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To the First Meeting of the Provisional NAS Committee on
"Prospects for Inertial Confinement Fusion Energy Systems"

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I believe all members of the Committee should already have received my detailed comments on the overall balance of the committee and the potential conflicts of interest of the various members, so I won't repeat those now.

What I would like to do is to outline why a failure to re-balance the Committee is likely to be significant, and could result in a flawed and potentially harmful report. I would like to do that by looking back briefly at the conclusions of the three previous pivotal NAS reviews of ICF, completed in 1986, 1990 and 1997.

The 1986 NAS review, chaired by Professor William Happer of Princeton, concluded that a 1-10 MJ driver would likely be required to reach ignition, with the driver technology at the low end of that scale being "the more energetic (1-2MJ) PBFA II light-ion facility." But given the difficult technical challenges facing ion beams, the Happer Committee concluded it was "prudent to continue to carry out exploratory research on larger laser drivers . . ."

> "A 10 MJ laser may be needed to ignite a pellet. The cost of building the ~100kJ Nova laser was about $200 million. The weapons program cannot afford the $20 billion needed to build a 10 MJ laser at the same $2000/J construction cost. Revolutionary new design methods for lasers and perhaps new types of lasers are needed to bring the cost of a 10 MJ laser down to an affordable level." (NAS First Review, March 1986, p. 24)

In the event, it's ironic to note that based on delivering 1.3 MJ of frequency tripled light to the target chamber at a cost of $3.5 billion, the NIF laser system has cost $2700//J (before including the additional hardware costs of achieving ignition). In essence, DOE went ahead and built the "unaffordable" laser driver the 1986 committee warned against, but only got about 1/10 the amount of laser energy the 1986 committee thought necessary for ignition.

Now why did DOE do that? That question brings us to the conclusions of the Second Review of the DOE's ICF program, chaired by Prof. Koonin of Caltech and completed in September 1990. That report concluded:

> "…it should be possible to closely approach and probably achieve ignition and modest gain in the laboratory by the intermediate step of a few-mega joule class laser driver, which might be constructed for less than $400 million. The glass laser is the only candidate laser driver that could be used for an ignition demonstration in the next decade [1990's]. Indeed this demonstration is the natural next step in the Nova program and is referred to by LLNL as the 'NOVA Upgrade.'. . . 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." (NAS Second Review, Sept.1990, p. 8)

At the time, the only empirical confirmation that such micro-scale fusion via indirect drive might be feasible came in the 1980's, from a secret DOE weapons lab ICF project called Halite-Centurion, which used the x-rays from underground nuclear test explosions to implode fusion capsules that were small in relation to nuclear weapons, but still quite large in comparison to what might be practically pursued in a fully-contained laboratory setting. Based on these still secret results, a separate panel Los Alamos panel chaired by Greg Canavan had concluded in 1987 that "laser energies in the 100 MJ range . . . may be

required for [laboratory] fusion experiments." If one uses the proposed driver-to-target coupling efficiency of 20-25% given by Lindl in his 1998 book on indirect drive ICF, this suggests that Halite-Centurion targets absorbing less than about 20 MJ failed to ignite. So it was a giant and essentially undocumented leap for the 1990 Koonin Panel to conclude that a 2 mega joule laser could "closely approach and probably achieve ignition and modest gain in the laboratory," and even more conjectural to conclude that LLNL could build the driver for this effort for a mere $400 million. However, it was no doubt "the natural next step" in Livermore's Nova program.

Needless to say, there were significant dissenters at LANL and elsewhere to the 1990 Committee's conclusions, but these concerns were ignored as LLNL pressed ahead with NOVA Upgrade, which quickly morphed into NIF, a supposed example of the more cost-effective laser design the Happer committee had said was required to pursue laser fusion at the mega joule scale.

The $400 million buy-in cost estimate for the NOVA upgrade soon went by the wayside. By the time of the Academy's third review of the Department of Energy's ICF Program, held in 1996-97 and again chaired by Professor Koonin, the direct construction cost of a "National Ignition Facility" had increased to an estimated $1.148 billion, plus costs for laser and optics R&D, diagnostics and instrumentation, startup costs and a budget contingency that brought the total to $1.7 billion, for a promised 1.8 MJ of frequency-tripled laser light delivered to a target chamber.

So that's slightly less than $1000 per joule, half of NOVA's nominal cost, had it actually reliably delivered 100 kilojoules, which it didn't, due to optical damage from frequency tripled light and nonlinear optical effects, never overcome, which kept it at 20-30 kJ. NIF Project completion was set for the third quarter of 2002.

