Addendum to copy of this memo from 5 July 2004: Since the manuscript hasn't changed much between versions 5 and 6, and because the version of Nino's comments that the July 5 memo refers to is still the most recent, I haven't changed anything in the copy of the memo.

However, the issues related to problems with my analytic calculations of the drive temperature (discussed on page 4) have been resolved (and that's reflected in the new ms_v6). Some of the new simulations you requested have now been done and results are posted on the website. There was one new set requested near the end of this memo for which I didn't understand exactly the parameters you thought would be interesting.

Note that the URLs of the links have changed (/hohlraum/ → /projects/hohlraum/)

Everything below here is original from July 5 (except I've updated the URLs).

D. Cohen
5 July 2004

Response to Nino's comments from 24 May 2004 (re: manuscript ver. 4), going along with the new manuscript ver. 5

My responses (plus a few extra comments) in blue…obviously. No response indicates I incorporated your comment.

David, 5/24//2004

I'm still a little confused about the desired terminology, so I'm still using "halfraum" after initially defining it and giving the alternate nomenclature. "Half-hohlraum" and "single-ended hohlraum" sound too cumbersome to me, but I'm open to persuation.

I wonder about our ignoring glint in the modeling and whether, since we've arbitrarily been showing the 100 ps output, whether we're systematically getting high temperatures (all relative trends should scale out, I would think (though maybe not exactly, since scattered light gets deposited in specific locations in the code, if that option is turned on).

My further suggested revisions, comments and additions by Page #:

Of course, the page and figure numbers have changed slightly.

1) Include Lindl *et. al*. PoP 2004 new reference.

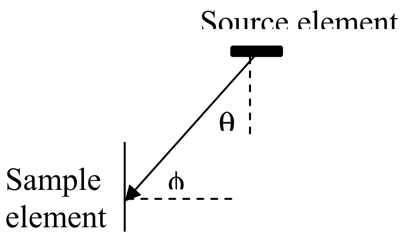
Good paper, but rather lengthy. I've eliminated the reference to his old review in favor of this new one throughout.

"**We find that** the spatial variation…

… of wall reemission **from a particular direction**, using an absolutely calibrated..

2) 10 42° OMEGA beams (Cone 2) and 20 59° OMEGA beams (Cone 3)… illuminated..

3) After sentence multiple Planckian sources, include brief description of what a view factor calculation is. What I have in mind is adding Figure like this,



and then stating that Lambertian flux on sample is proportional to cosθcosϕ.

I acknowledge that the image quality of the new figure (Fig. 2) needs to be improved.

The albedo value peaks at 0.73 **at 1 ns, for a constant power drive reaching 190 eV**.

OK, can I say "approximately 190 eV"? – especially since we're challenging the idea of a single "drive temperature" in this paper (though certainly the incident radiation onto the gold in the hydro simulations from which the albedo values are derived is perfectly well defined – and was near, but probably not exactly, 190 eV at its peak)?

Regarding XCE seeming low at 0.55, this may be because you are assuming peak power per beam a bit high at 0.5 TW. I need to look over energy and pulse shapes for June 1996 Decker shots, but I believe energy was of order 480 J/beam spread over a 1 ns flat-top pulse with ≈ 150 ps rising and falling edges, which means flat-top portion of power was only 0.42 TW/beam. This would then increase XCE to fit Tr data to 0.5/0.42 * 0.55 = 0.65, close to 0.7 reached in Lindl paper at 1 ns.

I will be glad to redo the modeling with a capsule (see preliminary capsule modeling on website) once we get the new beam power profiles and, perhaps, some confirmation that the capsule was included in the modeling presented in Decker et al. I have to admit, I was surprised to hear from you that there was in fact a capsule in the experiments. You sure can't figure it out from reading the paper (and the comparison to DANTE views through a diagnostic hole in the barrel made the presence of a capsule seem unlikely).

4) This, as expected, is somewhat less than the DANTE Tr **since DANTE, unlike any point inside hohlraum, does not see any of the cold LEH regions. It is close to Tr on sample, as expected.** Leave out last sentence as don't expect to be average of Dante and sample Tr.

…**but mainly** due to the contributions from the cold LEH.

…wall reemission **flux**, which is **equal** to … **multiplied by the albedo**.

Not obvious that midplane Tr looks harder. I would plot spectra on log-linear. Make dotted lines dashed and thicker. I am surprised Dante Planckian and multi-Planckian curves don't appear to have same integral on plot (worth a check).

Good call. I'd used a DANTE temperature in my Planck calculation from the wrong simulation (the area under the sample spectral curves was equal, but the area under the two DANTE curves was off by 10%). It's fixed now. The cosmetics of the figure have also been modified as you suggested. Note that the hardness w.r.t. Planckian is a bit more for the sample than for DANTE. I've put a figure at:

http://hven.swarthmore.edu/~cohen/projects/hohlraum/hohlraum_spectra_ratio.bmp

It shows the ratio of the view-factor calculated spectra to the equivalent Planck spectra for the sample and for DANTE. But I agree it's hard to see this small difference in the figure in the paper.

…of cold LEHS **replacing part of wall** leads..

… Dante Tr increases **for a different reason**, … pointing moves in, hence **out of the partial occlusion of the LEH lip.**

Should explain that sample Tr increases as spots move in due to combination of spots closer to mid-plane and further from LEH, hence less spot radiation is lost directly out of LEH.

5) On Figure 5, I would overplot wall sample Tr as X's at 90°.

