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Interactive comment on "Asymmetries in the Earth's dayside magnetosheath: results from global hybrid-Vlasov simulations" *by* Lucile Turc et al.

Anonymous Referee #4

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The paper studies asymmetry in the Earth's dayside magnetosheath using global hybrid-Vlasov simulations and compares numerical results with a statistical dataset of THEMIS observations. The paper is clearly written and the results are new and interesting. However, some details about modeling are missed. I partly agree with the comments of three other reviewers and mention several important points from their reports below. I could recommend the paper for publication after major revision.

Major remarks

1. Although the Vlasiator model is well known and I believe it has been thoroughly described in the literature, the paper should provide more details on the runs under

discussion. In particular, (as also mentioned by one of the reviewers) the paper says nothing about spatial resolution. It would be useful to compare the resolution with the ion inertial length and gyroradius. The authors have already answered this issue in their reply to Reviewer 1 and I suppose it will appear in the paper too. The paper does not describe the simulation domains in each case; it only mentions that their size is different between the runs. I would be also curious to know what happens if the simulated intervals in Runs 2A and 2B would be increased since now they are shorter than in Run 1.

2. Both the Reviewers 1 and 3 noted that comparison with MHD runs for exactly the same solar wind conditions will be useful because this would emphasize which variations in the magnetosheath downstream of the quasi-parallel bow shock are essentially kinetic structures and cannot be predicted by MHD models. However, I do not think that it is necessary to run all MHD models available from CCMC, but it would be enough to make three runs with at least one model (e.g. SWMF/BATSRUS).

3. I also note that the solar wind conditions in the hybrid simulations are different from the typical solar wind conditions at L1. I am satisfied with the author's reply to Reviewer 1 that the Mach numbers in the solar wind stay in the typical interval and therefore the bow shock-magnetosheath properties may not be changed in comparison with those in observations. However, I would emphasize that the solar wind density of 1 cm^{-3} is significantly smaller than the average in observations (usually between 5 and 10 cm^{-3}). I think the paper should clearly explain this because I guess that the low solar wind density may be a reason for the stronger fluctuations in the magnetosheath than those in the data.

4. Since the authors use average parameters both in the simulations and observations, I think it would be possible to add standard deviations to the figures, e.g. in the form of error bars. This would be helpful when comparing the differences between the runs (how significant is the difference with respect to the standard deviations). Besides, the authors mention in the text that they calculated longer time average intervals (line 290). How long are they and does this make any difference to their conclusions?

Minor remarks

1. The bibliography list in the paper is long, but I would like to mention two more papers, Zwan and Wolf (https://doi.org/10.1029/JA081i010p01636) who first mentioned the plasma depletion layer and Samsonov et al. (https://doi.org/10.1029/2000JA900150) who compared magnetosheath profiles downstream of the parallel and perpendicular bow shock using the anisotropic MHD model.
2. Line 83. “These processes would thus favour the quasi-parallel flank.” But the results in the paper show the $Q_{\perp}$-favoured velocity asymmetry. How is this consistent?
3. Lines 149-152. The figures in the paper show that the spatial bins are asymmetric with respect to the Sun-Earth line. Please, explain how this asymmetry is taken into account if you use the same shape as Shue et al.’s model which is symmetrical.
4. Caption to Figure 2. Please, define θ_{Bn}.
5. Lines 233-235. Is θ_{Bn} equal to $0°$ and $90°$ near the terminator plane?
6. Lines 265-266. I think it is better ”density compression ratio” instead of ”shock compression ratio”.
7. Label on Figure 5 says that the lines correspond to runs 1 & 2A and 2B but this contradicts the text (lines 265-266).
8. Figure 4. The author may add an arrow to indicate the stagnation point.
9. Lines 367-369. Is it better to say about an increase in the magnetic field on the quasi-perpendicular flank than about a decrease on the quasi-parallel flank?

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