Response to Referee

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Thank you for your constructive comments in your review of our paper. Here are our detailed response.

1) Top of page 7 they say that they assume pure hydrogen. They can't really mean that since the cooling function assumes a solar abundance. I presume that they have not set the mean molecular weight to that of hydrogen.

This is correct. What we really meant is that the gas we considered is a single fluid with a uniform atomic mass. This part is rephrased (see bottom of Page 6 to top of Page 7), and we also explicitly stated the average atomic mass we were using to avoid confusion.

2) On page 7 they state that they have used the Dalgarno and McCray (1972) cooling function. This is a bit old: it would have been more realistic to use Wolfire et al. (1994) to get a cooling function. They could also have included heating to get an equilibrium temperature (as in Van Loo et al. MNRAS, 406, 1260, 2010), rather than just switching off the cooling at 50K.

The main purpose of this paper is to study how the self-contained magnetic field configuration can lead to different post-shock behavior. We include radiative cooling as a contributing factor to reduce the post-shock fragmentation, but it is not decisive: notice that the cooling time is shorter than the cloud crushing time, but not by a large factor. We have added text and citations to point out that the cooling included in our simulations is not necessarily very realistic, and more accurate approach could be used (see Paragraph 2, Page 29).

3) The choice of parameters for the cloud on page 8 seems somewhat eccentric. A density of 100 and a temperature of 100 is fine since this is roughly what Wolfire et al. would predict for thermal equilibrium, but the size seems very small. The translucent clumps in molecular clouds have these properties, but they are much bigger (1 pc instead of 150 a.u.). Objects this small are only interesting if they have much higher densities.The cloud size only affects the ratio of the cooling time to the other timescales. A more realistic simulation would have a very short cooling time: so short that one might as well assume that the gas is in thermal equilibrium.

We treated most of our simulation discussion and analysis using dimensionless computational unit because it allows us to scale the problem to objects with different size. In order to scale the 150 a.u. clump to the size of a globule, we have to increase the length scale by a factor of 1000. But since the only scaling parameter that is changed when changing the length scale is the simulation time scale, we would end up with the same simulation result because we use the cloud crushing time as the time unit, which also depends on the length scale. Another thing that will change when scaling up the length scale is the ratio of the clump radii against cooling length scale. When the cloud has a radius of 150 a.u., this ratio is about 5.6 (see the discussion for Comment 4). But when the cloud is 1000 times larger in size, the cooling length becomes very short comparing to the clump radii (cooling length depends only on cooling time scale and post-shock temperature thus remains unchanged), which makes the result un-scalable. We added text to explicitly pointed out the issue of scaling (see Paragraph 3, Page 11).

4) They do not tell us the numerical resolution, except to say that they

could not use AMR in the Blue Gene calculations. We really do need some assurance that the simulation is properly resolved. This is related to point 3) above since they would have had a much smaller cooling length relative to the cloud size if they had used realistic parameters for the cloud. Incidentally I don't think they can claim that the cooling time is much smaller than the cloud crushing time when they only differ by a factor of 2.

We now explicitly give the numerical resolution of the simulations, in the first paragraph of Page 9. To be accurate, we also rephrased the part "is well below the clump crushing time to ensure effective cooling and is given by" in the second paragraph of Page 11 to "is below the clump crushing time to ensure noticeable cooling and is given by". For whether the cooling is resolved, we explicitly calculate the cooling length. Since bow shock does not efficient cool, we only consider the transmitted shock. Its cooling length can be deduced from its cooling time:

$$l_r = v_{ps}\tau_r \tag{1}$$

where v_{ps} is the post shock sound speed for the transmitted shock:

$$v_{ps} = \sqrt{\frac{\gamma k_B T_{ps}}{m_A}} \tag{2}$$

k_B and m_A denote Boltzmann constant and average atomic mass unit, respectively. From the above equations, we can calculate the ratio of the clump radius to the cooling length behind the transmitted shock:

$$r = r_c/l_r \approx 5.64 \tag{3}$$

Since there are 54 zones per clump radii, we find that the number of zones per cooling length behind the transmitted shock is about 10. Although as indicated in Yirak(2010), the numerical resolution for clump cooling cannot have true convergence, we think 10 zones per cooling length is sufficient enough for our purpose. We have added the calculation of the cooling length into our paper, see paragraph 3 on Page 11.

5) In table 1 on page 9 they give eta for the cloud. This is confusing since eta is only defined in the analysis in section 5.1. It might be a good idea to swap sections 4 and 5 so that we get the analysis before the discussion of the numerical results.

To address this issue, we added a new paragraph (Paragraph 3 on Page 9) to introduce the η factor earlier so that the reader can understand what η is in Table 01. However, since the mathematical modeling is closely tied to the line plots which are derived from the simulation results, we think it is better to introduce the simulation results first, then describe the line plots to the reader and finally introduce the model that can explain the plots. Moreover, we wanted to bring out the most important part of the paper, which are the 3-D images of the simulation result, earlier in the narrative. So we left the logical order of the narrative unchanged. To be precise, we also changed "Then, assuming that the initial field configuration has ηB_0^2 stored in the perpendicular component, $(1-\eta)B_0^2$ stored in the parallel component" to "Then, assuming that the initial field configuration has $\eta B_0^2/8\pi$ stored in the perpendicular component, $(1-\eta)B_0^2/8\pi$ stored in the parallel component", because there is an 8π factor in the energy density expression.

6) In the analysis in section 5, it is assumed that the clouds all evolve to the same shape. This assumption is relaxed in appendix C. Again this is confusing. It would be better to move the material in Appendix C to section 5.

In the paper, we put emphasis on Equation 22 because its concise form and that it is a good rule of thumb: η and α can be deduced from the initial field geometry, e and μ

can be deduced from the energy contained in the contained field and the incoming shock. This equation serves our purpose of giving the reader a good understanding of what physics parameters are governing the compression phase without making things too complicated. On the other hand, Equation 42 is more precise since it removed a constraint we introduced when deriving Equation 22, it is also more complicated with confusing parameters like α_x, α_y and γ. We think it is better to make sure the reader get the basics in the main context, at which Equation 22 already did a good job, and put the more complicated explanation in the Appendix for those who are interested to explore.

7) The flow in figure 3 and 5 is not axisymmetric, although the configuration is. Of course a real flow will almost certainly not be axisymmetric because of instabilities, but these are just excited by asymmetries in the numerics. Some comments are needed here.

The TA and PP cases have axisymmetry, so in an ideal numerical simulation they should resemble 2.5-D results. However, we can observe asymmetries in the flow especially in the later frames (Figure 3, 5, 7, 9). This is caused asymmetries in the numerics we employ, another reason is the finite domain size. We have added this to our result discussion section to make sure readers will not get confused (bottom of Page 17).

8) Giving the number of cells in figures 15 and 16 is not very useful. It would be better to give the fractional volumes.

Figures 15 and 16 are changed so that the vertical axis is the fractional volumes (ratio of the volume with a certain mixing ratio to the volume of the entire domain).

9) Figure 17 serves very little purpose and is actually a false description of what happens in the shock interaction.

We removed Figure 17 and instead added more explanation in the text to make sure readers can understand how we approximate γ (See bottom of Page 32).