include:

- Long-wavelength growth during deceleration phase that can result in low-mode hot-spot distortion, incomplete stagnation, an increased hot-spot volume, and consequently a decrease in hot-spot pressure.
- Too much mass in the vapor before deceleration.
    - Decompression of the rear surface of the shell, which could be due to 1D effects such as additional rarefaction waves, EOS, opacity uncertainties etc.
    - Short wavelength growth at the ablation surface.

The evidence for these hypotheses are: a lower inferred hot-spot pressure compared to the 1-D simulation is observed for experiments with CR≥16; 3D simulations that include the effects of beam-to-beam laser imbalance and target positioning offset (long wavelength non-uniformities) also indicate the same trend; Measured burn rate tracks 1-D simulations and then truncates prior to the 1-D simulated bang time, which is reproduced in 3-D simulations.

Summary of the Indirect-Drive Session

Discussions focused on hot-spot shape, hot-spot flow, hot-spot temperature and fuel/ablator areal density and asymmetry.

Hot-Spot Shape

Hypothesis: Hohlraum drive asymmetry, especially time-varying asymmetry, due to Cross-Beam-Energy Transfer (CBET) or wall-induced spot motion is driving asymmetric (oblate) and incomplete stagnation.

Evidence:

- Multiple line-of-sight measurements of x-rays and neutron self-emission broadly indicate that the size of x-ray and neutron images (P0) are similar on a ~10% level, but higher modes can be different.
- The variation in P2 from ConAs (shell) to hot spot emission (hot spot) appears to depend on design (gas fill and case-to-capsule ratio). The late-time shape swings are not understood and are difficult to predict due to the uncertainties in CBET in high-gas fill hohlraums, and Au wall expansion in near-vacuum hohlraums.

- Understanding the evolution of the hot-spot shape during the 'missing time window' between the 2D ConA radiography and self-emission imaging measurements will be essential.

Hot-Spot Flow

Hypothesis: An unknown source of one-sided drive asymmetry is generating hot-spot flow and incomplete stagnation. Flows from this and other drive imbalances (see 3.1) lead to incomplete stagnation and a loss of hot-spot internal energy to unconverted kinetic energy in the shell and hot spot.

Evidence:

- A similar picture of the fuel bulk motion (translational velocity) is provided by neutron and x-ray measurements. It is also observed in x-ray data that the bulk motion is function of signal contour suggesting that differential motion of brightly emitting regions within the hot spot can be determined. Source of bulk motion is not understood at this point. It is clear from simulation that this type of translational imbalance produces damaging hot-spot flow and incomplete stagnation.
- Ti measured with different neutron-time-of-flight detectors, positioned at various locations around the implosion, indicate a fairly isotropic temperature distribution (< 400 eV). However, post-shot simulations with sizable asymmetries show temperature distributions that vary about 200 eV. The data isotropy and the code-predicted level of anisotropy are not consistent.
- Using an energy balance model, the total residual kinetic energy in the dense shell and hot spot is estimated to be in the range of 70-100% and 0-30%, respectively. Uncertainties in the modeling are large, of the order of several kJs.

Hot-Spot Temperature

Hypothesis: The differences in DD and DT Ti differences and the associated DD/DT yield ratios are caused by 3D asymmetries that produce both hot-spot flows and distortions of the angular fuel areal density distribution.

Evidence:

- The observed differences in the DT and DD Brysk Ti are larger than expected for a static, equimolar, Maxwellian fluid.
- Measured DT to DD neutron yield ratio is also larger than expected on the basis of measured Brysk Ti and a simple model to account for the difference in down-scatter fractions. Fuel stratification cannot be an explanation as this would drive the ratio in the opposite direction. One hypothesis is that the simple down-scatter corrections are not accurate due to significant 3D distortions of the fuel.
- The observed electron temperatures (Te) are currently not accurate enough to evaluate the effect of bulk flows on the Brysk Ti.

Fuel/Ablator Areal Density

Hypothesis: Time-varying drive asymmetry and engineering support features are damaging the cold fuel configuration leading to large areal density variation.

Evidence:

- Time-varying, low-mode drive asymmetries and the tent perturbation are believed to be the dominant factors affecting the implosion performance. In high-foot implosions, the yield reduction due to low-mode drive asymmetries and tent perturbation is estimated to be 20× and 5×, respectively, while in low-foot implosions the yield reduction is estimated to be 5× and 10×, respectively.
- Significant areal-density asymmetries are often inferred from FNADS data. This data measures the composite effect of hot-spot neutron source distribution and out-scattering of primary neutrons by cold fuel areal density. The areal density is believed to dominate the angular variation. From fits to FNADS data, the areal density is generally much higher at the poles, resulting in 1-1.5g/cm2 asymmetries.

