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Review of the Department of Energy's Inertial Confinement Fusion Program

Final Report

Second Review of the Department of Energy's Inertial Confinement Fusion Program
Final Report

Committee for a Second Review of the Department of Energy's
Inertial Confinement Fusion Program

Commission on Physical Sciences,
Mathematics, and Applications
National Research Council

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PREFACE

The Department of Energy National Security and Military Applications of Nuclear Energy Authorization Act of 1985 directed the President to establish a "technical review group to review thoroughly the accomplishments, management, goals, and anticipated contributions of the defense inertial confinement program." The congressional language called for an interim report by June 1, 1985, and a final report by May I, 1986. The more detailed terms of reference for the study were as follows:

> The Technical Review Group will review the accomplishments, management, goals, and anticipated contributions of the Defense Inertial Confinement Fusion Program. The Technical Review Group shall consist of individuals who are highly qualified in scientific disciplines associated with the development and testing of nuclear weapons. The Technical Review Group will review all major areas of the inertial fusion program. The Technical Review Group shall prioritize activities within the present and future [Inertial Confinement Fusion] ICF Program and present an appropriate time scale for attaining the program goals.

In his letter of December 20, 1984, the President's Science Advisor called on the National Academy of Sciences to carry out the directed review. In response, the Academy established the Committee for a Review of the Department of Energy's Inertial Confinement Fusion Program under the chairmanship of Dr. William Happer, Jr., of Princeton University. The committee, made up of ten carefully chosen scientists, conducted its review during 1985 and issued its final report in March 1986. Prominent among the several findings of that review were the following.

> It is important to recognize that the present state of knowledge does not permit a narrow focusing of the ICF Program. At the same time, budget limitations require a prioritization of activities. We believe that approximately five years at current budget levels is required to resolve critical technical issues of ICF feasibility.
>
> The Committee unanimously recommends that the budget should be stable during this five-year period and at a level adequate to achieve the highest priority objectives, which, we believe, is approximately at the current level ($155 million/year).

In considering the findings contained in this report, it is important to remember that the five-year research and development period set by the Happer committee will not end until March 1991, and that the ICF Program has not been level-funded during the 1986-1989 period. Further, congressional interest in a second review three years after the Happer report was undoubtedly stimulated by very positive research results obtained during the interval and by growing interest within the ICF community in selecting and building a driver at the next energy plateau.

The present review of the ICF program resulted from language contained in the FY89 authorization and appropriating bills that directed the Secretary of Energy to commission an independent review, the interim report of which to be due by January 15, 1990, and the final

I. INTRODUCTION

This report of the second review by the National Research Council of inertial confinement fusion (ICF) contains the appointed committee's final conclusions and recommendations. An interim report was issued in January 1990. The charge to the committee was as follows:

1. Determine whether the recommendations of the 1985 NAS review are still appropriate to advance the technology efficaciously.
2. Provide an assessment of the most promising technologies for continuation of the program.
3. Assess the potential contributions of the program under the following scenarios: (a) a comprehensive test ban on underground nuclear testing and (b) prohibition of underground nuclear testing to levels of 1 kiloton, 5 kilotons, and 10 kilotons.
4. Assess the civilian energy potential of ICF.
5. Assess the adequacy of the ICF target performance data base for supporting program plans and decision milestones.
6. Identify major technical and programmatic issues facing the program.
7. Determine the status of each major candidate inertial fusion driver (including heavy-ion drivers), and specify the critical issues involved in the development of each.
8. Recommend program priorities, particularly with regard to the Centurion/Halite program, driver development, and laboratory experiments and theory. Recommend relative priorities of individual support laboratory activities.
9. Examine the strategies and plans of the ICF Program, comment on their soundness, cohesiveness, and programmatic effectiveness, and recommend management initiatives that could improve the progress of the program toward achieving of its goals.

The major difference between the 1985 and 1989 reviews is the request for greater attention to the energy potential of the ICF Program and to the heavy-ion work being carried out by the Lawrence Berkeley Laboratory (LBL) at the University of California. The LBL heavy-ion effort is currently supported by DoE's Basic Energy Sciences Program under its Office of Energy Research.

The findings contained in this interim report are based on the extensive briefings and documentation obtained through our organizational meeting (July 18-19, 1989), writing session (December 4-6, 1989), and site visits to all but one of the participating laboratories: Lawrence Livermore National Laboratory (LLNL; August 15-16, 1989), LBL (August 16, 1989), KMS Fusion (October 16, 1989), University of Rochester's Laboratory for Laser Energetics (LLE; October 23-25, 1989), Los Alamos National Laboratory (LANL; November 1-3, 1989), and Sandia National Laboratories (SNL; November 3, 1989). The committee did not meet at the Naval Research Laboratory (NRL), but the results of that laboratory's research were amply briefed at other meetings. A meeting was held at DoE Headquarters on June 5-6, 1990, to review comments by the representatives of the laboratories and DoE on the Interim Report, and to hear a presentation of a new LLNL proposal (NOVA Upgrade) for an accelerated effort to achieve ignition and gain in the laboratory, Another meeting was held at DoE Headquarters on August 1, 1990, to hear comments from the ICF program participants on the NOVA Upgrade proposal, and a subsequent meeting was held in La Jolla on August 21, 1990, to consider future experiments possible with the AURORA facility. On August

29-30, 1990, a meeting was held at SNL to review progress on milestones for the light-ion driver program and to write the final report. In addition, the laboratories provided written responses to detailed questions from the committee. Although chemical lasers are not a part of the current ICF Program, the committee also allowed time for two presentations on the hydrogen fluoride (HF) laser and reviewed extensive documentation on that approach.

Since the ICF Interim Report was issued, DoE has constituted the Fusion Power Advisory Committee (FPAC) to recommend an overall policy for fusion energy, including magnetic confinement and ICF. There has been a strong and mutually beneficial interaction between FPAC and our committee.

This final report has a number of important changes from the interim report. The recommendations and associated discussions have been revised completely. It should be read as a stand-alone document.

II. TECHNICAL BACKGROUND

The technical goal of DoE's program in ICF is to produce small thermonuclear explosions in the laboratory. The potential military uses of these explosions include extending capabilities to simulate the effects of nuclear weapons on hardware that must function in a nuclear environment, exploring the basic atomic physics and hydrodynamics important for weapons design, developing instrumentation and techniques to study full-scale nuclear tests, and exploring advanced weapons concepts. The potential long-term civilian application of ICF is energy production.

The basic idea in ICF is to use a "driver" to compress and heat a small capsule of nuclear fuel (the deuterium and tritium isotopes of hydrogen) to the point where the fuel ignites[1], releasing energy and radiation. To do this, a large amount of precisely controlled energy must be delivered to the capsule in a very short time, and the capsule must be constructed to absorb the energy efficiently. Driver performance and capsule design/fabrication are thus the major requirements for a successful ICF Program.

The current ICF Program is pursuing a variety of possible drivers: glass lasers, krypton fluoride (KrF) excimer lasers, light-ion beams, and heavy-ion beams. Other drivers have been considered and abandoned. The current driver options have their relative advantages and drawbacks and are in various stages of development. None of the existing facilities is powerful enough to achieve ignition.

There are two types of ICF targets. "Direct-drive" targets absorb the energy of the driver directly into the fuel capsule. "Indirect-drive" targets use a cavity (hohlraum) to convert the driver energy to x-rays, which are then absorbed by the fuel capsule. This latter method can tolerate greater inhomogeneities in driver illumination, albeit at the expense of efficient delivery of energy to the capsule.

The ICF Program has passed a number of significant technical milestones since the time of the Happer report. Experiments have been done with the NOVA laser operating at its full design energy; techniques have been developed for smoothing laser beams; new diagnostic techniques and instrumentation have been developed; and direct-drive targets have been imploded to densities several hundred times that of liquid hydrogen. Further work in the Centurion/Halite program of underground experiments has shown qualitatively that the basic concept behind ICF is sound. Although increasingly sophisticated laboratory experiments and computer models have tempered the early optimism that small drivers would be sufficient for laboratory ignition, they are also leading toward firmer estimates of the minimum driver size required.

A significant near-term goal of the ICF Program should be to achieve ignition and modest gain[2] (2-10) in the laboratory by a dedicated effort using a largely renovated NOVA. Other goals are to construct a laboratory microfusion facility (LMF) that would produce high gain and yields exceeding several tens of megajoules, and/or development of ICF for energy production (inertial fusion energy (IFE). An LMF would have important military applications almost immediately. A crucial issue for our panel was whether there is now sufficient

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[1] By ignition, as used in this context, means that the "hot spot" fuel in the center of the implosion attains sufficient temperature and density so that its thermonuclear reactions not only heat the hot spot further, but also promote burning of the compressed pusher fuel.

[2] The "gain" is the ratio of the thermonuclear energy released (yield) to the energy delivered to the target by the driver.

confidence in driver and target technology to proceed directly with the LMF, or a large IFE project, or whether the intermediate step of demonstrating ignition and modest gain (2-10) with a less ambitious facility should be undertaken first.

III. ICF AND NUCLEAR WEAPONS

The relationship between ICF and nuclear weapons is real and substantial, although complex. Observations on this relationship are presented in the following paragraphs.

The ICF Program is part of the nuclear weapons program because the basic physics of ICF capsule heating, implosion, and burn is the same as that in nuclear weapons. In addition, many of the radiation/hydrodynamic phenomena (e.g., mix and instabilities) that influence nuclear weapon performance also influence ICF capsule performance, although there are important differences (due largely to the different sizes). Some of these phenomena, as well as the more basic physics, can be studied more easily in laboratory ICF experiments than in underground tests (UGTs), both because many more experiments can be done and because some diagnostics can be used in the laboratory that cannot be used underground. Sophisticated computer codes have been developed to model ICF physics and to aid in target design, notably the LASNEX code. Results of these theoretical efforts have been of interest to the nuclear weapons program, as they have many similarities to weapons work addressing similar issues; these developments in the ICF and weapons programs have been mutually supportive. Diagnostic methods for UGTs can be, and have been, developed using ICF experiments, and challenges involved in ICF target fabrication have led to improvements in the laboratories' capabilities in materials processing and precision finishing, which are now available for use in the weapons program.

The ICF facilities themselves, apart from capsule implosion experiments, have been useful to the weapons program because of their unique capabilities and relatively easy availability. Such uses have included experiments on hydrodynamic instability phenomena, investigations of the opacities and equations of state of matter at high temperatures and densities, and studies of physical processes in X-ray lasers. ICF facilities have also proved useful in investigating some types of nuclear weapons effects and the lethality of directed-energy weapons.

To date, ICF has had a limited (but real) effect on the weapons design program. However, its impact is growing. One measure is that 20 percent of the NOVA experiments carried out in FY89 were related to weapons physics, and half of those were supported with non-ICF weapons program funding and personnel; such weapons. use has increased dramatically in the past few years. The target development leading to an LMF will increase the benefits to weapons and a working LMF would raise this ICF/weapons design interaction to a significantly higher level. These increased benefits could be realized even before ignition and burn are achieved, as pre-ignition phenomena are also important. Of course, ignition would open a new and important way to study thermonuclear phenomena in the laboratory.

Another important aspect for the weapons program is that ICF can attract scientists and engineers, and develop and maintain skills directly applicable to weapons design. This benefit and the more technical benefits described above are closely related ‘to the amount of data available from underground nuclear testing, which is influenced both by funding and by treaty limitations on test yields or numbers. Physics understanding gained efficiently from ICF experiments can reduce risks of failure in UGTs or allow larger parameter ranges to be tested. However, the different sizes, the absence of fission phenomena, and many other considerations mean that ICF experiments alone cannot provide-today or in any foreseeable circumstance-an adequate basis either for the design and production of new nuclear weapons or for the complete evaluation of stockpile reliability, although ICF would play an important role in maintaining competence under severe test limitations.

