Espoo, 1.2.2021

Dear Referee #2,

Thank you for your thorough review of our paper. We have addressed all of your very good points which we think will significantly improve the quality of the paper. Below, we go through the points in detail.

The authors should clarify their claim that jets can be spontaneously born in the magnetosheath and reach levels of dynamic pressure larger than the background magnetosheath plasma state. Which physical process would allow for such extreme nonlinear plasma behavior? Also, the authors conjecture that some experimental observations of jets might be „contaminated" by spontaneously born structures like the ones observed in their simulations, looks too strong.

Thank you for this comment. There is a misunderstanding. We do not claim that jets can be spontaneously born within the magnetosheath. We mean that within the magnetosheath different processes can change the overall conditions such that local regions within the magnetosheath fulfill the jet criteria. These have been mentioned e.g., by Hao et al. (https://doi.org/10.1002/2015JA021419). These regions are obviously not those jets that the literature speaks of, i.e., those which are thought to originate at the bow shock due to bow shock – foreshock interactions (see e.g., the review by Plaschke et al., 2018, reference in the manuscript). The processes which can make the local conditions to fulfill the jet criteria are, for example, magnetosheath waves, and the transferred ULF waves from the foreshock (which can be seen as pressure ridges within the magnetosheath e.g., in Fig. 1) Since the spacecraft has no way of knowing whether a detected pressure increase formed locally or whether it was carried by the flow from the bow shock, these local regions cannot be removed from the observational jet statistics. Therefore we do not take them away from our statistics in this paper, to facilitate comparison with spacecraft observations. We have tried to make this point clearer in the paper.

The argument put forward to explain why a truncated approach disregarding electron kinetics is valid, is not convincing : „As for electrons, the electron kinetic physics can be neglected for magnetosheath jets mainly because the kinetic pressure of the electrons downstream of the Earth's bow shock is smaller by about a factor of 10 with respect to the ion pressure. Additionally, the fact that the jet dimensions are between fluid and ion kinetic scales is also an indication that an ion kinetic model should be sufficient. Naturally the electron physics inside jets is important to maintain quasi-neutrality and one will certainly find electron kinetic fluctuations that are relevant to the local thermodynamical properties of the plasma, but that is a separate problem. Hence, we conclude that the 2D3V approach neglecting kinetic electrons is sufficient to investigate the jet formation and transfer through the magnetosheath." I understand the discussion on the electron to ion kinetic pressure ratio, however, the environment simulated numerically is a collisionless magnetized plasma where the motion of particles is dominated by the magnetic and electric fields. It was shown that electron dynamics at kinetic scales, i.e. the one where the spatial scales of the order of electron Larmor radius is resolved, play a crucial role for the electric and magnetic configuration (see classical plasma textbooks like, e.g., G. Schmidt, 1966). Obviously, I do not ask the authors to perform global fully kinetic Vlasov simulations, I believe they could use some insight from theory and local 3D fully kinetic particle in cell simulations to put their simulations in the right context.

Thank you for directing us to the Schmidt book. We have now spent time in studying 3D fully kinetic simulation papers to investigate the role of electron dynamics in the propagation of the jets. One particularly interesting paper, Voitcu and Echim (JGR 2016, doi:10.1002/2015JA021973) investigates idealized plasma bubbles propagating in transverse magnetic fields mimicking northward IMF. They find that a polarization electric field forms inside the ”jet” (i.e. a bubble), and it contributes to the propagation of the “jet” across the transverse magnetic field. This process is of course neglected in our simulation. What is a pity is that the Voitcu and Echim (JGR2016) limit their studies to the transverse IMF, making it difficult to assess how the polarization electric field would affect our simulations. This is because we have a radial to Parker spiral IMF. In the radial runs the field is highly turbulent at the subsolar magnetosheath, and in the 30° cone angle runs a bit less turbulent but definitely not full-on transverse. Since according to the Voitcu and Echim papers the polarization electric field contributes to the forward propagation of the jets under transverse fields, we are not entirely sure how the effect would affect our jet propagation results; maybe primarily to the speed of the jets, which is not under scrutiny in the paper. However, as this has not been studied, we leave the more direct speculation out from the paper and note that this should be investigated in future papers. We added discussion about this effect to the paper.

A note on the normalization used in figure 5: all the physical parameters are normalized to solar wind conditions. As I asked in my previous report, to which exactly values of the solar wind variables ? Insofar simulation results are concerned, this is rather clear. I assume the normalization is performed with respect to the solar wind parameters given in Table 1. But what about normalization of MMS data? I understand the solar wind data are taken from OMNI but which values, at which moment of time ? To be more explicit: there are 6142 jets in MMS data base. I assume one value per jet is included in the histograms shown in the 3rd column of figures 5 and 6. The question is which value of the OMNI solar wind density, velocity, dynamic pressure, magnetic field, temperature is used to normalize each of the MMS data shown in Figure 5 and 6. Is this an OMNI sample extracted at exactly same UT as the MMS sample ? Or the normalizing value is derived from an average over a time interval ? Do the authors consider some time delay between OMNI and MMS data ? This question seems important to me given the variability of solar wind parameters, thus choosing one or another solar wind sample might significantly change the shape of MMS distributions shown in figures 5 and 6.

