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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.

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We thank the reviewer for their careful examination of our manuscript and their constructive comments. Please find below our point-by-point response in bold font.

The paper describes the Earth magnetosheath response to the solar wind inflow using the Vlasiator code. The focus is put on the various asymmetries of plasma and magnetic parameters in three cases with varying IMF orientation and Alfven Mach number. The results are then compared to an analysis of THEMIS observations which was published previously (Dimmock et al.'s papers). The objectives are sound, the code and

the analysis appropriate, however a number of key points make the paper not mature enough in the present form. They are listed first, then minor issues follow.

Major points: - References: the references to previous works are not adequate. Concerning hybrid codes for the magnetosheath, the literature was already vast before Vlasiator and 6D simulations of solar wind / planetary plasma interactions exist, e.g. Travnicek et al., 2007 (GRL), Hercik et al., 2013 (JGR), Modolo et al., 2017 (PSS), ... For magnetosheath asymmetries, see the works with Cluster data of Génot et al., and with ISEE data of Tatrallyay et al. For the discussions on Alfven Mach number effects see Lavraud Borovsky, 2008.

Thank you for providing these references, we will add them to the introduction in the revised manuscript.

- Foreshock effects: it seems to me that the foreshock effects are over emphasized. Actually the perturbations linked to turbulence processes in the magnetosheath are more directly connected to effects associated to the physics of the parallel shock than to the foreshock itself which lies upstream of the shock. In that respect I disagree with the last sentence of the abstract and similar statements in the paper (for instance l353). Could the authors demonstrate why the foreshock is so important and for which effects it should be distinguished with the parallel shock?

We emphasized the importance of the foreshock because the density variations in the quasi-parallel magnetosheath largely come from density variations that are already present in the foreshock and that are amplified when crossing the bow shock (see also our response to the point 2 raised by Reviewer 1). Also, these alternating patches of higher and lower densities in the magnetosheath appear to be associated with irregularities of the shock front, whose scale is comparable to that of the foreshock waves. Previous studies have established that foreshock waves modulate the shape of the shock front [e.g., Burgess, 1995]. Finally, the lower density and velocity variability at lower Mach numbers may be related to

the lower amplitude of the foreshock disturbances, or to their smaller scales.

We fully agree with the reviewer that the quasi-parallel shock physics likely also plays an important role in the quasi-parallel magnetosheath, and that bow shock and foreshock effects are hard to disentangle in this global context. In the revised manuscript, we will reword the ending of the abstract and the relevant parts in the discussion and conclusions to include quasi-parallel shock physics together with foreshock processes. We will also add more discussion as to how the foreshock affects the quasi-parallel magnetosheath, as detailed just above.

- Kinetic effects: on l300 simulation results on density asymmetry are opposed to those coming from an analysis of MHD equations. The authors point to kinetic effects. Why is it that kinetic effects matter specifically on this issue and not on other where simulations and MHD match? This requires more discussion. Even though this may be outside the scope of the paper, a comparison with 3D MHD simulation (for instance available at CCMC) would help pointing to specific kinetic effects inherent to the Vlasiator code.

We fully agree with the reviewer that it is rather surprising that one of our results regarding the plasma density asymmetry contradict MHD predictions, while a good agreement is found for all other parameters. We thought that this may stem from the fact that foreshock and quasi-parallel shock processes control to a great extent the spatial variations of the density in the quasi-parallel magnetosheath. Because the density asymmetry was more sensitive than the magnetic field strength or the plasma velocity to kinetic processes in the quasi-parallel flank, we argued that kinetic effects might dominate over fluid processes to explain the inconsistency.

However, after reconsidering our quantification of the "global" value of the asymmetries in the different runs, prompted by the first comment of ReviewerÂă2, we now find that the variation of the density asymmetry with the Mach number is actually inconclusive. Our statement regarding the decrease of the density

asymmetry level at low Mach number, which contradicts MHD predictions, was based on the median values of the asymmetry level in the different runs, which were shown to change from -5% (Runs 1 and 2A) to -2% (Run 2B). However, the standard deviations associated with these median values are 5%, 4% and 2% for Runs 1, 2A and 2B, respectively. Also, when comparing visually the curves displayed in Figure 6d-e, there is no evident difference between the different runs, again due to the large variation from bin to bin.

We will therefore reformulate the paragraph at lines 295-300 to state that there is no conclusive difference in the density asymmetry level between the different runs. We thank the reviewer for drawing our attention to this point that helped us resolve the apparent contradiction between MHD and kinetic modelling results.