The value and optimal schedule for an LMF depends on the status of underground testing. An LMF becomes more valuable with greater limitations on underground testing, whether in number of tests or in yield. Under a limitation by quota, while the difference between no tests and even a very few is a significant one for the weapons program as a whole, the importance of an LMF depends less on the precise testing situation. If there were only one or two tests a year, LMF benefits might be almost as great to the weapons program as if there were no testing. This may be especially true for diagnostics developed in the ICF Program for underground tests. If numbers of tests are limited by funding rather than treaty, a trade-off is implied against progress toward an LMF, which is also funding-limited. We cannot evaluate that trade-off, but suggest that it may soon become an important decision criterion.

With regard to yield threshold treaties, the value of ICF to the weapons program may change qualitatively at a few kilotons. Below that level, the importance of ICF is expected to increase greatly. Because of the severe trauma that would be induced in the weapons program, it is difficult to spell out in any detail what normal UGT functions could or should be shifted over to an LMF. It is clear, however, that under a complete test ban ICF would be extremely important for maintaining vitality and technical capability in the weapons program.

Another military benefit from ICF is the potential to provide above-ground radiation sources for studying the effects of nuclear explosions on military hardware. ICF explosions emit x-rays and neutrons with a time dependence that is characteristic of the fastest nuclear weapon outputs; both can be slowed by external means. The energy spectra of the x-rays and neutrons are characteristic of the most energetic weapon outputs, but again the energies of both can be moderated and fast neutrons can be converted to gamma rays by inelastic scattering in an external layer. The simulation fidelity achievable for both x-rays and gamma rays should be superior to that from present electrically pulsed x-ray simulators.

Yields from ICF of 30 MJ or more would allow exposing electronics boxes to x-rays and gamma rays. Larger yields would provide radiation for groups of boxes and their interconnecting cables. At 1000 MJ, the upper end of the range of ICF yields being considered for an LMF, interesting x-ray and gamma ray exposures could be provided for electronic systems with dimensions up to several meters. The effects of x-rays on such structures as reentry vehicles could also be tested. For example, 1000 MJ should provide about 100 cal/cm^2 of x-rays at a distance of 3 m from the ICF source.

It is clear that the convenience and repeatability provided by an LMF for such tests would be a great advantage. We are not advocating the cessation of UGTs, but note that a total nuclear weapons test ban would not have to stop radiation testing if an LMF were available. If the allowable yield threshold dropped below 1 kiloton, the need for an LMF would become great.

IV. NATURE OF THE ICF PROGRAM

Seven laboratories are involved in the DoE's ICF Program:

- Lawrence Livermore National Laboratory (LLNL), pursuing a predominantly indirect-drive program (with a small direct-drive component), featuring a glass laser system (NOVA) and, until recently, also the Halite program of underground experiments;
- Los Alamos National Laboratory (LANL), pursuing an indirect drive KrF laser (AURORA) and, until recently, also the Centurion program of underground experiments;
- Sandia National Laboratories (SNL), pursuing a light-ion driver (PBFA-II);
- Naval Research Laboratory (NRL), pursuing the direct-drive approach with a KrF laser (NIKE);
- Laboratory for Laser Energetics (LLE) at the University of Rochester, pursuing the direct-drive approach with a glass laser (OMEGA);
- KMS Fusion, Inc., a commercial organization working on target fabrication; and
- Lawrence Berkeley Laboratory (LBL), pursuing heavy-ion drivers.

The ICF Program began formally in 1963. Total funding since that time has amounted to $2.52 B. Currently, all efforts except that on heavy-ion drivers are funded by the Office of Defense Programs; its total FY90 budget for ICF was $169.2 M, divided into "operating" $160.6 M and "capital" $8.6 M. Work on heavy-ion drivers is funded out of the DoE's Office of Energy Research and amounted to $6.3 M in FY90. Roughly 1250 scientists and engineers are supported by the entire ICF Program.

The committee was impressed by the high level of science and technology in the ICF Program. There is an inherent scientific interest, technological spinoffs, and a relevance to weapons physics and weapon effects simulation. The ICF Program strengthens related scientific programs in the national laboratories by attracting a high calibre of personnel.

It is the committee's impression that the present ICF Program was somewhat distracted from orderly scientific progress by a desire to push ahead directly to the LMF. As a consequence, existing facilities were not being fully utilized, and important experiments were not being done, Recent efforts to exploit NOVA have begun to rectify this situation.

The Happer panel recommended a five-year program at level funding (after correction for inflation) to ascertain the physics credibility of ICF, with a subsequent decision about continuing the program. Good progress has been made in the intervening years, but because funding has fallen significantly short of the recommended level and because the ICF effort is divided among six different laboratories with different approaches, all program objectives would not be met with the funds available. However, the program is now at the point where, with adequate funding, the physics and driver technology could be in place in a few years for a firm decision whether to proceed with an ignition demonstration facility, and subsequently a larger facility, whether for defense or IFE.

V. OVERVIEW OF THE RECOMMENDATIONS

The committee believes that, after some 27 years and $2.5 B, there is a reasonable chance for laboratory ignition and gain with the next major ICF facility, given favorable results from a few well-defined crucial experiments. This conclusion is based on a number of independent elements, which include increasingly sophisticated laboratory experiments, the results of the Halite/Centurion program of underground tests, and improved theoretical understanding and modeling of hohlraum and capsule physics. Ignition and gain in the laboratory represent an important milestone for the ICF Program and a necessary first step toward fusion applications, be they defense or energy. Only after such a demonstration will it be possible to plan realistically and quantitatively for the future of ICF.

Discussions within the ICF community during the past few years have assumed an LMF (with yields to 1 GJ and immediate defense applications) as the next large project. An LMF for defense purposes may indeed be a natural product of the ICF program on a 10- to 20-year time scale, but considering the extrapolations required in target physics and driver performance, as well as the likely $1B cost, the committee believes that an LMF is too large a step to take directly from the present program.

However, recent theoretical arguments, experimental results, and laser designs have convinced us that it should be possible to closely approach, and probably achieve, ignition and modest gain in the laboratory by the intermediate step of a few-megajoule class laser driver, which might be constructed for less than $400 M. A successful "ignition demonstration" would allow exploration of the drive and target parameters to optimize gain, study of the physics of thermonuclear burn in the laboratory, and testing of advanced target designs. These programs would form the basis for a high-confidence decision on an LMF and would motivate and support a concerted effort toward IFE.

The committee believes that it is now realistic to begin planning for such an ignition demonstration, although a final commitment to construction will require several more years of experimental and design work. In particular, experiments are required to confirm adequate understanding of laser-plasma interactions and capsule performance in the relevant regimes. These can be carried out with existing facilities, somewhat improved, and with those facilities currently proposed.

The glass laser is the only candidate laser driver that could be used for an ignition demonstration in the next decade. Indeed, this demonstration is the natural next step in the NOVA program and is referred to by LLNL as the "NOVA Upgrade." The committee cannot judge whether, if all drivers were sufficiently mature, a glass laser would be the "best" driver; much target work with KrF lasers and light- and heavy-ion accelerators would be required to match the database of driver and target performance that exists from the NOVA and OMEGA programs. The real point is that a glass laser will likely allow an ignition demonstration for a reasonable cost, and there appears to be no compelling reason to wait for other drivers to "catch up."

It would be prudent to pursue other driver technologies in parallel with an ignition demonstration, even if only at a modest level. As the committee has noted, while success with a glass laser appears likely, it is not certain. Experiments in the next few years might not turn out as expected, or the required driver performance might prove to be too costly (or even impossible) to achieve, or suitable targets may not be available. Moreover, glass lasers are unsuited for IFE; the efficiency and pulse repetition rate of KrF lasers and heavy- and light-ion accelerators make them much more attractive for this application.

Unfortunately, budget realities are such that the present level of funding for the ICF Program cannot continue to support all candidate drivers in view of the cost of planning for, and attempting, an ignition demonstration. Unless additional funds are made available, our recommendations therefore amount to a de *facto* selection of a driver for an ignition demonstration and a focusing of resources to pass the major milestone of ignition.

VI. RECOMMENDATIONS

The recommendations below are made with an appreciation of the likely very tight federal funding situation. The committee wishes it to be understood that the ICF Program is regarded overall, in its entirety, as a well-managed and well-executed effort that attracts extremely able scientists and engineers to a clear and important national objective. Even those parts of the program that are deemed to be of lower priority are worthwhile and, if they could be funded, would give the ICF effort more resilience, strength in depth, and confidence in reaching its goals.

Recommendations 1 though 3 below have the highest priority in the program and are given in priority order. By this the committee means that it is more important to respond to the earlier recommendations than the later ones. With the very restricted budgets that are expected, this could mean that some low-priority programs will be funded inadequately or perhaps even not at all. In this connection, it is noted that two of the great strengths of the ICF Program have been the variety of different approaches pursued and the involvement of six different laboratories. A breadth of approach and a diversity of viewpoints have been very important. Although this breadth might be diminished by budgetary limitations, it would be very unfortunate at this stage if the program were to degenerate into a one-laboratory program pursuing a single approach. On the other hand, the worst future for the program would be one **in which** a **multiplicity of approaches** would all be pursued at subcritical levels.

> **Recommendation 1: The expeditious demonstration of ignition and gain should be the highest priority of the ICF Program. Adequate funding toward this goal must be assured.**

The committee believes that such a demonstration could be achieved by the end of the decade. It is, in any event, a necessary preliminary to energy or defense applications of ICF. To achieve this goal, concurrent programs in target physics, driver development, and target fabrication are required. In target physics, there must be a concerted national effort to resolve the most important remaining uncertainties about laboratory ignition. It will involve the use of some existing facilities to their full potential, the development of appropriate instrumentation, and an extensive experimental campaign with multi-laboratory participation. In driver development, there must be a concurrent effort to refine and validate an architecture for a glass laser driver so that it is available when the physics milestones have been met. Finally, there must also be a commensurate concentration on advanced target design and fabrication. Optimally designed targets are very important for an ignition demonstration and will also allow the program to take maximum advantage of the drivers now available; the committee suspects that crucial subtleties (or even radical designs) remain to be explored. It is also important that targets be fabricated with high precision. Recent demonstrations of the beta-layering[3] technique for cryogenic targets have been encouraging, but many fabrication challenges remain.

In view of the key role of an ignition demonstration, the committee urges that it be attempted as a national, interlaboratory program, rather than as a single laboratory (LLNL) program. In particular, LANL should play a key role in the target design and fabrication and

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‘Beta-layering is a process by which, due to the heating of Beta particles emitted in tritium decay, layers of frozen deuterium-tritium mixtures can be more uniformly deposited in a capsule.

in the experimental program in preparation for an ignition demonstration. Such a national program, with greatly expanded participation from LANL (and perhaps other laboratories) in the experimental campaigns, would accelerate and enhance the credibility of the effort. Discussions to define appropriate roles for the various laboratories and specify modes of collaboration among them should begin promptly to allow for meaningful national input to the effort before designs are frozen.

