

**UNRESOLVED ISSUES
Related to the
NATIONAL IGNITION FACILITY**

by

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November 30, 1994

DRAFT FOR COMMENT

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Introduction

A commitment to the National Ignition Facility (NIF) beyond limited design work is premature and should not go forward unless further research and evaluation leads to favorable resolution of a number of outstanding issues. The most critical questions are:

1. Can NIF achieve ignition?
2. Can the NIF design energy of 1.8 megajoule (MJ) be achieved during routine operations?
3. Can Lawrence Livermore National Laboratory (LLNL) construct NIF for \$1.1 billion as advertised?
4. Is NIF the best facility for exploring the feasibility of inertial confinement fusion (ICF) for civil energy applications?
5. Will NIF contribute significantly to the resolution of technical issues that may arise in the future concerning the reliability or safety of the existing U.S. nuclear weapons stockpile?
6. Can the U.S. conduct a large national ICF program, with a specific emphasis on maintaining computer modeling skills relevant to the design of nuclear weapons, without continuing to generate and release data and computer codes that will enhance thermonuclear weapons research and design capabilities in threshold and non-weapons states?

None of these questions can be answered "yes" with high confidence. Rather, as indicated below, "no" or "probably not" now appears to be the more appropriate answer to each of the above questions.

1. Can ignition be achieved?

The principal design objective of NIF is to achieve ignition and moderate gain (1-10) in hohlraum targets. The design criteria, and in particular the driver energy of 1.8 MJ, were developed by extrapolating from two sources: upwards from the data provided by NOVA experiments, and downwards from the large high-gain hohlraum capsules successfully ignited in the formerly classified series of Halite-Centurion experiments using abundant X-radiation from underground nuclear tests as the "driver." However, in both cases, the target designs were significantly different from those proposed for use in NIF. There are additional significant scaling uncertainties associated with the Halite-Centurion results because the hohlraums were illuminated by x-rays from a nuclear device rather than lower-frequency monochromatic laser light, and because time dependent asymmetries were not measured.

Because the ability to achieve symmetrical compression and thus higher gain involves a complex interaction between the pulse shaping of the driver energy and the target design, there remains substantial technical uncertainty about NIF's ability to achieve ignition.

A recent report by the head of the laser fusion program at the Naval Research Laboratory [Stephen E. Bodner, "Time-Dependent Asymmetries in Laser-Fusion Hohlräume," NRL, November 11, 1994 (submitted for publication)] argues that hot plasma moving inward from the hohlraum wall could result in time dependent asymmetries at the surface of the target that could easily reach 5-10 percent during the high-power portion of the laser pulse, whereas the target can only tolerate asymmetries on the order of $\sim \pm 2$ percent if ignition is to be achieved.

To reduce the time-dependent asymmetries, NIF is designed so that the laser beams entering each end of the hohlraum are divided into two sets that can be focused independently on two "rings" of focal spots on the inside surface of the hohlraum, and the pulse power of each ring is to be shaped independently. Also, LLNL recently proposed a new hohlraum design. A helium and hydrogen gas fill has been added to the hohlraum to better control the inward motion of the hot plasma. The introduction of the gas fill, however, can also affect how and where the laser beam propagates in the hohlraum, and also the fraction of the laser energy that is converted to x-rays; and these changes can reduce symmetry and convergence of the target. To date the experiments that have been conducted with gas-filled hohlraum targets in NOVA are inconsistent with theoretical predictions, and none indicate that ignition will be achieved at 1.8 MJ in NIF.

Bodner further observes:

even if it can eventually be shown that the NIF target design is feasible, it is generally accepted that the computer models will never be sufficiently accurate to predict the laser pulse shapes for the inner and outer beams on the NIF. There has to be some way to experimentally diagnose the asymmetries with the NIF. Without a diagnostic there can be no retuning. Several diagnostics of time-dependent asymmetry have been proposed over the past few years, but none has yet been shown to be feasible during the high-power portion of the laser pulse. Diagnosing the inside of a hot hohlraum is not easy, *and it is not evident that there is any solution (emphasis added).*

Bodner concludes, "The ICF community is not yet ready to shift from basic research to the engineering and construction of this [NIF] billion dollar facility."