So this was the context for the Academy's third review, which focused on the scientific and technical readiness of the NIF project to proceed to construction. In March 1997 this review concluded:

> "In sum, the committee believes that *the NIF can be delivered to specifications within the stated Total Project Cost*, as augmented by LLNL-projected operating funds, allowing the high-energy-density and ignition experimental programs to proceed; *there are no identifiable 'show stoppers.'* The achievement of ignition appears likely, but not guaranteed. The steady scientific and technological progress in ICF during the 6 years since the last National Research Council review, *the plausibility of ignition estimates based on the experimental and modeling results and capabilities in hand*, and the flexibility of the facility all support the committee's finding that the *NIF project is technologically and scientifically ready to proceed as planned with reasonable confidence in the attainment of its objectives*. (NAS, [Third] Review of the DOE's ICF Program: The National Ignition Facility, 1997, p. 2)

Not only have events proven that these conclusions were deeply flawed, but they were hotly contested at the time by knowledgeable persons inside and outside the fusion research and nuclear weapons community. The Koonin Committee simply ignored all these voices, and constructed a flimsy brief for proceeding to launch construction of the NIF.

In 1997 NRDC brought a successful lawsuit against the NAS for failing to ensure the compliance of the Koonin Committee with the Federal Advisory Committee Act (FACA), In our own due diligence investigation of the Koonin Committee, we found that:

- Five out of the sixteen members were paid consultants to LLNL
- While serving on the committee, three members were directly involved in (successful) bids for closely related NNSA computer simulation contracts

- Overall, 14 out of 16 members had a personal or institutional connection with the very agency whose program was ostensibly undergoing "independent review."
- Eleven out of 16 members (i.e. two-thirds) of the committee had either previously stated positions supporting NIF or were consultants or advisers to LLNL and even the NIF program itself.

Not surprisingly, a committee with these attributes had no capacity or incentive to probe deeply and ask the really tough questions, or vigorously question the basis for the answers they did get. There is also the possibility that on some the key issues the committee was simply misled, which would not be unprecedented behavior for a DOE weapons laboratory when it is determined to land a big project. But Academy committees must account for that kind of behavior, and be prepared to neutralize it with well -balanced committees comprised of members that are eager, capable, and free to seek the truth of the matter, wherever that search might lead.

So let me review, briefly, what the 1997 NAS Review got wrong.

(1) Obviously, the Koonin Committee's "belief" that "*NIF can be delivered to specifications within the stated Total Project Cost"* was wildly incorrect, and not even close to what an ignition-ready NIF is ultimately going to cost. In the FY 1998 budget request, the NIF completion date slipped a year to "the third quarter of 2003. In late August 1999 came shocking disclosures that in the spring of that year, senior officials of LLNL had lied to then Energy Secretary Bill Richardson, and that he in turn had unwittingly provided false testimony to Congress regarding the cost and status of the NIF Project. GAO was brought in to investigate and LLNL and DOE responded with a welter of hastily assembled review committees to examine what were deemed to be serious "management deficiencies" in the project.

LLNL suddenly "discovered" that it had "miscalculated" the "construction management" aspects of the project, particularly the requirement for ultra-clean assembly of the laser beam lines and optical components, and the cost of the NIF project escalated dramatically. Physical construction of the NIF would now cost $2.25 billion, and GAO forced DOE and LLNL to recognize an additional $1.2 billion in "other related costs" required to complete the facility, bringing the NIF's "total project related cost to $3.5 billion. But none of these reviews probed the continuing and potentially crippling uncertainties surrounding the NIF's target physics and their sensitivity to continuing issues with laser performance.

At more than twice the cost estimate endorsed by the Koonin Committee, $3.5 billion became the new "baseline" for the NIF prospect. Project "completion" was delayed five years until "4Q 2008" (a subsequently slipped to March 2009).

But this was not the real "bottom line" of the growing NIF black hole in the budget. In the FY 2005 budget request, NNSA projected that an initial ignition demonstration, originally set to occur in that year, would now occur in FY 2014. Faced with harsh congressional criticism, NNSA officials suddenly announced in March 2004 that they were moving up the first ignition demonstration to 2010, but offered no credible public explanation of what had led to the delay, or how they had so quickly managed to reverse it. The current review should probe this issue and get the real technical explanations for what was actually happening within the program.

What did occur was a further stealth restructuring of NNSA's effort to fund the achievement of ignition on the NIF. The creation in 2005 of a "National Ignition Campaign (NIC)" created another funding stream into which the additional construction, hardware and R&D costs of equipping the NIF for ignition experiments could be dumped and pooled with the costs of supporting the other participants in NNSA's

ICF program, thereby maintaining the fiction of continuing compliance with the NIF "revised baseline" cost estimate of $3.5 billion.

The NIC also picks up all the target redesign and fabrication costs related to the still ongoing search for an ignitable target design to match NIF's reduced energy at the third harmonic and other deviations in the original design basis drive conditions. In August 2009 GAO estimated all these "NIF scope activity" costs, for Fiscal Years 2006 through 2012, at $2,044.2 billion dollars, bringing the total cost of the quest for ignition on the NIF to $5.54 billion dollars.