To implement this recommendation, the following four subrecommendations are suggested:

> Recommendation **1 .1:** We recommend **that funds be provided for Precision NOVA and the associated experimental campaign.**

A campaign to validate experimentally the theoretical and design predictions could be completed in 3 to 4 years. The important issues to be addressed and milestones to be passed are outlined in the LLNL Target Physics and the Cryogenic Target Development Technical Contracts. Unclassified excerpts from these documents are reproduced in Appendices I and 11, respectively. These research plans, endorsed by the committee, have been presented to, and scrutinized by, the broad ICF laboratory community. At the conclusion of this campaign, the following are expected:

a. Precision NOVA (-5% beam balance) will have been completed, as will the Hydrodynamically Equivalent Physics (HEP)l-4 campaigns, and HEP5 will have demonstrated hot spot densities of 40 gm/cc and pusher compressions to a density of 400 gm/cc The yield degradation compared to idealized one-dimensional calculations will have been diagnosed and understood as a function of Rayleigh-Taylor growths and convergence sufficiently to make a confident extrapolation to NOVA Upgrade performance-enough to guarantee at least a thorough exploration of the ignition physics, with a high probability of ignition.

b. The plasma physics and hohlraum issues will have been resolved by hohlraum and laser plasma (HLP)l-6, with HLP7 in progress, and it will have been shown that no strong stimulated raman scattering (SRS), hot electron production, or disastrous geometry changes are to be expected under NOVA Upgrade conditions.

c. Suitable cryogenic target fabrication will have been demonstrated.

> **Recommendation 1.2: We recommend that funds be provided to refine and validate the laser architecture proposed for the NOVA Upgrade. A 30 cm x 30 cm beamlet of the proposed multipass laser architecture should be constructed and demonstrated.**

The only possible technology for a near-term ignition demonstration is the mature solid state laser. Furthermore, large savings in time and money would accrue if the ignition demonstration were to take advantage of the existing experience, infrastructure, and building at LLNL. A prototype beamlet is essential, as the proposed multipass architecture differs from the current NOVA design. The committee endorses LLNL’s Laser Technical Contract defining milestones for the design and construction process (see Appendix III). This plan has been presented to, and scrutinized by, the broad ICF laboratory community. LLNL’s long experience with glass lasers gives us some confidence in success. When the beamlet has been successfully tested, we also expect that LLNL’s cost estimate (concurred with by LLE) of less than $400 M (plus operating funds) will have been validated. There is also the very attractive option (at some added cost) of constructing a full (16-beamlet) 150-kJ prototype beamline that could be used for experiments in the weapons program.

> **Recommendation 1.3: We recommend that the proposed Omega Upgrade be started immediately. It will contribute to the technology and physics expertise needed for an ignition demonstration through the NOVA Upgrade. It will also be able to explore the option that the NOVA Upgrade target chamber be configured for direct drive.**

There has been significant progress in recent direct drive experiments and calculations. Indeed, it is possible that this approach will ultimately prove to be the best path to ICF. LLE and NRL are the main centers in the United States pursuing this approach. The LLE program has been very productive, inventive, and cost effective; it is also an important university connection to ICF efforts in other countries.

The present OMEGA laser has met the milestones defined for it in the Happer report and has produced a solid record of significant additional achievements listed in Section VII.5. OMEGA has been almost fully utilized. Construction of the upgrade is essential to retaining momentum in the direct drive approach and to refining the optimum NOVA Upgrade design.

Recommendations 1.1 through 1.3 imply a focusing of the national ICF Program. Their successful implementation would require major changes in the sociology of the national laboratories involved. The committee urges the laboratory managements and scientific staff to begin discussions promptly to define an effective means to proceed. These recommendations would also require a change in management style for the multi-laboratory effort, with clearly defined milestones, and would need to be coordinated more closely than is presently the case. It therefore is recommended that DoE form an outside, independent committee to oversee the program and monitor its progress toward an ignition demonstration. (See Recommendation 1.4.) Given a successful implementation of Recommendations 1.1 through 1.3, a final decision to proceed with construction of the NOVA Upgrade can be made with high confidence. Although the proposed time line is tight, this decision could be made as early as 1994. However, project authorization and preliminary planning must begin immediately.

> **Recommendation I .4:** We **recommend that the DoE management of ICF be focused in a strong headquarters office with direct reporting to a senior level such as a Deputy Assistant Secretary. Further, we endorse the Happer report recommendation for the establishment of an Inertial Confinement Fusion Advisory Committee.**

The elevation in status of the ICF Program will help to assure that the important national effort toward an ignition demonstration will receive the necessary high-level attention within the DoE The Inertial Fusion Advisory Committee should play a role similar to that played by such exemplary DoE advisory committees as the High Energy Physics Advisory Panel HEPAP and the Nuclear Science Advisory Committee (NSAC). Its membership would be drawn from the national laboratories, universities, and industry, and it would provide valuable guidance to DoE on a regular basis (perhaps quarterly) in response to specific charges regarding program priorities and facility upgrades. All but one of the members of the present committee also believe that the long time horizon and multi-laboratory character of the ICF Program require that it continue to be funded as a line item in the Defense Programs budget.

Recommendation 2: We recommend that the NIKE laser be completed at an accelerated pace and that funding for the NRL program continue to ensure its vitality.

A KrF laser driver is a prime backup should the glass laser program falter. It is also attractive for direct-drive IFE. NRL has played a valuable role in developing KrF and in the ICF Program more generally. Among its important accomplishments is the demonstration of echelon-free induced spatial incoherence (ISI) for beam smoothing.[4] There will be much to explore with NIKE when it becomes operational, for example, the effects of bandwidth and techniques for beam smoothing. The NRL program using NIKE is a cost-effective effort of the appropriate scale for keeping the KrF option viable.

Recommendation 3: We recommend that the light-ion program at SNL continue at the present **level of effort for the next two years. During this period, the emphasis of the PBFA-II program should be on reducing beam divergence and on well-designed and well-diagnosed target physics experiments at increasing power concentrations. There should be a major review of technical progress in the summer of 1992 to determine whether the beam divergence issues have been adequately resolved and whether PBFA-II should be upgraded to higher energies for greater gain.**

Low cost makes a light-ion driver attractive, but there are many challenges in pursuing this driver option. At the time that Interim Report was released, there had been several recent significant advances, most notably the increase in the rate of PBFA-II experiments, the focusing of proton beams to intensities of 5 terawatts (TW)/cm^2, the improved understanding of collective instabilities that can induce ion beam divergence in the diode, and the advances in Hermes-III pulsed power technology. Nonetheless, overall progress in the light-ion program had been slower than projected by SNL at the time of the Happer report. We therefore deferred any recommendation on the light-ion program until this report. By this time, we expected to be able to assess technical progress toward selection of the preferred lithium source, together with beam characterization and optimization at 10 MV, 3 MA, and 14 milliradians (mrad) divergence, in preparation for focusing experiments in the 10-TW/cm^2 range.

In the eight months since our Interim Report, SNL has surpassed four of the five milestones established in December 1989, to measure progress in the light-ion program. Significant achievements during this period include the following:

- successful testing of a lithium diode at full energy;
- efficient coupling of a plasma opening switch to an electron diode;
- the development of time-dependent diagnostics; generation of a high-power, high-purity lithium beam at moderate power; and

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[4] In early versions of ISI, a broadband laser output beam is split, by external stepped echelons, into many effectively incoherent beamlets which are then brought to focus on a target. In echelon-free ISI, a spatial filter imposes an intensity profile on the output of a broadband multimode laser source. This profile, which consists of many small independent zones which take the place of the beamlets in ISI, is projected through the laser chain to form a symmetric drive pattern on a target.

- analytical and numerical identification of the collective instability mechanism that is likely to be responsible for increasing beam divergence above acceptable levels.

To achieve the power concentrations that eventually will be required for moderate gain and/or ignition-scale target experiments, it is essential that high priority continue to be given in the near term to reducing beam divergence. We also recommend that SNL draw upon the expertise of the other laboratories in developing advanced target designs with internal pulse shaping.

Recommendation 4: We recommend that no substantial TCF funds be used to upgrade the AURORA facility.

There are several potential advantages of the KrF laser, but it is clear that the development of gas lasers lags significantly behind glass. As noted above, an ignition demonstration with a glass laser can be considered seriously now, but a multi-facility, multi-decade program would be required to reach the multi-megajoule level with a KrF driver. It is apparently difficult and costly to configure AURORA for two-sided target implosions with adequate brightness, which might bear on an ignition demonstration. While further experiments with the facility operating at its current potential could add to the basic physics database, they would have little bearing on the ignition demonstration.

Some additional concerns about the AURORA laser facility that arose since our Interim Report and led to this recommendation are discussed in Section V11.2. The importance of an ignition demonstration warrants devoting additional financial resources to its achievement, even **at the expense of the** AURORA program, if necessary.

Recommendation 5: Unless substantial increased funding becomes available, we recommend against further experiments in the Halite/Centurion program.

The Halite/Centurion program was one of the highest priorities of the Happer report. It was recognized to have a finite lifetime, then estimated to be about 5 years. Since that time, an outstanding interlaboratory cooperative effort has successfully performed some complex Halite/Centurion experiments that have provided extremely important data. Because of these successes, the committee now believes that uncertainties in ignition arise only from considerations of mix, symmetry, and laser-plasma interactions-phenomena that can be studied best in laboratory experiments. Thus, while further Halite/Centurion experiments more directly addressing ignition conditions would be reassuring, they would still leave doubts as to whether mix and symmetry considerations are really the same as when laser-plasma effects were important. Because of the complexity, cost, and longer time scale for the next proposed Halite/Centurion experiment, the probable need for several such experiments, and the difficulty of underground tests, we do not feel that further Halite/Centurion experiments should be funded unless a substantial increment is added to ICF funding.[5]

Recommendation 6: We recommend against funding for research to investigate the suitability of hydrogen fluoride (HF) lasers for ICF.

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[5] Since this recommendation was made in the Interim Report, we understand that the Halite/Centurion program has effectively stopped at the laboratories.

There is no experience with pulsed chemical (HF) lasers anywhere near the required scale. The committee shares the prevailing view of the ICF laboratory community that a retreat to longer wavelengths makes little sense given the bad experience with 1-μ and 10-μ radiation. Although it is claimed that longer wavelengths could be tolerated on a large (100-MJ) scale, all present indications are that a driver at least an order of magnitude smaller than this will be adequate for an LMF or IFE. Given the absence of compelling arguments or any institutional support, we do not recommend starting a new HF driver program.

The proponents of an HF laser driver have done a remarkable amount of work on a very small budget, and while they have made an arguable case, it is far from a compelling one. At long wavelengths, in addition to worries about filamentation and hot electrons, it seems difficult to avoid large undesirable critical surface motions. In such a complex situation, an experimental database is essential. Obtaining one would require developing a novel large laser, which experience shows to be always slower and more costly than is expected at first.

At best, the committee views an HF driver as a backup to several backups (KrF and light ions); it is one of many candidate drivers. In a situation where inadequate budgets are forcing us to recommend constriction of other driver programs that are considerably more promising and advanced than HF, we cannot recommend pursuing the HF option. Indeed, if it did turn out that a 100-MJ driver were required for ignition and gain, one would have to rethink the entire approach to, and rationale for, ICF.

> Recommendation 7: Because the most immediate ICF applications are those related to defense, we recommend that the DoE continue to recognize ICF as primarily a defense program, rather than an energy program.

Energy applications of ICF are more difficult than defense applications. A program to lay the basis for an ICF option for producing a substantial fraction of the nation's energy will require development of a major driver, different from that for an ignition demonstration or for an LMF; this driver would have to be capable of a high pulse repetition rate and be reliable and low in cost. The program would include reactor design studies and technology and target development. We concur with the FPAC outline of the most promising path to an energy option, which recommends demonstration and evaluation of driver technologies that have the potential of satisfying reactor requirements, specifically an enhanced heavy-ion driver research program and, with lower priority, KrF and light-ion drivers. (Details can be found in the final FPAC report of September 1990.)

A heavy-ion driver is unsuited for an ignition demonstration, or for an LMF, but is likely the best choice for energy production. Good progress has been made in heavy-ion driver development, but some important issues are yet to be resolved; heavy-ion driver technology is not sufficiently developed to be ready if ignition is indeed demonstrated by the end of the decade. The committee recommends that a well-developed plan for addressing these issues be formulated, perhaps based on LBL's ILSE proposal. Subject to a thorough review by independent accelerator experts[6] and a careful comparison with other demands on funds (which our panel has not done), consideration should be given to expanding research in heavy-ion drivers to incorporate the next ILSE-scale project with enlarged institutional participation. LBL estimates that the ILSE project would cost a total of $15 M to 20 M/ year for about 4 years.

