Helsinki, May 9, 2018

Dear Referee #1,

Thank you for your thorough review of our paper. Below, we go through the points in detail; the original Referee questions are marked with italics.

The authors used a global hybrid-Vlasov simulation to study magnetosheath jets. They identified one magnetosheath jet that satisfies all the selection criteria of Plaschke et al. (2013), Archer and Horbury (2013), and Karlsson et al. (2015). They conclude that the size of magnetosheath jet is ~2.3 x 0.5 Re and the jet is generated because of an interaction of the foreshock ULF waves and the bow shock surface. These conclusions are neither substantial nor supported by the provided evidence. Therefore, the referee cannot recommend its publication in AG.

We would like to maintain that the size of the jet and its generation are important results warranting publication. Even with multi-spacecraft data, the scale-size observations have been indirect and inferred, based on statistics rather than individual structures. We think it is important to establish with a model that there is indeed a coherent structure, with generation and decay, whose size is in agreement with the interpretation of spacecraft data. Since this has not been done before, it is important to first do this rigorously and compare to different observational criteria, leading to a proof-of-concept that can then in the future be used more easily, without having to verify all different jet-like structures separately. The fact that we get similar scale sizes as compared to observations lends credibility to the observations as well, needed to properly interpret the observational studies so far. Further, the jet generation has not been verified with a model before. We further emphasize that the jet size and the generation mechanism are not the only results of the manuscript, as we also clarify how the different criteria in the literature are related, and verify that they can occur for steady IMF.

Detailed comments:

1. With just a short description about the foreshock ULF waves in the Discussion, it is difficult to understand how the high dynamic pressure is associated with the waves. The authors are required to do a detailed analysis, like what they did for an identification and validation of the magnetosheath jet.

We agree with this, and thank the Reviewer for pointing this out. Should the Editor decide to ask for a revision of the manuscript, we will add a detailed description of the upstream structure that caused the jet.

2. The authors chose 1 cm^-3 and 750 km/s for the solar wind density and velocity. The equivalent dynamic pressure is 0.94 nPa, which is considered a special solar wind condition. The authors need to explain why they chose such a condition. Can the magnetosheath only be seen in this condition or any other condition? Readers will be interesting in knowing about it.

This is due to the run conditions we originally chose. Vlasiator is a supercomputing code requiring a large computer to be run, and therefore for each run we need to separately ask for resources from different supercomputing centres. The 750 km/s is originally chosen because we have needed the solar wind to flush through the simulation box rather quickly so that the initialized magnetosphere appears without too much waiting. The density is chosen such that the combination of the density and velocity yields such an Alfvén Mach number that the foreshock will be representative of the reality. Thus we can trust the foreshock physics and consequently its bow shock interactions. The magnetosheath appears in our other runs as well, and has been verified in other peer-reviewed papers to represent reality, e.g., Hoilijoki et al., 2016 JGR.

A dynamic pressure of <= 0.94 nPa occurs 16% of the time throughout the solar cycle and 23% of the time under quasi-radial IMF, based on OMNI solar wind data for the last solar cycle. While our case does not represent the median conditions, it is not an outlier. A recently accepted review paper by Plaschke et al. (2018) states that observational statistics show a slight tendency in the jet occurrence for higher solar wind speeds and lower densities than usual. Full statistics of jet occurrence with conditions are not possible to be carried out with Vlasiator due to the computational demand, but may be possible with a limited number of runs. We are enthusiastic that such statistics could be carried out, however, first we need a detailed comparison of the different jet criteria so that we can run such a statistical study in practice.

3. A dynamic pressure of 0.94 nPa results in the subsolar magnetopause standoff distance of 11.5 Re. But the standoff distance derived from the model is about 7 Re, as seen in Figure 1. The same problem occurs for the position of the bow shock. From the movies S1 and S2, the bow shock is gradually expanding and the magnetopause is gradually shrinking. The locations of the bow shock and magnetopause never reach a steady state. This problem has made the referee think that this hybrid Vlasov model might not stable, giving unrealistic positions of the bow shock and magnetopause. To a validation of the hybrid Vlasov model, the authors are strongly suggested to add the locations of the bow shock and magnetopause to their simulation results using an empirical model.

Thank you for making this comment, this is very helpful indeed.