Summary of Hypotheses

We hypothesize that the stagnation phase of IDI implosions is compromised by non-spherical effects. The evidence suggests that both the hot spot and cold shell show 3D asymmetries. Measurements further suggest that the hot spot is incompletely stagnated and consequently suffers a reduction in internal energy due to residual flows.

Incomplete stagnation is likely due to two effects:

- Hohlraum drive asymmetry
- Perturbation from the support tent

Hohlraum drive asymmetry, especially time-varying asymmetry, is likely due to CBET or wall-induced spot motion. Drive asymmetry contains both P1 and P2 components (and likely other low modes). Further perturbations from the support tent lead to areal density variation in the confining cold fuel as well as additional damage to hot spot stagnation.

List of Action Items:

- Non-radial flow: emphasis on nTOF analysis, with peer review by LLE and LLNL. Sandia will look for precision requirements
- X-ray emission analysis: compare images and resolutions at LLE and LLNL. Are the images different (smooth, lumpy). Sandia might offer a non-spherical analysis perspective
- Compare consensus on image shapes and Ti variation. Shouldn't round images and isotropic temperatures go together?
- Scrutinize and compare current analysis of the pressure.
- Measurements of Te: Sandia, LLNL do nearly same differential filtration. Compare. Also compare to continuum spectrometry at LLE. Potentially develop a comparison with continuum and ross pairs at Omega
- Cold fuel analysis: think about cold fuel, dark region and hot spot. Can we backlight the shell, compare to hot spot?

- DD/DT yield ratios to understand scattering, species separation…
- Compare Te and Ti to understand thermal/non-thermal contributions.

1. Date and Agenda for the Second NISP Workshop

The second NISP workshop is planned to be held the week of March 7, 2016. At this workshop, action items 1-4 in Section 4 will be addressed.

Appendix: Questions/Suggestions by Panel

Magnetic Drive

- How stable is the Image plane of the ZBL laser?
- What is the radial and axial field distribution and how does this effect triton/alpha trapping?
- What effect does the observed morphology have on stagnation pressure, density, temperature, and confinement?
- How do we define the convergence ratio in the presence of a helical stagnation?
- Explore options for a burn history diagnostic on Z. Could we put a sacrificial scintillator within roughly a meter of the load to measure the burn history?
- Develop stronger collaborations with LLE and LLNL to learn potential ways to use our neutron diagnostics more effectively (better ion temperature measurements – axially resolved Ti? higher nTOF moments, Be back-scatter, environmental effects, DT spectral analysis, IRF effects, forward analysis, etc.)

Direct Drive

- Can we reduce the timing uncertainty for the NTD on OMEGA to make a better case for burn truncation as opposed to an overall reduction in neutron rate?
- Are we confident that other sources of non-uniformity (ice roughness etc.) are not responsible for the degraded performance?
- Hot spot radius is measured from a different direction compared to the areal density. So should there be consistency between the hot spot radius and areal density?
- Can a phase velocity of x-ray emission be extracted from the time-resolved hot spot images by using an indexed SEM grid instead? Or x-ray Doppler velocimetry can be used to extract RKE?
- Can fNADS be investigated for areal density anisotropy in areal density?
- Is there a correlation between Ti variations and the areal density variations between the MRS and nTOFs?
- Is there a correlation between neutron yield and Ti variations? In other words, can scattering from various fixtures etc. contribute to the Ti variations?
- Is there a correlation between CR and Ti variations?
- Is there a scenario from simulations where hot spots images including motion blurring, instrument response etc. are quasi round but there is significant Ti variations?
- Can deliberate non-uniformities such as increased power balance or increased offset be studied in cryogenic implosions?

- Since d(n,2n) results from a different portion of the compressed core relative to the n-D edge, how can you correct for that background when inferring areal density from backscattered neutrons?

Indirect Drive

- What are we learning from the calculations– 3D simulations low mode/high mode?
- What is not being done with the codes?
- What about density measurements of hot spot?
- Is a neutron temporal diagnostic possible on NIF?
- Is the Te inferred from time-integrated, Ross-filtered images measuring the same hot-spot plasma as the Ti inferred from nTOF diagnostic?
- Has a Fourier analysis of the DIXI hot-spot images been performed to study the evolution of the modal structure?
- How do magnetic fields in the hohlraum affect the drive asymmetry?
- Regarding nTOF calibration data, are hard x-rays a good surrogate for neutrons.