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[6] Since the release of the Interim Report, two such reviews have been held.

Energy applications could be possible only after many years of development. Meanwhile, the target physics investigations, the ignition demonstration, and the alternative driver research recommended above are on the critical path to energy, as well as to defense applications. Energy applications should be considered in a major way only after ignition has been demonstrated. Since fusion energy is a long-term prospect, we believe that alternative energy **sources,** such as advanced fission reactors, must be developed and employed as a bridge between the present situation and an ultimate fusion capability.

> **Recommendation 8: We recommend that the Secretary of Energy form a panel to review present** ICF **classification guidelines and to schedule future target physics declassifications.**

Classification of target physics information beyond a level necessary to hinder proliferation of nuclear weapons impedes the progress of ICF research. It restricts the size and variety of the community that can contribute to, criticize, and evaluate the target physics program. This situation is already limiting progress for both the defense and energy programs. Classification and concerns about critical technology transfer also limit participation by universities and industry, as well as international collaboration.

The review we recommend should also include an evaluation of how to deal with future international developments that might result in the publication of currently classified material. There are other regulations and practices of DoE and the Department of State that go well beyond classification in making international interchanges difficult to arrange and less productive. These policies should also be reexamined and relaxed where possible.

> **Recommendation 9: We recommend that a larger fraction of the ICF Program’s target fabrication capabilities be located at the program’s driver facilities.**

In the face of very restricted budgets, it is essential that all ICF funds be used for the maximum benefit of the program. As we have explained in Recommendation 1, target fabrication is as much a part of the ICF research frontier as is performing experiments or developing drivers, and advances in target fabrication capabilities are essential for an ignition demonstration. Currently, some target fabrication activities are performed by an outside contractor, and some by the individual laboratories involved. Locating a greater fraction of the target fabrication activities at the laboratories would allow a closer contact with the experimental programs and a better tailoring of targets to the requirements of each individual driver. These advantages must be balanced against needless duplication of facilities and expertise.

VII. CONCLUSIONS AND RECOMMENDATIONS FOR THE INDIVIDUAL LABORATORIES

An important part of our charge is to assess the performance and potential of the individual laboratories in the ICF Program. We respond to this in the following.

VII.1 Lawrence Livermore National Laboratory

Experiments, design, and upgrading the NOVA laser accounted for about 85 percent of the LLNL ICF budget last year. During this time, the laser operated for the first time at its design rating of above 100 kJ at 1.06 μ. This accomplishment was delayed by several years because of both faulty laser glass, and insufficient funding. Up to 70 kJ can be converted into the blue (3ω), and intensities up to 5 x 10^4-TW/cm^2 range have been demonstrated. NOVA is now, by roughly an order of magnitude, the world's most powerful ICF laser driver.

NOVA has already performed a number of key experiments including studies of neutron yield and symmetry as a function of convergence and aspect ratio, as well as filamentation and SRS studies at various intensities and wavelengths. These preliminary experiments are generally in agreement with expectations. Most recently, encouraging results on the beneficial effects of pulse shaping have been obtained and the experimental achievement of high-temperature hohlraums with satisfactory implosions has been of particular interest. This latter accomplishment is the basis of the ignition demonstration proposal. It is our assessment that the technological effort, the experimental expertise, and the theoretical and computational support for the NOVA program are all very high quality.

The accomplishments described above give some confidence that NOVA will meet its experimental goals in the next 3 to 4 years if an additional $25 M per year is devoted to NOVA experiments and operations. These goals include the HEP program, which should produce implosions with mix growth factors, convergence, and hotspot and pusher compressions in excess of what is now thought to be necessary for high gain targets. Further investigations of holhraum physics (the HLP program) are also needed and planned, although it may be more difficult to establish equivalence in this area.

The following sections define our expectations for the progress of the LLNL program during the next 4 years. We estimate a budget of $95 M annually to accomplish the objectives set forth. As discussed in Recommendation 1, a large part of these funds would be assigned to the proposed national ignition demonstration.

VII.1a. Target physics

LLNL has proposed a campaign of 3 to 4 years to support current predictions of ignition and modest (5-10) gain for driver energies of 1 to 2 MJ. We concur with the milestones proposed by LLNL, which are elaborated in Appendices I and II. They include the following:

- Targets with a linear convergence ratio up to 30, mix growth factors from 20 to 1000, fuel densities greater than 40 gm/cm^3, and corresponding pusher compressions.

- Attainment of these objectives depends on developing "Precision NOVA," characterized by 5 percent beam power balance in blue light, IO-mrad targeting accuracy, and 40: 1 pulse shaping. Further, an impressive set of diagnostics (4-p neutron imaging, neutron time-of-flight adequate for 1-μ resolution, 30-picosecond x-ray framing, 5-picosecond x-ray streak, and 10- to 20-picosecond x-ray) will be needed to successfully diagnose these extreme conditions.

- Issues of plasma stability and hohlraum robustness are more difficult to characterize by quantitative milestones. Separate campaigns involving single-beam experiments at relevant intensities and plasma scale lengths will be performed, and the effects of SSD smoothing will be characterized. Studies of plasma behavior in the hohlraum will require extensive simulation to extrapolate convincingly to the ignition demonstration.

VII.1 b. Advanced drivers

LLNL has proposed to construct a 30 cm x 30 cm beamlet, the basic architectural element of the proposed NOVA Upgrade, which will test the new multipass concept. We view this as essential to approval of full construction. It would be even more satisfactory if a full 4 x 4 beamline could be constructed for an additional $30 M, but we don't view this as essential.

VII.2 Los Alamos National Laboratory

Our perspective on the role of LANL in the national ICF Program has changed greatly as a result of the proposed ignition demonstration and our closer scrutiny of AURORA's capabilities. In our Interim Report, we made the following observations and recommendations concerning the LANL program:

> The first KrF laser operated approximately one decade after the first glass laser. Development of KrF lasers for ICF was initiated considerably later and has received considerably less total funding than the glass laser program. Consequently, the LANL ICF program lags the LLNL program. Additional funding in FY89 allowed the AURORA facility to make impressive strides and 100 TW/cm^2 have been achieved on target. As of December 4, 1989, AURORA was putting 1.3 kJ in 36 beams on target, with spot sizes smaller than 400 μ, pulse widths ranging from 3 to 7 nanoseconds, and peak irradiance larger that 200 TW/cm^2.
>
> Many advantages have been claimed for KrF lasers over glass lasers for ICF:greater electrical efficiency, wider bandwidth, shorter wavelength (which couples energy to the capsule more efficiently), and superior pulse shaping capability. To evaluate these claims, in the next 12 months AURORA should be made fully operational at the original specifications, operate reliably, (2 to 3 kJ to target, 5 shots per week) and achieve smaller focus. Also of high priority is an upgrade of the Intermediate Amplifier, an upgrade of the window for admitting electrons to the Large Amplifier, and a modified front end for pulse shaping and increased bandwidth. It is also important to initiate work on target diagnostics and a backlighter, taking advantage of techniques and instrumentation developed at other ICF laboratories.
>
> Important improvements to AURORA that may take up to two years are to increase the energy on target to 10 kJ by optics changes and to 20kJ

> to target with the bandwidth increased to 300 cm-‘, so that a full implosion campaign can be undertaken.
>
> Important experiments to carry out with AURORA are a study of unstable hydrodynamics in a planar system using a variety of temporal pulse shapes and with varying beam smoothing techniques. AURORA can also test important questions in hohlraum and plasma physics (especially filamentation) including long scale lengths, long pulse lengths and various degrees of spatial beam smoothing. The effect of modest bandwidth can also be tested.
>
> With the steps we have outlined above, AURORA will become an excellent implosion facility to test the capabilities of KrF lasers for ICF research and also to test many essential aspects of KrF laser design. Comparison with NOVA (perhaps through a coordinated set of comparable experiments) could yield valuable insights on the relative ease and importance of smoothing, bandwidth enhancement, and pulse shaping in glass and KrF lasers.
>
> We urge that LANL weapons and ICF personnel make greater use of the NOVA facility. NOVA’s growing relevance for the weapons program should not be limited to LLNL. LANL experiments on NOVA can help in achieving a convergence of understanding between the two laboratories, as well as benefit the LANL weapons effort and the AURORA experimental program we have recommended.

It now appears that it would be too difficult and expensive to activate the additional 48 beams and enable a full implosion program to be implemented. Moreover, the AURORA laser cannot be used to prove that the NOVA Upgrade laser will succeed in reaching the goal of ignition in an x-ray driven hohlraum target. The inverse question, “Can it be used to show significant areas of risk?” has been examined by both LLNL and LANL at our request. Informal reports from both laboratories pinpoint three areas of interest: laser-plasma coupling, x-ray conversion efficiency, and hydrodynamic or other hohlraum stability issues.

Although there are physics experiments that could be done with AURORA, the low peak power limits the achievable radiation temperatures to less than one-half of those already realized on NOVA; the resulting ablation pressures would be smaller by a much larger factor. Additional limitations of AURORA include the relatively large spot sizes (0.5 mm or greater) and the restriction to single-sided illumination.

The proposed NOVA Upgrade would operate at 0.35 μ while KrF lasers operate at 0.25 μ. It is probable that the shorter wavelength light would be more effective in desirable laser coupling to the plasma, but the parameter range in which AURORA could examine this issue has already been surpassed significantly by NOVA with only very small SRS being observed. Similarly, experiments at NOVA have measured x-ray conversion efficiency under conditions that surpass what would be possible at AURORA. Also, earlier Novette experiments at 4ω demonstrated high x-ray conversion efficiency at much higher intensities than are possible with AURORA.

The single-sided illumination geometry of AURORA limits most types of hohlraum experiments. There are some experiments that could be done with parameters relevant to the long “foot” on the leading edge of a high-intensity pulse. Because the NOVA Upgrade would use fewer beams in this part of the pulse, it is important to examine stability and conversion issues here as well. We note that the LLNL Target Technical Contract (Appendix I) outlines an experimental campaign that will examine conditions relevant to this part of the pulse.

Given this perspective, and the fact that AURORA has just begun doing laser and target experiments, the best course for AURORA appears to be a program that would define some of the practical requirements for large KrF drivers, as well as some comparisons with glass laser experiments. The effects of bandwidth, pulse shaping, and smoothing could also be investigated.

While this AURORA program would be a significant contribution to the ICF program and very desirable to carry out if funding could be found, we now recognize that funding limitations may well severely curtail the development and use of the AURORA facility if the priorities of our recommendations are followed. Of course, we envision that LANL will become a major participant in the ignition demonstration program outlined in Recommendation 1.

VII.3 **Sandia National Laboratories**

The rate of progress in the light-ion pulsed power program has been impressive during the past three years. Initially, the overall progress was slower than expected immediately following the Happer report. However, in mid-1987 the experiment rate in PBFA-II increased to about one shot per day. Although beam divergence was initially larger than anticipated, protons were successfully focussed to 5 TW/cm^2 by April 1989. In parallel with beam focusing experiments, there have been several advances in the development of lithium ion sources and improved plasma opening switch configurations. It is the committee's assessment that the SNL staff is of very high quality and is a world leader in the areas of advanced pulsed power engineering and technology development. The laboratory is commended for establishing a high-calibre scientific team that integrates experiment, theory, and engineering design in the PBFA-II program. The light-ion effort at SNL is augmented by excellent related programs in experiment, theory, and system studies at Cornell University, NRL, and the University of Wisconsin.