Regarding the expansion of the magnetopause and the bow shock, we would like to point out that in all hybrid-kinetic simulations, hybrid-PIC included, there is a gradual increase of the bow shock position for two reasons. First is the magnetic field pile-up due to the 2D setup of the run. The field piles up at the magnetopause because it cannot slip towards the nightside as in reality. We emphasize that this is a feature in all hybrid-kinetic simulations, and there is not much one can do about it. There are several other peer-reviewed papers showing this feature, indicating that it should not be regarded as a showstopper. Second, and smaller issue in our case is the artificial heating in the hybrid-kinetic simulations due to numerical diffusion. We have managed to develop such a good solver that the numerical heating stays at a tolerable level and does not largely contribute to the gradual expansion.

The simulation is initialized with the geomagnetic dipole field and the IMF pervading the box, while the plasma flows with the solar wind parameters. This causes the magnetosphere and bow shock structures to develop self-consistently during the initialization of the run. Regarding the magnetopause, we have looked more closely at the magnetopause position in Figure 1. First of all, in simulations (3D included), the magnetopause position is determined by 1) gradient of density, magnetic field or current density, 2) last closed field line, or 3) the so-called fluopause method introduced by Palmroth et al., 2003 (JGR). These parameters often do not agree with each other, while it has been shown that the fluopause gives the closest agreement with empirical models, as shown e.g., in Palmroth et al. 2003. The gradients of the abovementioned parameters vary between 2 Re in Figure 1, while the fluopause method puts the subsolar magnetopause into about 10 Re. The density enhancements that are shown closer to the earth, at about 7 Re are due to the pile-up, and they are not related to the magnetopause according to the above criteria. They originate from plasma that has been brought there before and is being squeezed by the new incoming plasma.

If a revision is decided, we will explain these issues in the manuscript and add the magnetopause position to the plots to guide the eye.

4. The definition of a magnetosheath jet is a bit confusing. In my opinion, it should go with a criterion of flow speed, but the selection criteria of Plaschke et al. (2013), Archer and Horbury (2013), and Karlsson et al. (2015) are all related with the dynamic pressure or density. The authors need to classify this issue and add a definition of the magnetosheath jets to the beginning of the Introduction.

The Reviewer is right in that especially the early observations are more related to the flow speed, while especially in the later years the vast majority of previous studies have used dynamic pressure and not velocity as the key quantity. However, since the flow speed appears quadratically in the dynamic pressure, it is also strongly reflected in the used criteria. In the revision, we will classify this in more detail in the Introduction, as the Reviewer suggests.

5. In Figure 3a, it shows that the geometry of the magnetosheath jets by Archer and Horbury (2013) is well aligned with the surface of the magnetopause. Are they really jets? The jets found by Karlsson et al. (2015), as shown in Figure 4a, look tiny and sporadic. Are they really jets? The features, which are shown in Figure 2 by Plaschke et al. (2013), are jets-like. But these jets never touch the surface of the magnetopause, which is different from the results by Plaschke et al. (2016).

We would like to point here that the observational community has adopted the term "jet", which has a connotation of an elongated feature. However, without proper modelling of them, we cannot actually say, based on the observations, what their dimensions are and what is their time evolution. It is true that some of them reach the magnetopause (while others probably do not, we cannot say this based on observations, either), indicating that at least some of them could be elongated features. However, observations nearer the shock have not estimated the sizes/shapes of jets, and therefore they may be more like "blobs" there. It is exactly the simulations that allow more detailed comparison of some of their properties (like size/shape and how large a fraction of them reach the magnetopause) that observations may be limited in inferring.

We also note that there is a magnetopause effect caused by the jet, visible in the S1 movie. We omitted this discussion because we tend to avoid making conclusions at the magnetopause due to the pileup effect. The other Reviewer urged us to add this in the manuscript and describe the magnetopause effect as well, which we shall do pending decision from the Editor.

6. The X and Y scales in Figures 3, 4, and 5 should be the same.

Will be corrected.

In summary, only one conclusion about the size of the magnetosheath jet is not substantial for a publication in AG. The authors are required to add more conclusions, such as a proof on the association between the high dynamic pressure and the foreshock ULF waves (Item 1), and the solar wind condition for an occurrence of the jet (Item 2). In addition, the authors are required to clarify the potential problem in their model (Item 3) and the issue in the definition and features of the jets (Items 4 and 5).

These will be clarified in the revision according to the above answers.

Thank you again for your very helpful and constructive comments, which will significantly increase the quality of the manuscript, we appreciate the time you spent on our work.

On behalf of all the co-authors,
Minna Palmroth