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PRP Comments for ICF Q1/Q2 FY17 Experiments 3/10/16

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PRP Comments for ICF Q1/Q2 FY17 Experiments 3/10/16 – R2004/2005 B481
Attendees: P. Graham, W. Garbett, J. Hammer, R. Kauffman, J. Knauer, and J. Wark
Absent: M. Rosen,

The PRP generally endorsed the Program plan during the short time for discussions. We agree that the strategy to develop a hohlraum that is symmetric and has low laser-plasma instabilities and to develop an alternative method for supporting the capsule is the best path forward for making progress in understanding ignition performance.

The Program is oriented toward a milestone in 2020 for "determining the efficacy of NIF for ignition and credible physics-scaling to multi-megajoule yields for all ICF approaches." We are concerned that the time and resources are not sufficient to vet all of the various approaches that are being pursued to make an informed decision by this date. For NIF to meet this goal, a process will be needed to to select the most promising paths forward. We recommend that the Program develop this process for selecting the path forward to optimize resources.

We were glad to see that the direct drive program took our comments under consideration. We think that the proposed experiments have the program headed in a better direction.

The PRP had only a short time to discuss the detailed experimental proposals. The following are comments on the detailed proposals. We did not have time to discuss them as a group. They represent individual opinions and provided to you as feedback to your proposals.

Integrated Experiments:

We've endorsed the strategy of multiple base camps with incremental steps away. Omar described the approach for integrated experiments as a series of "projects" of reasonably well pre-determined, finite duration. (I think he is concerned about every campaign developing advocates and a life of its own). That is all fine but a clear description of what they are going to do with the information from each project is missing. The goal should still be to develop a framework or model, some combination of codes and empirical playbooks, informed by all of the projects so that they move toward something resembling predictive capability. I would suggest they couch the whole program as developing that framework, rather than relegating model improvement as a nice-to-have.

Both the HDC and Be capsules fielded in specifically tailored hohlraums offer complementary paths to ignition and should be pursued, however, continuing with 3 ablator material options is very expensive. The Bigfoot is a good opportunity for risk mitigation to achieve low-gain ignition with a more robust hotspot, however, it does rely on a very high (>400km/s) implosion velocity. Has the full design space for this option been explored thoroughly before the choice of a HDC ablator, etc?

The wetted-foam capsule offers a similar path to a more robust hotspot but an assessment of its benefits and risks relative to Bigfoot is needed to set priorities. The 2-shock platform has low sensitivities and so is of limited use for ignition, although the convergence-ratio scan is worth pursuing given that only a few shots are required. I would not pursue double-shell capsules since this will be a major effort and will pull resources from other areas. Double shells would be better done under HED.

There is good reason to support a variety of different integrated implosion basecamps, which probe different aspects and failure modes. Given the multiplying effect on shots, some rationalization should be considered.

- Strongly support probing of 2shock CCR and CR variations. Although a DT ice version was said to be cost effective, it is possible that ice effects and CR effects could be confused. A higher CR gas capsule route should be considered. Also, taken together with the HED 2shock allocation, there would be a significant effort on this platform. Can the team support this large number of shots? Should NIF invest in a large number of shots on this single platform, or is some prioritization within the ICF & HED campaigns preferable. I would prioritize the ICF 2s shots.
- A significant number of shots are still being devoted to CH capsules. There is merit in examining the impact of an alternate mount and in imaging the cold fuel for the high foot, as these are believed to be the key issues degrading performance. The value in additional shots beyond this, or on the 0.9 scale CH 672 (UHP and repoint / CBET scope) is not clear at this time, and some rationalization may be possible.
- Both the Bigfoot and wetted foam platforms allow study of potentially more robust hotspot assembly, which is complementary to the other basecamps. It is unclear whether both approaches are necessary. The Bigfoot provides a potential low gain path to ignition which could be a viable alternative on the 2020 timescale. The immediate goal of the wetted foam effort is to establish where in CR a 100% YoC is recovered. The effects of the foam matrix on the wetted foam target may not be well modeled and may distort conclusions about the YoC-CR relation. This relation is also being probed by the 2shock platform, although admittedly the range of CR variations within an ice target may be more limited.
- Double shells provide a very different alternative route to ignition, a back-up option outside of the existing NIF experience. The few shots dedicated to this are insufficient to make adequate progress. Given the immaturity of double shells relative to the existing route, there is unlikely to be a decisive answer on their prospects on the 2020 timescale without significant shot investment. I would recommend development of double shells as a physics platform through the HED program only.

While the PRP supports the double shell campaign, the small number of shots proposed seem inadequate to making significant progress. If double shells are intended as a risk mitigation, perhaps more resources should be devoted to them. If, rather, they are intended to explore the ICF science in a low effective convergence ratio regime, then maybe they should be in the ICF Science trade space.

Seems like the double shell stuff is – and should be – design intensive for the moment. Having a couple of shots as incentive provides motivation for the design work. Creating a big campaign on NIF should depend on how promising the designs look. Of course, reality can always be worse than the code, but is unlikely to be better.