There has been a long and concentrated development program for lithium ion sources with the laser evaporation source (LEVIS) and LiF sources emerging as the most promising candidates in mid-1990. Other sources, such as the boil-off lithium vapor source/laser ionization based on resonance saturation (BOLVAPS/LIBORS) source and the electrohydrodynamic ion source are also being investigated for high-repetition rate applications, although such a capability is not needed for an ignition experiment or LMF.

The advances in pulsed power technology since the Happer report have been outstanding. The development of HERMES-III (which was completed under budget and ahead of schedule) at 20 MV has demonstrated that a ramped voltage can be used for beam bunching on a 40 nanosecond time scale. The low cost of this technology for an LMF could reduce the driver cost relative to a laser driver, if the beam focusing problems can be solved.

At the time of our Interim Report, we established a series of milestones for the light ion program to pass by the end of summer 1990. The scientists at SNL met or exceeded four of the five milestones (PBFA-II operating at 10 MV, 3 MA, producing a lithium ion beam of at least 75 percent purity at an ion production efficiency of 80 percent). SNL did not meet the milestone of 14-mrad divergence for the lithium beam (best value was 23±2 mrad), although they did develop an impressive array of diagnostics and analytical and numerical models that identify the likely collective instability mechanism and possible ways to reduce the beam divergence.

Detailed milestones should be established for the next two years of the PBFA-II program, and technical progress should be monitored closely. Major emphasis should be placed on reducing ion beam divergence and increasing power concentrations to acceptable levels. In addition, it is imperative that well-diagnosed ion target and hohlraum characterization experiments be carried out. The committee strongly recommends that SNL draw on the expertise of other laboratories in developing advanced target designs with internal pulse shaping.

VII.4 **Naval Research Laboratory**

There is a relatively small but innovative part of the ICF Program at the NRL. NRL is a proponent of direct-drive. It has been a pioneer in experimental and theoretical studies of Rayleigh-Taylor instabilities, and has provided much of our understanding of how ablation suppresses the potentially fatal growth of these instabilities and makes it possible to contemplate direct-drive targets.

The NRL group was also the first to demonstrate the value of induced spatial and temporal incoherence for ICF lasers. One of their concepts, echelon-free ISI, will work particularly well with the large bandwidths of KrF lasers. This concept, together with the favorable short wavelength of a KrF laser, led NRL to begin construction of the NIKE laser. Cooperation between the NRL and LANL laser groups will help to ensure that KrF lasers for ICF applications are developed in a sensible way.

An acceleration of the NIKE construction schedule is appropriate to more rapidly explore the potential of KrF lasers; periodic reviews of the construction progress by a competent oversight group are recommended.

Experiments with the NIKE laser can address important physics issues of nuclear weapons, for example, growth rates of Rayleigh-Taylor instabilities. Like the LLE, NRL has provided informed and constructive criticism of the larger ICF efforts at the national laboratories.

VII.5 Laboratory for Laser Energetics

There have been a number of significant accomplishments by the LLE at the University of Rochester since the Happer report. Among them are the following:

- Development of smoothing by spectral dispersion (SSD), which reduced the overall rms inhomogeneity of illumination from 27 percent to 4.6 percent.
- Improvement in the power balance of the 24 beams of OMEGA to 1 to 3 percent, routinely.
- Implosions of cryogenic targets on the OMEGA laser to compressions 100 to 300 times liquid hydrogen density.
- Implosions of gas-filled capsules in good agreement with one-dimensional calculations over much of the implosion (up to convergence ratios of about 25).
- Development of a variety of liquid crystal systems, including polarizers (incorporated into OMEGA beginning in 1986), apodizers, and polymer systems. Other outstanding technical achievements are an ion-assisted deposition technique for coatings, high-damage-threshold thin film linear polarizers, and a thermal conductivity technique for measuring thin films.
- Development of a broad array of x-ray diagnostic capabilities.
- Development of techniques for coating selected KMS microspheres with high-quality ablation and metal layers, as well as x-ray and interferometric techniques for characterizing capsules.

- Design of the baseline configuration of an upgraded OMEGA system that will provide 30 kJ of blue light in 60 beams.

This is an impressive list of solid accomplishments in a focused program exploring direct-drive ICF. In addition, LLE has continued to be a valuable source of graduate students trained in ICF physics and laser technology. During the next five years, we expect that the LLE will continue this track record as follows:

- Exploring and developing improved SSD as needed.
- Pursuing further the understanding of the importance of bandwidth in judging the relative merits of various laser drivers.
- Continuing OMEGA experiments to better understand the discrepancies between measurements and calculated one-dimensional performance.
- Intensifying efforts to improve capsule fabrication capability,with special emphasis on using advances from the international ICF community.
- Constructing the upgraded version of the OMEGA laser with a flexible configuration in anticipation of experiments that speak to varied and sometimes unexpected capsule physics issues, in particular, exploring the possibility of operation with green light and alternate smoothing schemes. The OMEGA and NOVA Upgrades have some technological aspects in common, so that rapid completion of the former would be beneficial to the latter.
- Initiating hydrodynamically equivalent and ignition scaling experiments on the upgraded OMEGA system, with results bearing on the possibility of configuring the NOVA Upgrade target chamber for direct drive.

VII.6 KMS **Fusion, Inc.**

In the course of the ICF Program, KMS has developed a capability to routinely supply ICF laboratories with quantities of glass microspheres from which to select subsets of capsules with those properties (e.g., sphericity, and uniformity of thickness and of density) that are required for various experiments. They have also developed the beta-layering technique for cryogenic targets, which is likely to be of great importance for LMF-scale targets.

The ICF Program’s current capsule fabrication capability and a continued effort to invent and implement advanced fabrication techniques are of significant value to the ICF Program and continued support for these activities is mandatory.

It is our understanding that KMS now operates under a 3-year DoE contract and that DoE has reopened competition for that contract. In the near future, the need for great flexibility in capsule design will require even greater imagination and skill in capsule fabrication. During the next 5 years, we expect that the ICF Program will do the following:

- Develop the capability to make plastic shells with the required accuracies for the (ever-changing) requirements of capsule design.

- Develop and/or refine coating and layering techniques so that both room temperature and cryogenic capsules of sophisticated design and adequate uniformity can be produced.

- Acquire an awareness of those target fabrication techniques that are being developed throughout the international ICF community and adopt or adapt those relevant to the needs of the U.S. ICF Program.

VII.7 **Lawrence Berkeley Laboratory**

For more than 10 years LBL has been a significant participant in efforts to understand and develop systems for commercial ICF energy production using a heavy-ion driver. Most recently, LBL has served as the focal point for these efforts with continuing interest from LLNL and to some extent from LANL, Standard Linear Accelerator Center (SLAC), and the University of Wisconsin. Currently, funding support for the program is provided by the office of Basic Energy Sciences (BES) within Energy Research.

Since 1985 considerable progress has been made. Demonstrations of sources of heavy ions, transport of multiple, space charge dominated beams, and current multiplication by shaped acceleration (all with very small brightness dilution) have been carried out, albeit at low-beam current. A multiple beam source and injector that could serve as a model for a HIF driver is also well along in development.

In 1987, the Heavy Ion Fusion Systems Assessment (HIFSA) study was published. Led by LANL, with significant participation from LBL, LLNL, McDonnell Douglas, the University of Wisconsin, SNL, and SLAC, the study showed that, given the anticipated fusion target performance, the linear induction accelerator is a prime candidate for an energy production driver. The U.S. program is investigating the induction linear accelerator as a heavy-ion driver and there is some work in the USSR using this approach; there is substantial work in Germany on the RF linac driver option.

Achievements in heavy-ion fusion (HIF) at LBL are significant, particularly in light of the rather modest resources that have been invested to date. However, important issues in beam transport and stability, as well as essential component development, need to be addressed before the apparent superiority of the induction linac driver is confirmed. Some of these can be addressed at modest current in the LBL-proposed Induction Linac Systems Experiment (ILSE), or a variant of it. High current beam stability under acceleration and transport can be dealt with preliminarily using available simulation codes, supplemented by three-dimensional field codes that can account for the time-dependent fields in the full electromagnetic environment of the beam, including focusing and acceleration apparatus.

Support for such a development program, with its mission, goals, and schedules, is unlikely to come through BES; both the size and the nature of the program are inapropriate to BES's role of supporting small research groups and user facilities. A more appropriate point of attachment in DoE would be a new Division of Inertial Fusion Energy within the Office of Fusion Energy.

Appropriate milestones during the next several years would be as follows:

- Completion of the Multiple Beam Experiments with 4 beams (MBE-4).

- Completion of the 2-MV, 16-beam injector.

- With high priority, formulation and completion of a simulation campaign, perhaps in conjunction with others, to evaluate the whole of the heavy-ion driver beam behavior, particularly dynamic space charge effects.

- Formation of an institutional collaboration to propose a follow-on experimental campaign in light of the three efforts above. This should envision the eventual transfer of the center of larger-scale experimental activity to a more appropriate physical and institutional environment.

- Reviews of the proposals for this collaborative experimental campaign by independent panels of accelerator and fusion experts.

Relevant and complementary research on heavy-ion drivers is also being carried out in Europe, where programs are developing rapidly. Soon to be in operation in Germany is a major new facility for heavy ion research, SIS/ESR. Experiments using this facility, though not fully duplicating conditions in the United States, will provide valuable information on the conversion efficiency as a function of beam intensity and other parameters; they will also be relevant to determining target conditions. Other programs being discussed in Europe may result in a renewed heavy-ion driver development effort there. It will be important for U.S. workers in heavy ions to continue to be well informed of these efforts and, in some cases, to collaborate on them.

VIII. BUDGET CONSIDERATIONS

Table 1 shows the recent funding history of the Office of Defense Program's ICF Program. Actual funding has fallen short of the **Happer** report's recommendation of constant funding at the FY85 level (after correction for inflation).

TABLE 1

ICF Program Operating Funds ($M)

	Happer Report Recommendation	Actual	Shortfall	Integral Shortfall
FY85	154.8	154.8	0	0
FY86	156.1	137.9	18.2	18.2
FY87	157.3	142.7	14.6	32.8
FY88	165.2	151.0	14.2	47.0
FY89	173.5	155.5	18.0	65.0
FY90	187.1	160.6	26.5	91.5

The integral shortfall through FY90 is about $91.5 M, or an average of about $18 M per year, about 12 percent of the program. This underfunding has contributed to the program's failure to progress as rapidly as envisioned in 1986.

Table 2 shows the FY90 ICF operating budget for each of the laboratories.

TABLE 2

FY90 Laboratories' ICF Operating Budgets ($M)

LLNL	61.434
LANL	3 1.045
SNL	27.376
KMSF	17.040
LLE	11.032
NRL	4.753
Other (DoE HQ)	1.937
Total	160.6

Recommendation 1 should allow the ICF Program to make orderly progress toward a decision on the ignition demonstration facility at a rate commensurate with time required to resolve the issues remaining before a final commitment is made. Implementing Recommendation 1 would require increased funding over present levels, or reductions in other components of the ICF Program.

To implement the program of Recommendation 1, we estimate the following additional costs (averaged over a 4-year period):

- Precision NOVA Upgrade and experimental program, $25 M/year
- OMEGA Upgrade, $15 M/year

We also recommend a small additional expenditure of $2 M/year to complete the NIKE laser on an accelerated schedule (Recommendation 2). These additional costs are partially offset by an estimated $20 M/year from termination of the Halite/Centurion program. We estimate that the net result of these changes, using 1990 figures for the ICF operating budget, would be about $18 1 M, somewhat short of the level recommended by the **Happer** report which would be about $187 M for that year.