In a dissenting May 25 letter to the Chairman of DOE/DP's Inertial Confinement Fusion Advisory Committee (ICFAC), which endorsed proceeding into Engineering Design (KD-1) of the NIF on May 20, 1994, Dr. Timothy Coffey, Director of Research at the Naval Research Laboratory, expressed similar concerns, as follows:

I can not bring myself to believe that we have sufficient information in hand to credibly assert that an engineering design program will successfully lead to a true ignition facility. Indeed, it is quite clear that our understanding of ICF targets is rudimentary at best.

More generally, it is very worrisome that the current target design requires detailed knowledge of the time-dependent radiation asymmetry in the hohlraum in order that it may be corrected by temporal adjustments in the laser pulse. One has to worry whether or not the temporal conditions in the hohlraum can ever be determined to the extent required by this approach. The history of target design within the ICF program does not inspire great confidence that the current design will survive serious scrutiny. Consider just the volatility of target design during the relatively short lifetime of ICFAC.

Coffey also noted that the Los Alamos results showing ignition in NIF "had been obtained only two weeks prior to the ICFAC meeting" that endorsed KD-1, scarcely a sufficient time for competent and thorough peer review. These calculations showed that achieving ignition was very sensitive, "not only to the physics included in the calculation, but also to the numerical algorithms used." In the case of Livermore's target design for achieving ignition, "it was clear that their design was very sensitive to the physical conditions which existed in the hohlraum. For example, a few electron volts angular variation in hohlraum temperature could make the difference between ignition and non-ignition." Likewise, the ICFAC had no experimental data available to it confirming the performance of NIF-like targets in gas-filled hohlraums (as noted above, the evidence from the recent experiments is negative).

Coffey further observed that the NIF target designs are all based upon cryogenic pellets, "yet no NIF-like cryogenic pellets have been experimentally tested in laser-produced hohlraums." "The entire program," he noted, "hinges upon the assumption that these cryogenic targets can be fabricated to the tolerances necessary, yet there are no data to support this." According to documentation submitted to the ICFAC by a committee consultant on the laser design, "LANL has been unable so far to get code calculations to agree with LLNL (i.e. get ignition) at the 1.8MJ level, let alone lower levels."¹ Coffey concluded his dissent as follows:

When one reviews the above situation, one could conclude that the ICFAC has designated the engineering design program as a research program itself. This is most peculiar for the engineering design of a major new system....A number of ICFAC members have rationalized their acceptance of proceeding with KD-1 on the basis that they can always withhold the KD-2 decision if things don't work out.

¹ U.S. Government Memorandum, 2 May 1994, to Dr. William Simmons from John M. McMahon, Chief Scientist, Optical Sciences Division, NRL.

In fact, what will happen is that the flexibility in the program will rapidly diminish as the reality of engineering decisions take hold. ICFAC will find that while it can theoretically withhold an endorsement of KD-2, to do so becomes an extraordinarily high stakes game. Having done the engineering design, and then not recommending KD-2 approval, would be tantamount to killing the entire ICF program (emphasis added).

2. Can NIF's design power of 1.8 MJ be achieved?

NIF is comprised of 192 separate laser beams, each with a 34.8 cm x 36.8 cm aperture, converging on a single hohlraum target. The total energy incident on the entrance holes of the hohlraum is 1.8 megajoules (MJ) during a pulse that is 16-20 nanoseconds (ns) long with 3.5 ns peak near the end. Each beam, therefore, must achieve (1.8 MJ)/192 = 9 kilojoule (kJ).

The NOVA Upgrade Prototype Beamlet (hereafter Beamlet) energy design criterion is 7.6 kJ for a shorter 3 ns pulse and a smaller (32 cm x 32 cm) aperture. To date Beamlet has not achieved its design criterion.

LLNL does not have a good history of achieving its laser fusion energy criteria. NOVA was designed to achieve 40-70 kJ at the third harmonic ($3\omega = 3.5 \mu\text{m}$) [1982 Laser Program Annual Report, UCRL-50021-82, August 1983, p.2-2]. Most shots have been in the 25-30 kJ range, with a few shots high as 35 kJ. NOVA could achieve 60 kJ, but at this energy the beam could not be focused.

3. What will NIF cost?

DOE estimated NIF will take seven years to design and construct (FY 1996-FY 2002) at a cost of \$843 million of construction funds and an additional \$233 million of operating funds for the conceptual design, National Environmental Policy Act (NEPA) compliance, R&D, etc., for a total of \$1.076 billion (expressed in spent dollars). After completion of the facility it is estimated to cost \$60 million per year (expressed in FY 1996 dollars) to operate the facility.