This comparison of tcc vs mid-plane wall Tr is becoming a hot topic again as we think about shock timing for an ignition capsule. Current plan is to do shock timing in planar package on side using VISAR, then assume we can match Tr at capsule at center of hohlraum. Should mention this.

I added a sentence. And in the Conclusions too.

6) Ditto on log-linear plot and bolder, dashed lines on Fig. 6.

..and less **lower** energy **wall** radiation…

7) … divide the hohlraum in half, **hence length 1150 μm**.

I think you should say that halfraum sample Tr is lower because of extra divider wall losses, and say that expect of order –2% in Tr, (-10% in flux) due to 20% extra walls with 50% of losses due to wall albedo.

OK, so now I've done this power balance calculation, and redid the one for the hohlraum. I get a decrease of more than 2% in temperature (the extra wall area represented by the disk is more than 20%). See my notes:

http://hven.swarthmore.edu/~cohen/projects/hohlraum/power_balance_Tr.jpg

The problem is that I get a different answer for the hohlraum drive than I did before. I've got to put this calculation down at this point. If you or Joe is willing to check it, I'd appreciate it. I get 180 for the hohlraum and 173 for the halfraum. I haven't changed the text of the manuscript to reflect this yet, as these temperatures seem too low .

…it is clear that the **proportionately larger drop in DANTE Tr vs sample Tr** in the halfraum…

I have changed my mind on differences depending on whether or not low angle beams cross mid-plane, as both cases would yield both cones in view in hohlraum case and only 1 cone in view in halfraum case, barring pathological pointings. So, would say:

See the new simulations on the website:
http://hven.swarthmore.edu/~cohen/projects/hohlraum/midplane_crossing_beams/

In situations where low angle beams are used, which cross mid-plane, **one should expect similar trends, with the low angle beams hitting the back wall rather than sidewall of halfraum.**

The DANTE drop is the same, but should we mention the bigger relative sample temperature drop? Or the fact that the DANTE drop in temperature is for the same reason – the hot spots that would be caused by beams entering from the far LEH don't exist when the disk is put in the simulation?

I think you should calculate case of say 21.4° OMEGA beams to prove this.

8) Can you arrange it so Fig. 8 and 9 and relevant text are on same page, and so on for pairs of figures?

Say first that general trend is for Tr to go up with inner pointing for same reason as for hohlraums (Spots closer to sample, further from lossy LEH)

Say that roll-off on sample Tr for inner pointings is due to increasing obliqueness (large values of ϕ) of beam spot rays incident on sample facing LEH.

… the distinction **between weak wall re-emission and cold LEH is minimal relative to hot spots, so sample seeing wall and LEH and Dante seeing wall give similar Tr.**

… in Figs. 10 and 11 **are qualitatively different than for Fig. 9. The sample Tr drops as even faster with pointing moved in because rays from the Cone 2 spots arrive even more obliquely on the sample.**

Some rewording, but the content of this suggestion is now in that paragraph.

I would flip order of Figs. 10 and 11 with Figs. 12 and 13 (first talk about effect of LEH lip size varying, then spreading Cones).

9) To explain Fig. 13 relative to Fig. 9, I would first say: DANTE Tr rises more quickly as beams move in for 75% LEH case because spots move into FoV from behind LEH lip. Sample Tr drops more than DANTE Tr when removing LEH lip because while sample sees more cold LEH, DANTE actually sees more of spots (especially for outer pointings). The last well-written 3 points on Page 9 can then follow.

Got it all in, but in a somewhat different order.

In all Figure captions, I would add % LEH.

10) Sample Tr 100 ps points on Fig. 13 seems odd and too high by at least 10 eV.

Well I checked the output of the calculations. It's not a typo. (Fig. 12 now, by the way.) I guess I'm note sure why I should think the early time sample temperature point is high I guess they are not much lower than the comparable pointings for the no-lip case shown in Fig. 10. I can look into this more next week, but I haven't found anything wrong so far.

11) Explain that drop in sample Tr for shortest halfraum length is due to obliqueness of rays onto sample from especially Cone 2.

Figure 16 Caption: explain that pointing and halfraum length varied by same amount so spot to sample distance constant.

12) Give mean pointing on Fig. 17 (620 μm).

530 microns from back wall (referencing it this way makes sense to me, since this is the quantity that's constant). It's mentioned in the caption now.

Explain the Dante Tr on Fig. 17 drops as length increased because Dante seeing progressively more of wall further from spots for which Tr is dropping given axial negative Tr gradient emanating from spot,

I think important to understand why sample Tr stayed the same as hohlraum lengthened (I still expected it to go up (in the limit of an infinitely long hohlraum, shouldn't sample see

Well, there's a weak upward trend if you ignore the last point where focus issues might play a role.

no LEH and any low albedo wall is better than LEH?)). Could you do another run with pointing moved out say 150 μm (to average pointing of 470 μm), and a series of 150 μm longer hohlraums?

I'm sorry, can you clarify what simulations you'd like to see? I'll do them next week.

The idea of Au LEH shields (internal) is becoming a hot topic for ICF again (your Ref. 13 Amendt paper + Amendt 1997 PoP) as a means to increase Tr in hohlraums and help symmetry perhaps. Issues will be what does DANTE see with such a shield (NIF Dante also at near 37.4° view), and comparison of sample Tr facing wall vs facing shield. Could you do viewfactor runs with shield in Omega hohlraum as in Amendt papers (say 50% of LEH, 600 μm)?

I agree these would be interesting too, and they too will have to wait till next week. If these are to be incorporated into the manuscript, should I use a 2300 micron hohlraum or should I really make the configuration identical to that in the Amendt et al. paper?