This budget would permit continuation of the SNL light-ion program at a constant level of funding, as well as the ICF Program at LANL. The precise funding required for the latter will depend on the details of the redirecting of the LANL effort discussed in our Recommendation 4. Some additional funding would be needed to support a final commitment to proceed with the ignition demonstration facility (NOVA Upgrade). Based on a construction start in FY94, $10 M to $15 M of funding would be required in FY92 to demonstrate a prototype laser **beamlet,** and $9 M for final design and other preconstruction activities.

Table 3 shows the recent history of **DoE** funding of heavy-ion drivers. Implementation of Recommendation 7 concerning heavy-ion driver research would require increasing the current level of support to $15 M to $20 M/year.

TABLE 3

OER Funding of HIF Research ($M)

FY86	FY87	FY88	FY89	FY90
5.1	5.6	6.0	6.0	6.3

Appendix I
Target Physics Technical Contract

This appendix includes unclassified excerpts from the Lawrence Livermore National Laboratory (LLNL) technical contract specifying the target physics milestones to be passed in preparation for an ignition demonstration through the NOVA Upgrade. Complete classified or unclassified versions of the full document can be obtained from LLNL.[7]

Introduction

The target physics program has two principal elements: (1) hohlraum/plasma physics, which addresses driver-plasma coupling, generation and transport of x-rays, and the development of energy efficient (i.e., coupling of the driver energy to the capsule) hohlraums, which provide the appropriate x-ray drive (spectral, temporal, and spatial) to a high-performance capsule; and, (2) hydrodynamically equivalent physics (HEP) program which, addresses the capsule physics associated with ignition and gain. Elements of this program include hydrodynamic stability, the effects of drive non-uniformity on capsule performance, and the physics associated with ignition (energy gain/loss to the fuel) in the absence of alpha deposition.

While the demonstration of specific milestones is an important aspect of these efforts, the emphasis of the target physics program is to demonstrate and refine our ability to model/predict target performance, particularly those aspects which scale to and influence ignition and gain. The Program is thus directed towards instilling confidence in the successful outcome of ICF and of specifying the performance and characteristics of both the driver and targets required to achieve the objectives.

We expect to undertake and complete the program described below in 2-3 years, depending upon the funding **level available.**

Hohlraum/Plasma Physics (HLP)

An extensive experimental and modeling hohlraum physics effort has been under way at LLNL for over a decade. In particular, substantial progress has been made in the past several years on NOVA. As a result, the NOVA Upgrade hohlraums are in many aspects, an extension of the ongoing experiments. For example, the ignition/moderate gain hohlraums are typically only several times larger than that of the nominal NOVA targets.

Present calculations give performance specifications for the NOVA Upgrade hohlraums, including:

1. Drive conditions that generate the necessary ablation pressure to implode capsules.
2. Adequate time average drive uniformity in energy efficient hohlraums.
3. Acceptable hot electron production. The exact levels are dependent on the target.

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[7] The document numbers are UCRL-TB-104287 and UCRL-TB-104288 for unclassified Parts I and II.

4. The amount of scattered light from Stimulated Brillouin and Raman Scattering. The exact values depend primarily on the influence of this scattering on drive symmetry control.

The objectives of the NOVA experiments are to experimentally demonstrate and predictively model hohlraum performance in properly scaled targets consistent with NOVA's performance limitations and to further develop our understanding (experimental and theoretical) of laser-plasma interaction physics in the plasmas associated with hohlraums. Where appropriate, the hohlraums will also contain capsules to better simulate the NOVA Upgrade targets.

The hohlraum/plasma physics program will also continue to explore, within the capabilities of NOVA, the limits of achievable peak temperature in laser driven hohlraums. While specific objectives will not presently be assigned to this task, these experiments will help define the "operating experimental parameters" of the NOVA Upgrade.

Specific milestones and objectives of the Hohlraum/Plasma Physics Program are the following:

(HLP1) Demonstrate acceptable hohlraum-plasma coupling and gross hohlraum energetics with targets (properly scaled from NOVA Upgrade designs) with temporally shaped pulses.

The goals and objectives for HLP1 are:

Acceptable hohlraum coupling, with specified values of:

- Absorption fraction
- Stimulated Brillouin fraction
- Stimulated Raman fraction
- Hot electron fraction

(HLP2) Demonstration of acceptable hohlraum-plasma coupling with peak equivalent radiation temperature appropriate for the NOVA Upgrade. As the power (40 TW) of NOVA limits the target scale and pulse duration for these high temperatures, specified laser pulse formats and appropriately scaled pulses (i.e., shorter overall pulses or more limited shaped pulses compared to those utilized in HLP1) will be used.

The goals and objectives for HLP2 are specified values of:

- Absorption fraction
- Stimulated Brillouin fraction
- Stimulated Raman fraction
- Hot electron fraction

All of these values are to have a peak hohlraum temperature appropriate for the NOVA Upgrade.

(HLP3) Hohlraum experiments with a range of temperatures obtained with a variety of pulse formats and targets. Hohlraums up to ignition scale, albeit at lower temperature and shorter pulse duration, will be examined. Measurements will focus on the x-ray environment within the hohlraum.

This task will also include characterization of x-ray and laser driven ablation plasmas.

The goals and objectives for HLP3 are:

- Confirmation of our ability to calculate energy balance in a hohlraum and to model the details of the plasma within the hohlraum.

(HLP4) Demonstration of symmetry control with advanced hohlraums. Experiments will utilize a variety of x-ray imaging techniques to measure time-integrated x-ray drive asymmetry in low order l modes. Supporting experiments will also be conducted.

The goals and objectives for HLP4 are:

- Specified low order l–mode asymmetry in an advanced hohlraum with pulse shaping properly scaled to ignition targets. Peak radiation temperatures will be consistent with NOVA's power and focusing capabilities. The achievable flux uniformity is limited by the limited number of NOVA's beams (10) and potentially by the individual beam quality. Experiments will also establish our understanding of time dependent drive asymmetry. The experiments will both achieve the stated level of flux uniformity with NOVA and demonstrate our quantitative understanding of the limitations (physics and technology) so that flux uniformities (both instantaneous and time integrated) required for ignition/moderate gain will be achieved on the NOVA Upgrade.

- Experimental demonstration of time integrated flux asymmetry control. These experiments, supported by quantitative modeling, will further demonstrate our ability to control and achieve the flux uniformity required for ignition/gain targets.

(HLP5) Stimulated Brillouin and Raman scattering in long scale-length plasmas. This task will quantify levels of stimulated Brillouin (back and side scatter) and Raman scattering (back, side, and forward) in plasmas. The present NOVA hohlraum experiments, in which good laser-plasma coupling is observed, have many relevant features that are nearly equivalent to an ignition hohlraum but differ in scale-length and plasma dimensions. As such the experimental goals described in HLPI, HLP2, and HLP4 will continue to be relevant.

Experiments covered in this task will also involve the exploitation of open geometry targets such as exploding foils/disks and will focus on long scale-length plasmas. Plasma conditions will be characterized and experimentally controlled (for example, allowing sufficient delay between the plasma forming beams and interaction beam to allow hydrodynamic smoothing of density ripples and bumps and to reduce fluctuation and other noise sources to thermal levels). Freely expanding and interpenetrating plasma sources will be examined.

The goals and objectives for HLP5 are:

In long scale-length plasmas quantify:

- Stimulated Brillouin scattering fraction
- Stimulated Raman scattering fraction

These experiments will initially be conducted with the narrow frequency ($\Delta\nu/\nu < 10^{-5}$), spatially modulated NOVA beams and thus represent a "worst case" scenario. Experiments, utilizing spatially smooth beams (with smoothing times < 20 psec) are planned as part of the NOVA program. Initial experiments will be conducted on the 2-beam facility in parallel with HLP6 and will eventually be performed on the IO-beam chamber where larger interaction plasmas are possible. Only the interaction beam, however, would have a smooth intensity profile. These latter experiments would be completed after the go ahead for the Upgrade (provided the successful completion of HLP1, HLP2, HLP4, HLP6) and would be complete in time to have an impact on the Upgrade configuration.

(HLP6) Filamentation and its influence on coupling with large scale-length plasmas.

The goals and objectives for HLP6 are as follows:

Experimentally characterize the nonlinear state and the influence/coupling of filamentation with other instabilities (SRS,SBS) in low density plasmas. Experiments will also examine the predicted stabilizing effects of multiple beams, angular divergence, and beam smoothing. This effort will proceed in parallel but extend beyond the completion dates of HLP1, HLP2, HLP4, and HLP6. If objectives in these tasks are not met, then the priority of this activity would be increased with a resulting completion date before the Upgrade construction starts. The results would then be of importance in establishing the baseline beam smoothing requirements for the new facility.

(HLP7) X-ray conversion efficiency physics.

To achieve the hohlraum conditions required for ignition/moderate gain with the output performance of the NOVA Upgrade, sufficient x-ray conversion efficiencies are required. Disk experiments on NOVA have already demonstrated these efficiencies with measured values exceeding 70% (and instantaneous values exceeding 80%) at relevant intensities and pulse lengths relevant for the main pulse of the ignition target.

> In addition, experiments with disks have demonstrated or inferred that x-ray conversion efficiency (at a given wavelength, plasma composition and intensity) increases with pulse length, spot size, and potentially beam quality. While our present modeling can match much of the data, some of the trends seen in the data (i.e., spot size dependence) are not quantitatively understood.
>
> X-ray conversion experiments planned over the next several years will further build upon our extensive data base and attempt to develop an improved modeling capability. Experiments will focus on areas of applicability to the NOVA Upgrade. The power and energy capabilities of NOVA will limit the overall parameter range that can be experimentally addressed.

The goals and objectives for HLP7 are:

> Since the required conversion efficiency, η, (more importantly ηl) has been already achieved and the favorable trends in conversion efficiency as a function of the parameters associated with the NOVA Upgrade (compared to NOVA) have also been observed, we feel no additional milestones are required. The experiments will be used primarily to improve our understanding of the appropriate physics so that we will be able to better predict the range of hohlraum performance that will be available on the Upgrade.

Capsule Physics (HEP)

An extensive program addressing the implosion physics of ignition and gain is presently underway on NOVA. The goal of this effort is to further develop and demonstrate a quantitative and predictive understanding of the performance of capsules (properly scaled from ignition/gain designs) including the effects of hydrodynamic instability and x-ray drive non-uniformities (with known initial fabrication tolerances of the target and a detailed knowledge of the hohlraum environment). The extensive diagnostics utilized in the HEP Program also allows confirmation of our ability to model ignition physics in imploding capsules, i.e., the balance between PdV work and electron conduction and radiative losses in the fuel in the absence of alpha heating.

The HEP experiments utilize both planar targets and capsules with/without prescribed perturbations (incapsule fabrication or x-ray flux uniformity). The targets do not include cryogenic fuel configurations. The experiments are extensively diagnosed with x-ray and neutron diagnostics including x-ray backlighting. The experiments also emphasize pulse shaped drive to enable minimum entropy implosion trajectories to be examined.

Although not one of the principal objectives of the HEP Program, the pulse shaping and the reduced levels of preheat from high energy photons and superthermal electrons lead to non-cryogenic implosions with final fuel densities in the range of 40 g/cm^3. The specific goals/objectives of HEP are the following:

(HEP1) Demonstration of increased fuel/pusher compressions with high contrast pulse shaping with noncryogenic targets. These experiments have been successfully conducted but due to the high compressions achieved, limited diagnostics were employed. For example, the pusher and fuel density increase were inferred from a measurement of the pusher areal density, $\rho\Delta R$.

The goals and objectives for HEP1 are:

- Diagnosis of fuel densities in the range of 20-40 g/cm^3 inferred from measurements of fuel areal density, ρR, using advanced neutron based diagnostics. This quantity and other features of target performance neutron yield, ion temperature, fusion burnwidth) will be modeled taking into account both hydrodynamic stability and x-ray flux non-uniformities.