The DOE estimate of NIF construction costs translates into \$1.076 billion/1.8 MJ = \$600/J. The hardware costs alone for the 60-beam 30 kJ Omega Upgrade were \$60 million. Thus, large glass laser projects in the past have cost on the order of \$2-3000/J, or four to six times the DOE estimate for NIF. LLNL argues that NIF uses a different laser technology. But, the cost of the Beamlet laser system is believed to be on the order of \$20-22 million. On this basis NIF would cost (192*\$20 million =) \$3.8 billion, less economies of scale. It is difficult to discern the basis for LLNL's expectation that it will achieve a unit cost reduction of 80% by ordering 192 beams rather than one.

LLNL also claims \$2-3000/J scaling is inappropriate for NIF because LLNL will invest \$130 million in a three year "risk reduction program" to bring down the cost of the optics and lasers. (Optics and lasers represent over 60% of the construction cost, exclusive of contingency and escalation.) In its April 20-21, 1994 report on Key Decision One, the Laser Subcommittee of DOE/DP's Inertial Confinement Fusion Advisory Committee (ICFAC) stated,

Although we did not examine in detail the NIF cost estimates, of which the laser driver is the major element, we believe the cost estimates are credible. In addition, however, advances in technology expected by the expenditure of \$130M for the three year risk reduction program, presented as Core Development, are essential to achieve these performance and cost estimates.

The Manufacturing Readiness Plan for NIF claims,

The main goal of the development phase for optics manufacturing is to develop, with the optics industry, production methods which can meet the cost estimates contained in the CDR [Conceptual Design Report]. The main goal of the development phase for laser components is to demonstrate the performance levels which are presumed in the laser design described in the CDR.

In sum, achievement of a large cost saving is based in part on LLNL asking potential vendors whether they could bring their costs down if LLNL gave them \$130 million in R&D. The incentive of the vendors was to say "yes," since they are not obligated to achieve the lower price target after accepting the R&D funding.

4. NIF is not the right driver for providing commercial power by ICF.

In order for ICF to compete with other base load electric power technologies, there is widespread technical agreement that Nd:glass lasers would have to be pulsed at their full energy on the order 5 times *per second*. Because the laser optics must be cooled between pulses, NIF can only sustain about 3 full power pulses *per day*. The NIF pulse frequency is about 150,000 times less than that desired for commercial energy production.

If a primary objective of the ICF program is to demonstrate the feasibility of commercial power production using ICF, DOE should cancel NIF and place a higher priority on the research and development of drivers that have at least the *inherent potential* of becoming commercially viable. Heavy ions and krypton fluoride (KrF) excimer lasers are the two most promising ICF drivers for a reactor because they have the potential for a high repetition rate, a relatively high efficiency, and an acceptable cost. Both drivers are now under development by DOE, but at a low budget level.

There are scientific uncertainties related to whether high current heavy ion beams can be focused through the chamber onto ICF targets, and whether the energy gain of the pellet is sufficiently high. Reactor studies indicate that the gain from the heavy ion hohlraum target should be at least 40. The NIF hohlraum has, even according to its proponents, a projected gain of about 4-6; this is about a factor of ten too low. A more sensible strategy, therefore, would be for DOE to first demonstrate high current heavy ion beam focusing. DOE could then evaluate the target design using this heavy ion driver. To achieve high gains with heavy ions would probably cost about the same as the NIF. Under the current strategy DOE will spend several billion dollars on NIF to do target research, and then have to spend another billion or more for a heavy ion driver. NIF hohlraum target designs have significant differences from the hohlraum designs that would be used with a heavy ion driver, with a factor ten difference in target gain. There is no obvious saving by first demonstrating just ignition using a Nd:glass laser.

