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Referee's report on Blast-wave diagnosis of self-focusing of an intense laser pulse in a cluster medium, by Symes et al.

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I have listed concerns sequentially as they arise in the paper.

1. Abstract: MeV x-ray data….. The data is hardly presented in the paper. I would reword this along the lines that hard X-ray measurements support self-focusing, unless of course the authors are prepared to expand the hard x-ray discussion in the paper, which I do not see as necessary. Also the long pulse is referred to as 700 fs, but 1ps in the main text.
2. It is claimed that the blastwave radius scaled as $t^{1/2}$, yet we only see data at 5ns (& 1.2ns). This hardly seems adequate to make this claim. If there is additional data to confirm this, is it true along the entire length of the filament? It would seem some form of confirmatory data or explanation should be shown or given.

3. It is claimed that energy is deposited nearly uniformly along the filament in Fig 1a. The very different radius vs Z would seem to contradict this. In fact the radius of the filament near best focus is at least 2x less than that near the entrance on the left of the image. To attribute this entirely to energy deposition would require the energy deposited at the left hand end of the plasma to exceed that near best focus by ~ 2^4=16 if the blast wave scaling is to be applied, hardly uniform. This also raises the questions of the f# of the focusing optics which I was unable to find in the text. A schematic of the beam diameter vs Z overlaid as a dashed line perhaps on one of the images, or as a separate plot in Fig 1 (but on the same spatial scale as the other images) would be useful. If the beam diameter is changing significantly over the length of the image shown, this needs to be taken into account in the analysis.

4. There is also significant structure in Fig 1a. The blastwave radius does not vary monotonically with Z. For example, there is a local minimum near -1.0 mm, which should be explained. Is this for example due to some self-focusing? Again, presumably an early time image would be illuminating.

5. The explanation for Fig 1b seems plausible, although I would argue that no fringe shifts can be discerned past 0.25 mm rather than 0.1 mm. Also please record the laser conditions. Simply stating "lower energy" is not really adequate. It also makes the author wonder if there are other fielding differences between for example Fig 1c & 1d which the authors have not mentioned albeit because they deem them not especially important.

6. I agree with the assessment in of Fig 1c that there is an abrupt halt to the observable blastwave at ~ -0.5 mm, but it is not clear to me this indicates the onset of self-focusing, which results in no further coupling to the medium & no blastwave beyond -0.5mm. In fact the shape of the blast wave is very similar to those in 1a & 1b up to -0.75 mm or so, including the feature at -1.0 mm. It seems

unlikely that no blast wave would be observed if any laser energy exists beyond this point even if there were strong self-focusing. It would require a very long, and very narrow channel to have been set up for Z>-0.5 mm if the explanation given in the text were true, and no laser deposition along the channel. There are no estimates given on how long it takes the clusters to loose their focusing properties, but I imagine it is on a time scale shorter than that in ref 9 which had 30 nm Ar clusters under similar conditions (except much lower electron density). There is also no explanation given for why a narrow channel would deposit no laser energy, except that it would intercept fewer clusters. Well, yes, but isn't the beam very much more intense too? Doesn't this offset the effect to a large extent? Such estimates and more explanation might support the arguments given in the text. The authors should present this. Even better would be early time images as in ref 9, and/or a transmitted beam diagnostic. It may be that the image does indeed result from self-focusing, but the argument needs to be made more strongly.

7. Figure 1d is confusing. It has a different Z scale to the other images, and deposition clearly out to 0 mm rather than -0.5 mm. Is there something different about this shot (other than the image being taken at 1.2 ns?) Was this characteristic at 1.2 ns, or a one off?

8. As I understand it from the text, the only difference between Fig 1a,b and Fig1c,d is that the former is D & the latter H. The authors offer no explanation of why we see such different behavior. They should.

9. The assumption of Ecl remaining constant throughout the medium (pg 9) needs justification. The fact that Fig 1 b shows depletion indicates less energy at one end of the filament compared with the other. Is this not true in general? If not, why not? Also are we to assume Ecl is independent of laser intensity & therefore self-focusing (see comment in 5 too)? If so some brief explanation or reference to other work is in order.