(HEP2) Measurements of reduced growth and early nonlinear behavior of the Rayleigh-Taylor instability at the ablation surface for x-ray driven targets. The goals of HEP2 are to experimentally demonstrate and model reductions in the growth rate of the Rayleigh-Taylor instability due to finite density gradients and mass advection at the ablation surface. Planar targets with single fourier component area1 density variations will be examined over a range of peak radiation temperatures and with pulse shaping. Initial perturbations will be small and the acceleration history designed so that linear analysis is (ka_o<<1) valid for large growth factors (3-5 e-folds). Successful experiments to date, on which the HEP2 project is based, have observed ~1 e-fold of growth rather then the 2-3 e-folds predicted if finite density gradients and mass advection are not included. Perturbation wavelength and material opacity will be varied to establish an extensive data base for comparison with detailed numerical simulation.

The experiments will also address early nonlinear behavior such as harmonics (due to bubble and spike growth) and examine mode coupling. In these latter experiments the foils will nominally have two initial perturbation wavelengths.

The goals and objectives for HEP2 are:

- Observation of single mode growth factors at the ablation surface >30 from which reductions in the classical Rayleigh-Taylor growth rate of 2-3 are inferred. The experiments will utilize planar targets driven by x-ray ablation. Experiments will be compared to detailed simulations to confirm our ability to model RT growth at the ablation surface.

- Observation of early nonlinear behavior (harmonics) and mode coupling. Planar targets with single or multiple fourier components will be utilized. Detailed comparison with numerical simulations to demonstrate our ability to model this nonlinear behavior will be made.

(HEP3) Implosion experiments utilizing capsules with deliberately perturbed surface finishes to validate mix models with a multimode initial noise spectrum. These “bumpy ball” experiments are designed to validate our multimode hydrodynamic mix models. Detailed time resolved x-ray spectroscopy of dopant materials in both the fuel and pusher are used to infer mix and to compare with our modeling of both mix and its effect on the imploded state of the fuel. While the growth factors for these experiments are relatively low, the initial surface perturbation amplitudes are sufficiently large to result in diagnosable mixing between the pusher and fuel. The target convergence is limited so that the effects of drive asymmetry on target performance is limited. Targets with no deliberately fabricated surface perturbations are used for control and a well defined "null."

The goals and objectives for HEP3 are:

- Observation of pusher/fuel mix that is dependent on initial target surface quality using x-ray spectroscopy. Targets utilize an initial multimode spectrum of surface perturbations. Detailed comparison of experimental results with numerical simulations employing multimode mix modeling will be performed, Experiments will include targets with no deliberately perturbed surface finish as "null" comparisons.

In addition to the HEP1 through HEP3 objectives, a series of sophisticated experiments will be conducted which directly address the hydrodynamic stability of the NOVA Upgrade ignition targets. For example, these experiments will diagnose the performance of capsules when the instability growth factors (including both linear and, where appropriate, nonlinear behavior) as a function of &mode number are similar to that of ignition gain target designs. Other features of the experiments, such as achievable implosion velocity and convergence, will be either limited or influenced by the existing capabilities of NOVA. For example, the limited number of NOVA beams (10) will limit the drive uniformity and thus capsule convergence before degradation from one-dimensional performance will be expected. The NOVA Upgrade addresses this limitation by the use of a large number ($N > 200$) of independent beamlets. Specific laser technology issues addressible on NOVA that impact drive uniformity and that are required for the NOVA Upgrade will be demonstrated as part of the Precision NOVA program. Issues include beam to beam power balance and beampointing.

These experiments (and HLP4) and the associated modeling will specifically identify the performance and flexibility required of the NOVA Upgrade and will further validate our predictive capability of capsule performance including both hydrodynamic stability and drive nonuniformity.

The HEP4 and HEP5 described below are the experimental programs that address these issues. The two projects differ primarily in the associated capsule convergences which impact the choice and applicability of the target diagnostics.

(HEP4) Implosion experiments to low convergences with overall hydrodynamic mix growth factors and ℓ mode spectrum similar to ignition target designs. These experiments, which will make use of techniques developed in HEP3, will utilize capsules whose hydrodynamic stability (number of growth factors) is varied by target design. Capsule convergences will be limited so that the effects of drive non-uniformity on capsule performance will be minimized. The growth factors will be systematically varied from levels exceeding that of ignition targets. This systematic variation in hydrodynamic stability will also ensure that the effects of drive asymmetry are isolated.

The goals and objectives for HEP4 are:

- Detailed diagnosis and modeling comparison of capsules with various features to control hydrodynamic stability. Diagnostics will be primarily (but not exclusively) x-ray based. Hydrodynamic instability growth factors will be similar to ignition designs.

- Targets will be imploded with pulse shaped drive and will be limited to low convergences to minimize the effects of x-ray flux non-uniformity.

(HEP5) Implosion experiments to high convergences with overall hydrodynamic mix growth factors and ℓ-mode spectrum similar to ignition target designs. These experiments, which will make use of techniques developed in HEP3 and HEP4 will utilize D_2 and DT filled capsules whose hydrodynamic stability (number of growth factors) is varied by target design.

HEP5 is similar to HEP4 with the exceptions of higher target convergence and a reliance on both x-ray and advanced neutron based diagnostics to measure imploded core conditions. As stated above, the convergence will be limited by NOVA's finite number of beams and will be as large as possible commensurate with diagnosability (the majority of measurements will rely on target emission). As in HEP4, the growth factors will be systematically varied to levels exceeding that of ignition targets. This systematic variation in hydrodynamic stability will enable the effects of drive asymmetry to be isolated from that due to mix from higher &mode target fabrication "noise" sources.

The goals and objectives for HEP5 are:

- Detailed diagnosis and modeling comparison of D_2 and DT filled plastic capsules with various features to control hydrodynamic stability. Diagnostics will be neutron and x-ray based. Hydrodynamic instability growth factors will be similar to ignition designs.

- Targets will be imploded with pulse shaped drive. Convergences will be high, but at yields degraded from 1-D performance due to the uniformity limitations of NOVA. The achievable convergence will be commensurate with experimental diagnosability.

Appendix II
Cryogenic Target Development Technical Contract

In this appendix, we include unclassified excerpts from the LLNL technical contract specifying the cryogenic target development milestones to be passed in preparation for the NOVA Upgrade. A complete classified or unclassified document can be obtained from LLNL.[8]

Introduction

In establishing our readiness to proceed with the NOVA Upgrade, LLNL will apply a major effort during the next two-to-three years to experimentally demonstrating the scaling validity of our theoretical ICF models used for the design of ignition targets and providing an appropriate technical and cost basis for the NOVA Upgrade. A third, smaller, but nevertheless essential ingredient of demonstrated readiness is establishing the techniques for providing the targets for the facility. The major new attribute of these targets is that the majority of the fuel is condensed as either a liquid or solid spherical shell, and is therefore at cryogenic temperatures. In this paper we describe the techniques we will explore to fabricate these targets. Our goal is to demonstrate that we have fabricated a representative target, and have determined through measurement that it meets the appropriate specifications

Beta Layering

The redistribution of solid DT from a non-uniform mass to a uniform shell on the interior of a spherical container, driven by the heat released from beta decays, has been demonstrated in both high and low thermal conductivity shells. It has been demonstrated in a capsule size range and fuel layer thickness appropriate to a 1.5-2 MJ NOVA Upgrade. These demonstrations have been performed under well-controlled laboratory conditions. The layer has been characterized by optical shadowgraphy to a resolution of about 10 microns.

Our remaining work in this area involves two issues: improving and developing new characterization techniques for the layer; and developing a cryogenic hohlraum that will provide the necessary thermal environment.

The specifications for the fuel layer have not been completely determined. This involves applying an appropriate mix model to the layer to place limits on the inner surface roughness and on the fuel density uniformity. This procedure is very similar to that used to specify capsule surface roughness, and results in a limit to the rms roughness and a criterion for judging the amplitudes of the surface spherical harmonics, with similar specifications for a real density. This work will be completed in a time frame that allows assessing the quality of the layers in our demonstration target.

We need to be able to characterize both lateral spatial homogeneities and layer thickness uniformity. The former is within the resolving power of our present optical system. The latter will require application of interferometric techniques, which we have used for similar problems. An automated interferometric sphere sorter has recently become available that incorporates tomographic-like techniques that may be appropriate to our characterization problems.

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[8] The document numbers are UCRL-TB-104287 and UCRL-TB-104288 for unclassified Parts I and II.

The uniformity of the thermal environment around a fuel capsule is critical to the formation of a uniform layer, especially in low thermal conductivity capsules. The capsule will be contained in a cryocooled hohlraum, which will provide cooling for the capsule. Calculations have shown that conduction through a low density atmosphere of helium will give an adequate capsule environment, but the helium must be static. We must develop the details of such targets that will be compatible with in situ fills that will be required for the high DT pressures needed.

Low Density Foam Fuel Matrix

While we anticipate that the beta-layering technique will work, we have not completed the characterization of the layers. Should there be unforeseen problems, we can turn to a back-up capsule based upon holding the majority of the fuel as a liquid held in a low-density, low Z foam. This approach is complicated by the necessity of having a pure fuel layer of an appropriate thickness inside of this foam layer. It is possible, although untested, that this pure layer can be held by a thermal gradient. We have demonstrated such liquid layers in the past, although not inside a foam-held layer. The hohlraum for such a target would have to provide the required gradient.

We have already developed several candidate hydrocarbon foams with densities down to 40 mg/cm^3, and have filled these with liquid deuterium and DT. We would have to complete the development of these materials into the required spherical shells. In this regard, we have some experience with molding and machining, and would focus on these techniques. We have an experimental apparatus for examining the thermal gradient technique, and work is currently underway in that area. This is complemented by an extensive theoretical project to develop steady-state and stability models of the thermal-gradient-held layers, which would have to be modified to include the presence of the foam/fuel layer. We would have to address the characterization of the pure liquid layer uniformity, which will be difficult if the foam layer is opaque. We presently have plans to apply magnetic resonance imaging to characterization of fuel layers, which may be particularly appropriate here.

Appendix III
Technical Contract for the NOVA Upgrade Beamlet Demonstration

In this appendix, we include excerpts from the LLNL technical contract specifying milestones for a demonstration of the NOVA upgrade architecture. A complete document, as well as a larger document specifying milestones for the entire NOVA Upgrade project, can be obtained from LLNL.[9]

I. Introduction

The recently proposed NOVA Upgrade is an 18 beamline Nd:glass laser. The laser is a compact multipass design, fully relayed, with 4 x 4 segmented optical components. Each beamline is thus composed of 16 "beamlets," giving a total of 288 beamlets in the system. The beamlets are optically independent and individually pointed at the target for maximum control of illumination uniformity and pulse shaping. Specifications for the laser are determined by the ignition target requirements, summarized in Table 1.

Table 1. Ignition Target Requirements (Nominal)

Energy	1.5-2 MJ at 351 nm
Pulse duration	3-5 ns
Peak power	300-600 TW
Pulse shape	Continuous or picket fence
Dynamic range	100–200:1 (continuous) 10–40:1 (picket fence)
Power balance	5–10% rms
Pointing accuracy	IO-30 μrad

The NOVA Upgrade has been designed conservatively. All components and performance parameters have been demonstrated in the laboratory. In cases where the demonstrations were at subscale, the path to scaling to the full size device is well defined. To further validate the NOVA Upgrade design and to demonstrate component integration at full scale, we propose to build a single beamlet and to experimentally demonstrate that its performance meets the NOVA Upgrade design requirements. This paper summarizes the project scope, cost and schedule to accomplish that demonstration.