General approach is good (as discussed) but there was a comment made about fixing problems without an understanding of the underlying physics. If I can use Omar's tire analogy, If I fix the flat tires without knowing why they are flat, then they may go flat again at an inopportune time. I thought we learned this lesson from the NIC campaign. I sympathize with the problem of the size of the parameter space, so some kind of decision tree is necessary.

ICF Science Experiments:

Some of the ICF Science proposals seemed not to hew to the strategy that was articulated. Given the magnitude of the 2020 task, the Program should re-consider the amount of time devoted to issues unrelated to symmetry or capsule support issues for now.

The Indirect-drive Science Campaigns cover a wide range of experiments to assess hohlraum and capsule performance. The highest priorities should be experiments on tent alternatives and Compton radiography for cold-fuel imaging. Characterizing the hohlraum behavior (e.g. corona temperatures, wall/capsule motion) is welcome but more thought should be given to maintaining the same conditions and materials as a 1.8MJ ignition hohlraum, and focus on low gas fill designs. It will be a long term activity to develop new physics models to explain current discrepancies between hohlraum data and simulation and turning code 'knobs' is possibly the only practical quick solution; therefore the closer experiments are to the ignition target the better. Experiments with simpler geometries should also be considered to aid diagnostic access and interpretation, and I would also like to see radiography of the hohlraum dynamics. Foam-lined hohlraums offer the potential to reduce wall blow-in and possible other advantages and so are supported, although simpler geometries may be better for the initial experiments.

Measuring EOS data for capsule materials is welcome although no indication was given of the accuracies required or achievable. The impedance match EOS technique is limited by knowledge of the standard material used and by assumptions in the impedance match methodology, while absolute EOS measurements are more challenging. Off Hugoniot and release EOS measurements are also required. The hohlraum dynamics campaigns mentioned above would also benefit from supporting material properties experiments (both LTE and non-LTE) for the hohlraum wall material.

It is clear that integrated experiments should be balanced with science campaign shots to develop better understanding. However, some prioritization of science campaign activities is required. Science campaign experiments fall into two types: development of techniques and actual measurements.

- Technique development should proceed, but should be prioritized according to which measurements have potential for greatest impact. Some of the proposed measurements (e.g. hohlraum n_e) will be scientifically useful, but on the 2020 timescale may only result in empirical tuning rather than detailed theory and model improvements, as the latter could be a substantial undertaking. Empirical tuning is of questionable use for extrapolation.
- Actual measurements should only be performed on relevant platforms. Incomplete understanding makes it difficult to read across empirical results from one platform to another. This implies that most detailed characterization measurements should wait until some of the integrated basecamp work has been performed, so that measurements can be targeted at those platforms identified with relevant issues or failure modes.

Some detailed points:

- Given the focus on low and intermediate gas fill (low LPI) hohlraums it is not clear that there is value in the proposal to apply dot spectroscopy to high gas fill hohlraums at this time.
- The MagNIF experiments do not fit with the strategy. There may also be surrogacy issues between the NIF platform and Z experiments.
- The value of the EOS data campaigns is unclear. Do they address the right parts of phase space? How significant is the expected impact on predictive capability? (i.e. what is the sensitivity to EOS uncertainty compared to other modeling issues?) To what extent do existing growth facto experiments constrain ablator EOS?

Polar Direct Drive:

The MJ Direct Drive plan now appears much better with a focus on smaller physics-based tests instead of large integrated experiments. This will allow the physics on NIF-scale laser interaction to be studied so the correct decisions are made on direct-drive phase plates and other facility requirements. Clearly laser-to-target coupling including imprint, LPI (and its mitigation) and cross-beam energy transfer (CBET) are very important and not fully understood and campaigns are planned to study these issues. Good use is being made of existing platforms to study ablation-front instability growth and shock symmetry, although more than 2 axes (on VISAR) could be deployed for the latter. The imprint and instability growth experiments could depend on CBET and angle of incidence of the beams and sensitivity studies may be required. They will also need to be extended to 3D surface seeds and with emphasis on excellent imaging resolution with a well-calibrated modulation transfer function.

Previous proposals for FY16 Q3-4 included experiments to measure preheating using buried layers and irradiated foils to characterize laser spot profiles from X-ray emission, and yet no continuation work was mentioned here. It is good to see that 3-D modeling is now being used for these experiments in collaboration with LLNL.

Only added comments concern the delta lambda experiments to study CBET. The +/- 2.3 Ang. range may not give a difference in measured data that can be used to study the effect. The Cone-in-shell VISAR example showed very small differences. It was stated that this was a preliminary experimental design and I encourage increasing the priority for the experimental design development. Similarly, the implosion shape differences are of the order of the signal-to-noise for the data shown. I recommend that a noise analysis be done to determine the signal-to-noise needed to make a quantitative measurement. ie what backlighter should be used.

The direct drive strategy has matured since the previous meeting. The move away from purely implosion-based platforms is likely to yield more scientific insight to the current problems and is applauded. The team has responded positively to previous PRP comments.

One minor query is whether the proposed delta-lambda test will produce sufficiently different imploded configurations. Both the synthetic images and lineouts appear marginal. If the intention is to demonstrate the effect of delta-lambda mitigation and validate or constrain modeling, the team should consider using a modified target design to enhance the difference.