10. Using the asymptotic blast wave relation to back out an initial radius requires some care (Fig 2). The first requirement for doing this is that the measurements are taken in the asymptotic regime, which is probably true (this occurs once the radius has increased by ~ 2x or so). The second requirement, which seems harder to justify is that EL is known vs Z. Unfortunately EL enters as $EL^{1/4}$ so that one has a lot of flexibility in what EL to choose & still match the data. The authors should comment on this in the text. Finally, to use the relation in the way the authors have requires EL to vary slowly with Z, which is true in Fig 1a & b.

11. The long pulse data is excellent. I do not agree that there are no visible fringe shifts in BB region in Fig 3 though. Seems to me there clearly are fringe shifts there, although no evidence of a blast wave or shock front. Perhaps this is exactly what the authors mean, but it doesn't read that way until you get much further into the paragraph.

12. The region near BB appears to be less than 0.8 mm long to me, more like 0.4 mm. The authors should mark on the figure what they claim to be the self-focused region.

13. For figure 3b it would be useful although not essential to see the raw data to compare with a). It is not clear at all that the dense feature at BB results from radial propagation of a shock. In fact it is likely that axial propagation of the shock waves from either end help fill in the region in BB. In fact, the densification there (ie the red bit) suggests this is the case. 3d) may support this too, ie the density profile, although admittedly noisy, does not look much like a blast wave profile. The authors should find a way to substantiate the claim or expand the discussion.

14. The value of 12 μm (pg 11 – I assume this is a diameter since 12 μm is quoted as radius on pg10 - please make this clear) for the beam radius in the long pulse expt after self-focusing seems at odds with the 800nm expt, which had a vacuum focus of 16μm and showed very clear blast waves when no self-focusing was present, albeit for less energy. Is this really consistent? Also, even if it is reasonable to use the blast wave relation to estimate beam radii, one must be certain the effect in 11) is not dominant.

15. The study on cluster size is very interesting, but very little discussion of the results in terms of cluster dynamics is given. This is clearly the key point here. The authors should expand on this. The discussion is not particularly informative as it stands. It might also be useful to plot on Fig 5 cluster size as a second "x"-axis.

16. Fig 4 has unfortunately reproduced rather badly, and the data do not appear of the quality of fig 3(a). In fact all the images in 4 look different in character to that of 3(a). In 4(a) there is some indication of multiple foci at Z < -1mm. The image looks quite different to that shown in 3(a). Again, an early time image would help elucidate this.

17. In 4(b) (annotated as 7nm not 6nm as stated in the text) I cannot see the beam "emerge" from the narrow channel as claimed (after 4.9 ZR). In 4(c), there is clearly some form of channel from the left edge to at least -1.2mm. Is this a defect? If not, why does it appear to stop at ~ -1.25mm? The authors should clarify and add pointers on the images to help the reader follow the discussion. Following the waves to ~ 10 ns as in fig 3b) or even beyond would have been instructive.

18. The channel length claimed for the 3 nm clusters (pg 12) is 1.2 ZR, whereas for the 4nm clusters it is stated as 4ZR (pg 10, Fig 3a, see comment 11), and 4.9ZR for the 6nm clusters. Is this correct & were the laser conditions the same for all these expts which presumably all contribute to Fig 5?

19. The discussion of Fig 5 (pg12) seems at odds with the claim of 4ZR for the self-focus region of 3(a) – the discussion states a threshold at 6nm clusters, not 4. This needs to be clarified.

20. The discussion of critical powers is not particularly instructive. Once the beam is above the critical power, the growth rate for self-focusing depends on intensity. It is possible to be above the power & see no self-focusing because the beam intensity is too low. It is also possible to self-focus perhaps due to the cluster effect and raise the intensity to a high enough value that relativistic self-focusing could occur over the spatial scale of the interaction.

21. There is also little quantitative discussion of time scales for cluster expansion etc & how this affects the self-focusing for the different pulses. It would be instructive to relate the current results to previous work such as in ref 9 more quantitatively if this is possible.