II. Prototype Beamlet System Description

The NOVA Upgrade Prototype Beamlet (hereafter Beamlet) will be designed to demonstrate all components of the multipass laser, from injection through frequency conversion, operating at the design specifications on energy, fluence, pulse duration, pulse shape, beam divergence and bandwidth. Those specifications are summarized in Table 2.

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[9] The document numbers UCRL-TB-104287 and UCRL-TB-104288 unclassified Parts I and II.

Table 2. NOVA Upgrade Beamlet Projected Performance

Energy at 0.35μm	5-6 kJ @ 3 ns
1ω area-weighted average fluence	≤12 J/cm^2 @ 3 ns
3ω area-weighted average fluence	≤8.5 J/cm^2 @ 3 ns
Peak-to-average fluence ratio	1.5
Pulse duration/shape	2-6 ns rectangular pulse
Beam divergence	≤50 μ rad
3ω bandwidth	0.01% (3 cm-‘)

This system would be constructed in existing laboratory space formerly used for the Novette laser. Our baseline plan for the amplifier is to use multiple modules of the 2 X 2 multisegment amplifier test-bed. Twelve of these modules would be built. Assembly of the beamlet amplifier from these one-disk-deep modules would be the same as planned for the NOVA Upgrade 4 x 4 amplifier modules and will allow the testing of such issues as coupling of the modules to maintain uniform and efficient flashlamp pumping.

The full 2 x 2 array would be flashlamp pumped, but only a single beamlet would be used to demonstrate energy extraction, propagation and frequency conversion. To reduce cost, high quality, platinum free laser glass would be used only in that beamlet. The function of the other beamlets would be to provide data on amplified spontaneous emission, pumping efficiency and pump-induced optical distortions. One possibility is to have the amplifier configured with nine modules on one side of the spatial filter and three on the other. Other amplifier configurations would be tested to determine the optimum performance and to check the validity of our design codes.

An oscillator similar to the NOVA master oscillator will be installed to provide shaped input pulses with the specified, single-beamlet bandwidth of 0.01%. The full 0.3% bandwidth of the NOVA Upgrade will be achieved by propagating pulses of different frequency in each beamlet. On the NOVA Upgrade, both continuous and picket-shaped pulses will be provided using a set of pulses (typically 3–4), each with a nominally square temporal shape with duration of 2-6 ns. This array of pulses will propagate down different beamlets, and will be sequenced and stacked in time to produce the desired temporal shape at the target.

The oscillator pulse will be preamplified to approximately 1 J energy, and then injected into the multipass laser cavity at the spatial-filter pinhole. After making 3-1/2 passes through the cavity, the pulse will be switched out by the Pockels cell. It will then pass through the output spatial filter and frequency conversion crystals.

Appropriate diagnostics will be included in the project to measure pulse energy, bandwidth, temporal shape and near- and far-field beam quality. The amplified pulse extracted from the multipass laser cavity would be directed to the output spatial filter by a pair of 60-cm turning mirrors. The pulse emerging from the output spatial filter is then incident upon another pair of 60-cm turning mirrors before entering the frequency converter. The frequency converter consists of two 30-cm KDP crystals in which the incident laser pulse, at a wavelength of 1.05 μm, is used to generate optical energy at a wavelength of 0.35 μm by the sequential processes of second-harmonic generation and sum-frequency mixing. A full aperture beam splitter (approximately 60-cm diameter) would direct a small amount of the optical energy to a diagnostics package, while most of the laser pulse is diverged with a negative lens before impinging upon an 80-cm NOVA calorimeter.

The output sensor package, which would be based upon the NOVA output sensor design, would utilize a very small amount of 1ω energy transmitted through the final turning mirror. The sensor provides alignment information as well as measurements of energy, near-and far-field intensity profiles and pulse shape of the 1ω laser pulse. The diagnostics package following the frequency converter would be similar in design to the NOVA Two-Beam Diagnostic Station. The diagnostic package would consist of two arms, each of which would take its input from a full aperture beamsplitter. The beam in each arm would be down-collimated and spectrally filtered by a spatial filter. The first arm would provide measurements of the energy, pulse shape and near field intensity distribution of the residual 2ω light. The second arm would provide measurements of the energy, near- and far-field intensity profiles, power spectrum, phase front and pulse shape of the third harmonic.

III. Component Development Program

Components for the Beamlet are currently being developed and tested. Under the proposed schedule, this component development program must be completed prior to assembly and testing of the integrated beamlet. Specific objectives of the component development program for the prototype beamlet are given in Milestones DRV 3.1-3.6 in the NOVA Upgrade Laser Design and Development Plan. Those milestones include:

(DRV 3) Demonstration of a prototype 30 x 30 cm^2 beamlet. This prototype will include all features of the final amplifier and beamline from injection through frequency conversion, and operate at the design fluence, pulse shape, beam quality, bandwidth, frequency conversion efficiency, and other performance specifications.

There are many sub-milestones which precede this demonstration, and these must be passed on the way to the major milestone of full beamlet operation. Several important examples of these are:

- (DRV 3.1) Large area damage test of optical components at the design fluence and pulse shape.
  - (DRV 3.1.1) Polarizer
  - (DRV 3.1.2) KDP, KD*P
  - (DRV 3.1.3) Beam transport mirror
  - (DRV 3.1.4) Spatial filter lens
  - (DRV 3.1.5) Target focus lens
  - (DRV 3.1.6) Injection mirror
- (DRV 3.2) Prototype single beamlet Pockels cell switch.
  - (DRV 3.2.1) Subscale prototype having all lower-level milestones listed below.
  - (DRV 3.2.2) Mechanical design of prototype

(DRV 3.2.3) Electrical design of prototype, including electrical feed compatible with an array.

(DRV 3.2.4) Design and test of electrical driver into a Pockels-cell load.

(DRV 3.2.5) Optical performance (damage, contrast, loss) of prototype under operating conditions.

(DRV 3.3) Prototype full aperture frequency converter meeting or exceeding the baseline specifications (3 cm^{-1} 3ω bandwidth, >70% conversion from abour 1.6 to 3.2 GW/cm^2 at about 35 mrad beam divergence).

(DRV 3.3.1) Subscale demonstration (NOVA) and parametric study of effects of propagation, type of modulation, divergence, bandwidth, converter design optimization.

(DRV 3.3.2) Beamlet prototype demonstration.

(DRV 3.4) Prototype single beamlet injection system and spatial filter.

(DRV 3.5) Prototype target chamber optics package, (frequency converter, focus lens positioner, vacuum window, debris shield, and coherence control plates or lenslet arrays, if used).

(DRV 3.6) Demonstration of gain hold off in the baseline multipass amplifier.

(DRV 3.6.1) Using active Pockels cell switch.

(DRV 3.6.2) Using passive switch.

GLOSSARY FOR COMMITTEE REPORT

Ablator: The outer surface of a target capsule that absorbs the energy driving the implosion, forming a hot plasma. The ablated material accelerates rapidly outward, which causes a reaction force which, together with the plasma pressure, drives a radially inward implosion of the remaining target material.

AURORA: A KrF gas laser being constructed and tested at LANL.

Backlighting Illuminating from behind, by an x-ray source, to obtain images of target dynamics with an x-ray imaging or stream camera.

Beam balance: A measure of the degree to which laser beams, impinging on a target in a multibeam drive, have equal intensities.

BES: Basic Energy Sciences Program in DoE.

Beta-layering A process by which, due to the heating by Beta-particles emitted in tritium decay, layers of frozen deuterium-tritium mixtures can be more uniformly deposited in a capsule.

BOLVAPS/LIBORS: Types of experimental light-ion sources: boil-off lithium vapor source (BOLVAPS) and laser ionization based on resonance saturation (LIBORS).

Burn: The process in which thermonuclear fuel is consumed in thermonuclear (fusion) reactions, producing an energy release.

Capsule: The fuel-containing assembly of an ICF target.

Centurion/Halite: A program of underground nuclear tests involving the ICF Program.

Crvonenic (capsule): Contains frozen or liquid deuterium-tritium mixtures.

DoE: Department of Energy.

Driver A device that provides energy to implode an ICF target in the form of high-intensity, high-power beams of laser light or ions.

D-T: Fusion fuel consisting of deuterium and tritium in approximately equal amounts.

Excimer: A rare gas “molecule” which exists only in an excited state.

Filamentation: An instability amplifying narrow intensity modulations of light propagating through a plasma.

FPAC: Fusion Power Advisory Committee.

Gain: Ratio of thermonuclear energy released (yield) to the energy delivered to the target by the driver.

HEP: Hydrodynamic Equivalent Physics.

HEPAP: High Energy Physics Advisory Panel.

HERMES III: A pulsed-power x-ray facility at SNL.

HF: Hydrogen fluoride; a type of chemical laser involving hydrogen fluoride.

HI: Heavy ions.

HIF: Heavy-ion fusion.

Hohlraum: Radiant enclosure.

Hot Electrons: Electrons from plasma instabilities that have energies under higher than thermal, and which can penetrate an ICF capsule and preheat the fuel, diminishing implosive compressions.

Hot Spot: Small high temperature region at center of imploded ICF capsule.

ICF: Inertial confinement fusion.

IFE: Inertial fusion energy.

Ignition: Occurs when the “hot spot” fuel in the center of an implosion attains sufficient temperature and density so that its thernonuclear reactions not only heat the hot spot further, but also promote burning of the compressed pusher fuel.

ILSE: Induction-linac systems experiment; part of the heavy-ion program.

Implosion: Acceleration of material inward to a center of symmetry.

ISI: Induced spatial incoherence; in early versions of ISI, a broadband laser output beam is split, by external stepped echelons, into many effectively incoherent beamlets which are then brought to focus on target. In echelon-free ISI, a spatial filter imposes an intensity profile on the output of a braodband multimode laser source. This profile, which consists of many small independent zones which take the place of the beamlets in ISI, is projected through the laser chain to form a symmetric drive pattern on a target.

KMS: KMS Fusion, Inc., an Ann Arbor Company which has been involved in the ICF Program.

LANL: Los Alamos National Laboratory.

LASNEX: Computer code modeling ICF physics.

LBL: Lawrence Berkeley Laboratory.

LEVIS: Laser evaporation source for light-ions.

LiF: Lithium fluoride.

LLE: Laboratory for Laser Energetics, University of Rochester.

LLNL: Lawrence Livermore National Laboratory,

LMF: Laboratory microfusion facility.

M: microns = 10” meters; a measure of light wavelength.

NAS: National Academy of Sciences.

NIKE: KrF laser being constructed at NRL.

NOVA: Very large glass laser at LLNL.

NOVA Upgrade: Glass laser proposed by LLNL to achieve ignition.

NRL: Naval Research Laboratory.

NSAC: Nuclear Science Advisory Committee.

OMEGA: Large glass laser at LLE.

ω: Light frequency. 3ω is third, 4ω fourth harmonic of light frequency w.

PBFA II: Particle Beam Fusion Accelerator, Model II at SNL.

Picosecond: 10^{-12} seconds.

Pusher: Layer of a capsule that compresses the fuel and hot spot. It may itself contain fuel.

Rayleigh-Taylor: A mechanical instability at the interface of a low-density material accelerated into a high-density material.

SIS/ESR: Heavy-ion synchrotron/experimental storage ring for ICF at Darmstadt, Germany.

SLAC: Stanford Linear Accelerator Center.

SNL: Sandia National Laboratories.

SSD: Smoothing by spectral dispersion; a method of beam smoothing of a glass laser. Gratings are used to disperse laser light of broadened bandwidth to, ideally, irradiate each element of a distributed phase plate with a slightly different frequency. The focused interfering beams from the phase plates will have a smooth time-averaged intensity to drive a target.

SRS: Stimulated radar scattering; a process in which laser light, interacting with a plasma, decays into a Langmuir type plasma wave and a scattered light wave. Nonlinear SRS instabilities can generate hot electrons.

UGTs: Underground tests.