- Global approach: the model is 2D in space and the magnetopause is not completely resolved such that a model magnetopause needs to be used. This puts limitation on the term "global" to qualify the simulations. I wonder if the compression/expansion in this limited 2D space can be adequately compared with the real 3D situation. Could the authors discuss this aspect? or point to literature as this has surely been already addressed.

We apologize for the lack of clarity regarding the magnetopause description in our simulations. The magnetopause is self-consistently described in our simulation, and its position is determined by pressure balance, just like Earth's magnetopause. A reliable method to evaluate the magnetopause position in numerical simulations is based on the magnetosheath flow deflection around the magnetosphere [Palmroth et al., 2003]. This method is however computationally heavy, as it requires to determine the plasma flow pattern in the magnetosheath, and the magnetopause is defined as the boundary delineating the region of space where the solar wind streamlines do not enter. Since our study does not require a precise determination of the magnetopause position, we decided to use a simpler method to get an approximate magnetopause position, or rather an inner

boundary for our magnetosheath binning. We will reformulate this paragraph in Section 2.2 of the revised manuscript to better clarify this.

As discussed at lines 364-370 of the initially submitted manuscript, the main consequence of the 2D set-up is the enhanced piling-up of the field lines in front of the magnetopause. This results in a slow expansion of the bow shock and compression of the magnetopause. Therefore, the magnetosheath thickness is somewhat overestimated in the later times of our runs. However, this should not affect the global magnetosheath parameters, except near the magnetopause where the pile-up takes place. In the revised manuscript, we will elaborate on the 2D effects in the discussion.

- Scales: could the authors give information on the temporal and spatial scales resolved in the simulations? And compare them to typical scales like inertial lengths and typical periods (inverses of plasma/cyclotron frequencies). How does this compare with the 150s used for averaging magnetosheath parameters? This would help the interpretation of density variability mentioned l289 for instance.

The spatial resolution is 300 km in Run 1 and 228 km in Runs 2A and 2B. The ion inertial length in the solar wind is 228 km in all three runs, which means that we have 1 cell/ion inertial length in Runs 2A and 2B, and 1.3 cell/ion inertial length in Run 1. This resolution is sufficient to resolve ion kinetic processes in a hybrid-Vlasov simulation (see Pfau-Kempf et al., 2018, and our response to Reviewer 1).

The ion cyclotron period in the solar wind is 13 s (Runs 1 and 2A) or 6.5 s (Run 2B). In the magnetosheath, their values are even smaller because of the larger magnetic field strength. The ion plasma period is about 50 ms in the solar wind in all three runs. The 150 s averaging interval used in our study is thus significantly larger than both typical periods, and the variability of the density cannot be linked with the ion gyroperiod for example.

In the revised manuscript, we will add the values of these typical temporal and spatial scales and compare them with the averaging interval.

- Set-up: it is not clear to me why run 1 is set up in the XZ plane and arguments are sought for to justify it mimics correctly the XY plane. Why not using a proper set up in the XY plane from the start?

We agree with the reviewer that having all three simulations in the equatorial plane would have been ideal for our study. However, global hybrid-Vlasov simulations are computationally expensive. The runs presented here required from a few million to over 10 million CPU-hours to be carried out. For this study, we decided to make use of the already existing catalogue of Vlasiator simulations that was available to us, and which included runs with upstream conditions that were appropriate for the comparative study we are presenting. Since the different simulation planes are not critical with respect to the magnetosheath properties (provided that the cusp regions are carefully excluded, as we did in Run 1), running a new simulation was not deemed necessary for the present study.

We will add a mention to the computational cost of the simulations in Section 2.1, to make it clearer why we use a run in the XZ plane.

- Observations: for comparing observations and simulations the same statistical methodology should be employed, i.e. median or average for both, contrary to what is done in the paper.

Thank you for pointing out this lack of consistency, we should indeed have used the same statistical measure to quantify the “global” value of the asymmetry in each run. In the revised manuscript, we will give the range of values for each asymmetry, rather than the median or the mean which are problematic due to the large variations from bin to bin (see our response to the first point of Reviewer 2 for more detail).

Minor points:

- l95: ‘warranted’. Do the authors mean ‘mandatory’?

We will change the wording to “better suited”.

- Figure 1: mismatch between central / outer legends and d and e labels.

We will correct this, thank you for noticing the mismatch.

- l400: snaller

We will correct the typo.

- l427: ‘statistical’. Do the authors refer to observations here?

Yes, this refers to the observations. We will reformulate this sentence to clarify this.

Additional references (not previously included in the manuscript bibliography)

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