KrF lasers could be used to directly illuminate a pellet, without the need for a hohlraum. There are uncertainties as to whether a KrF gas laser can achieve sufficient efficiency and reliability at 5 pulses per second, and there are uncertainties as to whether the laser beam illumination can be smooth enough to symmetrically implode the pellet. The better the laser symmetry, the higher the predicted target gain. Reactor studies indicate that a KrF reactor needs a gain of 120. Existing KrF lasers are 6-10 times smoother than existing glass lasers. It clearly would be advantageous for DOE to evaluate the direct illuminations concept with the most uniform possible laser, especially when this laser is also the candidate for reactor applications. Because of its smoother laser profile and direct coupling to the pellet, a few megajoule KrF laser is predicted to produce pellet gains of 100-250, not just ignition or low gain. A two megajoule, low repetition rate, KrF laser probably could be built for a similar cost to the NIF. A parallel technology development effort on a small laser could demonstrate the necessary high efficiency, high repetition rate laser operation. By then combining these two programs, DOE could proceed with a prototype reactor.

5. Will NIF contribute significantly to the resolution of reliability and safety issues involving the existing stockpile of nuclear weapons to be retained under a CTB?

The short answer to this question is no. Despite misleading statements by both proponents and opponents of the facility, NIF cannot "simulate" nuclear weapons test explosions. Experimental data from NIF – which cannot replicate the fission-fusion "mixing" phenomena or the full spectrum of radiation, temperatures, and pressures experienced in a nuclear weapon test explosion – cannot be extrapolated reliably to model the performance of nuclear weapons. Calibrated U.S. nuclear weapon design codes employ empirical factors derived from nuclear explosive test data, and cannot be modified to incorporate data from new high energy density experimental facilities without incurring substantial – and most weapon designers believe unacceptable – technical risk.

A better approach, and in fact the one planned by the United States, is to use high resolution radiography and other diagnostic techniques to better assess small changes in the "hydrodynamic" (pre-nuclear) performance of previously tested primary implosion systems, and then to use nuclear test-validated computer codes to calculate the effects, if any, on nuclear performance. With or without the NIF or some similar program, if properly executed this conservative approach can maintain a high level of confidence in the future nuclear deterrent at a reasonable cost.

Notwithstanding the extravagant claims of NIF proponents at Livermore, NIF's role in stockpile stewardship is, at best, indirect. By providing attractive research opportunities in areas of experimental physics and computer modeling involving small-scale thermonuclear reactions, an ICF program can help retain a cadre of experts with the skills needed to *resume* a thermonuclear weapons design program, should a CTB regime break down in the future. But the need to preserve some capability in this area – how much is needed is obviously a matter for debate – does not automatically add up to a specific requirement for NIF. Nor is NIF likely to be the only, or even the primary, direct means of maintaining this capability. Thermonuclear weapons design is sufficiently different from the implosion physics of tiny laser-driven hohlraums that a dedicated "stewardship" effort is required in any case.

Particularly misleading is the recent claim by Livermore ICF program scientists that "NIF data would be used to benchmark and improve computer codes that are needed to certify the safety and reliability of our remaining stockpile." In reality, people who work on nuclear weapons safety are likely to see the inside of NIF only on family-day tours. Safety questions concern the primary stage alone, as do the vast majority of reliability issues, whereas NIF is geared to developing codes and some basic physics data that may – if ignition is achieved – help to refine some aspects of the physics models used to compute how radiation from a primary drives a secondary. In fact, the emphasis on NIF is already taking money away from directly relevant safety and reliability efforts. The most important people for safety research are high explosive experts and primary designers. NIF will not employ any scientists who work on safety, but it will compete with them for funds.

As for reliability, the anticipated energy gain, even if NIF works as intended, will be insufficient to investigate the fission-fusion "mixing" phenomena involved in "boosting" of the primary to its prescribed minimum required yield for driving the secondary, so the relevance of NIF to maintaining weapons reliability [in reality "confidence in reliability"] is negligible. In fact, until the NIF bandwagon really started to roll, it was universally acknowledged that a Laser Microfusion Facility capable of energy gains > 100 – compared to the factor of 1-10 gains potentially achievable in NIF – would be required before any weapon design issues could be seriously addressed. More importantly, however, the truly relevant reliability issues will continue to be addressed as they have been in the past, by careful weapon disassembly, component inspection, testing, and replacement; and by intensively diagnosed, non-nuclear explosive testing of the complete weapon assembly system.

As noted above, if ICF energy is the primary long-term goal, a heavy-ion beam or KrF laser driver program should take precedence, and target physics experiments of a type useful for maintaining relevant skills could continue on NOVA and other facilities until the heavy-ion beam or KrF laser was available for driving high-gain targets, albeit of a different design than those to be employed in NIF.