You might be interested to know that six years ago, in May 2004, NRDC produced it own cost estimate showing that the cost of completing the NIF, in as spent dollars, would be at least $5.2. The current GAO estimate still does not include the significant fraction of LLNL operating funds that have been and are continuing to supplement the NIF budget, including NIF overhead costs that have been deliberately misallocated to other LLNL projects, despite the protests of DOE auditors. Based on the Academy's own rule of thumb that 10-13% of the total project cost of high technology research facilities should be set aside for O&M costs, continuing to support a meaningful pace of experiments on the NIF will cost on the order of $350-500 million per year. This raises the question of how long, and for what purpose – weapons science or energy research – can the nation afford to operate the NIF.

(2) The second Koonin Committee concluded, *"There are no identifiable 'show stoppers.' The achievement of ignition appears likely, but not guaranteed…* This was tantamount to saying that LLNL's team had demonstrated sufficient scientific understanding, as well as the actual technical ability, to minimize both of the major show-stoppers—laser-plasma and hydrodynamic instabilities—to the degree necessary to achieve the conditions needed for hotspot ignition. This conclusion was unfounded then, and as Dr. Bodner makes clear in the review we are providing to the Committee today, it remains unfounded today. In fact we still don't even know, to a certainty, whether the fundamental premise of laser micro-fusion – that "energy is just a matter of size" – is valid. Are all the fundamental parameters for ICF freely scalable, or are certain minimum capsule dimensions, wall thickness, material strengths, pulse lengths, and absorbed energies required to conquer hydrodynamic instabilities.

(3) *"The plausibility of ignition estimates based on the experimental and modeling results and capabilities in hand*, and the flexibility of the facility all support the committee's finding that the *NIF project is technologically and scientifically ready to proceed as planned with reasonable confidence in the attainment of its objectives*." While it was certainly not true at the time the second Koonin committee rendered this sunny conclusion, the NIF team has made significant progress over the last 13 years on the UV optical fluence damage problem, raising the optical damage threshold from 600 kJ in 2000 to, apparently, 1.3 MJ today, based on the last reported test. This new review obviously needs to probe this issue and what it implies for the quest to achieve ignition at higher energies.

But as Dr. Bodner's report shows, the NIF team hasn't yet conquered a related and even more fundamental issue -- the debilitating effects of laser-plasma interactions that produce significant unwanted scattering of the laser light. These effects are potentially devastating for the indirect-drive approach that relies on very precise deposition of laser energy at particular points on the inner surface of the holhraum. In fact, NIF was set up to deliver this energy in a particular pattern that now appears unworkable and must be revised.

Even more disturbing is Dr. Bodner's discussion of the recent disclosure at the November 2010 Plasma Physics meeting that the NIF ignition target modeling effort has omitting the effects of dielectronic recombination, not just in the 1990's when the decision was taken to build the NIF, but right up to the present time, when billions of dollars of new computing power has been made available to the ICF

program. This is a stunning, almost unbelievable error for a program that is supposed to be making a substantial contribution to our continuing confidence in the nuclear weapons stockpile.

And that brings me to my final point. The hard fact of the matter is that the NIF Project was opportunistically marketed to Congress and the American people in 1997 as an urgent tool needed to maintain the safety and reliability of the nuclear weapons stockpile, beginning in 2003 when it was supposed to have been completed. But the stockpile has obviously been maintained and even upgraded these last 13 years without ignition on the NIF, and it will continue to be. The U.S. nuclear stockpile can endure just fine without the NIF. The other weapons justification for the NIF was as a training tool, for a new generation of weapons stewards, but the prolonged omission of dielectronic recombination from the ignition target modeling code raises questions about the institutional culture and competence of the NIF project as a training ground for people entrusted with the management of the nuclear weapons stockpile.

So it would behoove the committee, I think, to evaluate the future of the NIF Project solely for the value it may or may not have for making cost-effective contributions to IFE development. It is not a necessary component of the US nuclear weapons program, but rather a source of continuing embarrassment to the nation's credibility on nuclear nonproliferation. How can we at one and the same time advocate for peaceful international cooperation in the field of ICF while simultaneously pursuing a vigorous program to achieve ignition of the NIF in order to maintain our weapons and train the next generation of nuclear weapon designers.

I might add that personally, I do not support a major IFE development effort at this time, by reason of the urgent national priority to move rapidly to decarbonization of the global energy system with the more practical and prospectively cost-effective solutions that we have at hand, and that can be deployed in the next 1-20 years. IFE should remain a competitive science-based program with a modest experimental facility component until a truly promising and cost-effective solution emerges from that effort.