6. Can the U.S. build NIF as a principal component of a nuclear weapons stockpile "stewardship program" without contributing to the proliferation of capabilities for the design of thermonuclear weapons?

The answer to this question has both political and technical aspects. From the political perspective, to the extent that the United States and other weapon states, such as France (now formally a partner with the U.S. in NIF research) emphasize the nuclear weapons relevance of ICF research, the program will continue to spark interest in the weapons applications of ICF research in other advanced industrial states with undeclared nuclear weapons programs, such as India, Israel, and Pakistan.

On the one hand, to the extent that ICF research remains classified, political suspicions of an attempted U.S. end-run around the CTB will multiply. On the other hand, to the extent that weapons relevant NIF data – such as the radiation opacity of various materials at high temperatures – and computer codes are made available through joint research efforts, international scientific symposia, and the open literature, then the technological basis for thermonuclear weapons design in other countries inevitably will be strengthened. When the U.S. government last conducted a formal evaluation of the ICF proliferation issue, fourteen years ago, it concluded as follows:

Concerns exist within the French, UK, US, and USSR governments that an ICF R&D program *could be a precursor to an advanced nuclear weapon program* insofar as non-nuclear weapon states used ICF work to acquire the information, technology, trained people, and facilities applicable to nuclear weapon development.

....

ICF research could stimulate development of nuclear weapons technology in non-nuclear weapon states [deleted]. If an advanced non-nuclear weapon state with an ICF research program undertook a nuclear weapon program, it might subsequently be able to move more quickly to develop boosted fission and thermonuclear weapons than would otherwise be the case.

....

One relationship of ICF to weapon work is the similarity of computer codes used in the two activities. [Deleted] Many of these codes, especially earlier simpler codes, are unclassified and have been made available outside the weapon laboratories. *Growing interest in ICF creates pressure for release of current, classified ICF and weapon codes.* [Deleted].

....
 ICF research will not provide a viable source of energy or fissile material for civil purposes in this century – indeed, it may never do so – but the possibility of such a long term payoff is sufficient inducement to many countries to proceed with this research; *others may be attracted by the peaceful “cover” ICF provides for weapons-related research and development.*

....
 The possible applications of ICF to nuclear weapon programs are primarily indirect. However, *ICF programs in non-nuclear weapon states, and perceptions by non-nuclear weapon states of the potential value of ICF research to nuclear weapon states, could affect our arms control policy objectives.*²

The extent to which any or all of these conclusions remain valid today should be the subject of a comprehensive and detailed analysis before proceeding with the ICF/NIF program as currently structured.

Conclusion

The unresolved status of the major issues outlined above indicates that the Department of Energy’s decision to proceed with engineering design of the NIF was premature. The technical and analytical basis for proceeding further with the design and construction of NIF does not yet exist.

An alternative ICF development approach, worthy of consideration, would be to redirect the ICF program toward development of a viable beam driver for ICF energy research and development. This approach could mitigate many of the difficulties noted above. This strategy would essentially skip the multi-billion dollar step of developing, constructing, and operating a glass mega-laser like NIF in the near-term. Significant funding could be shifted into the civilian side of DOE’s ICF program, and the exaggerated emphasis on the value of ICF for “stewardship” of U.S. nuclear stockpile weapons could be greatly reduced.

From a political perspective, this step would have the beneficial political effect of lessening the damaging “perceptions by non-nuclear weapon states of the potential value of ICF research to nuclear weapon states” cited in the U.S. government’s 1981 analysis, while from the technical perspective, it would focusing the near-term research effort on the less proliferation-sensitive issue of beam driver development. Likewise, since the resolution of specific target physics issues would no longer be at the forefront of the program in the near term – because the work would have to be redone in any case when and if a heavy-ion beam or KrF laser driver becomes available – there would be less pressure for further

² "Inertial Confinement Fusion," *Fiscal Year 1981 Arms Control Impact Statements*, USGPO: 1980, p. 460 - 474.

declassification and international collaboration in this sensitive area, which currently has no direct bearing on the future of ICF for civil energy purposes.

This strategy would provide roughly another decade for strengthening the international nuclear nonproliferation regime before seeking to resolve the difficult high-gain target physics issues – an international collaborative effort that will inevitably involve the generation and release of sensitive data and algorithms that could be of assistance to thermonuclear weapon design programs.