First, similar association of OMNI values has been done in [Raptis et al., 2020a, Raptis et al., 2020b] using the same MMS dataset of jets. Furthermore, other works using similar datasets also followed a similar approach (e.g. [Vuorinen et al., 2019, Plaschke et al., 2020] using THEMIS data). Finally, in [Raptis et al., 2020b] there is an extended analysis on what can be done for the association to be as accurate as possible while discussing possible limitations. For the current paper we did something similar for the normalization and for the OMNIweb association to ensure an accurate comparison. Specifically,

1. As mentioned in the current version of the manuscript (p.8 , lines 6-7) we remove jets that have a maximum standard deviation of the magnetic field rotation angle higher than 45°

2. OMNI data were (a) time shifted/lagged and (b) averaged for an ideal match to the jets observation at the magnetosheath.

This was done by taking an average of 20 (1-min resolution) data points from OMNI, starting 15 minutes before the jet observation time and up to 5 minutes after the jet observation time.

It should be noted that the OMNIweb data we are referring are the 1-min high resolution, propagated to the bow shock nose data, taken from https://omniweb.gsfc.nasa.gov/ow_min.html

This unequal averaging was done because the average time from the bow shock to MMS location is ∼ 5 minutes as discussed in [Raptis et al., 2020b]. As a result, by taking under consideration this time lag effect (5 min), we effectively take a ± 10 minutes window around the jet's associated solar wind measurements.

Therefore, we not only remove the extremely varying solar wind conditions (1) but we also take care of (2a) the time shift required from the bow shock to magnetosheath and (2b) possible variations/errors of the OMNI database by taking a 20 minute average for the conditions. We added this information to the manuscript.

I have a remark on authors claim: "Figure 5 shows an excellent overall agreement especially between the Vlasiator jets at random times and the MMS jets." When one looks at the statistical results, the overall agreement is not so excellent. Indeed, Figure 5 shows significant differences between the numerical simulations and MMS histograms/distributions: (1) extend of jets: the standard deviation of MMS observations is one order of magnitude larger than simulations meaning that observed jets span a much broader range of scales; (2) density of jets: the MMS distribution is skewed towards larger (normalized) values while simulations distribution is more Gaussian meaning that MMS observations indicate a preference towards larger (normalized) densities; (3) maximum value of velocity: the skewness of numerical simulations and MMS are significantly different, indicating different probabilities in numerical versus MMS data for different ranges of velocities; (4) maximum of the dynamic pressure: numerical simulations show a flat top distribution while the MMS distribution is skewed with one peak.

Regarding Figure 5, we think it is to a large extent a matter of taste what one means by "excellent agreement". We mean this from the perspective that we have four runs spanning four solar wind conditions, and have a finite maximum resolution in the simulation restricting to investigate the scale separation. We have reformulated the wording.

I note the authors conjecture: „As we can see from Figures 8 and 9, the jets constitute a region of plasma that has very different properties than the surrounding magnetosheath. It is perhaps like the injection of a bubble of cold air into hotter air, or a low-pressure weather system". I tend to believe they are right. Nevertheless, this brings back my criticism about neglecting the kinetic physics of electrons. Indeed, earlier theoretical papers and recent local three-dimensional, fully electromagnetic kinetic (particle in cell) simulations of jets (also called plasmoids in some earlier publications) indicate that the edges of the „bubble" are precisely the key sites where electron kinetic physics is important and effective on the dynamics of the jet, regardless the electron to ion pressure ratio.

Thank you for this comment. We have formulated the discussion on electrons referring to studies by Voitcu and Echim (2016, 2018) which address the role of electron dynamics, however, unfortunately this cannot be studied in this paper. We have recently developed an electron capability within Vlasiator (Battarbee et al., https://doi.org/10.5194/angeo-2020-31), which we could use in the future to see this in more detail.

On behalf of all the co-authors,
Minna Palmroth

References

[Plaschke et al., 2020] Plaschke, F., Hietala, H., and Vörös, Z. (2020). Scale sizes of magnetosheath jets. *Journal of Geophysical Research: Space Physics*, 125(9):e2020JA027962. e2020JA027962 10.1029/2020JA027962.

[Raptis et al., 2020a] Raptis, S., Aminalragia-Giamini, S., Karlsson, T., and Lindberg, M. (2020a). Classi- fication of magnetosheath jets using neural networks and high resolution OMNI (HRO) data. *Frontiers in Astronomy and Space Sciences*, 7:24.

[Raptis et al., 2020b] Raptis, S., Karlsson, T., Plaschke, F., Kullen, A., and Lindqvist, P.-A. (2020b). Clas- sifying magnetosheath jets using MMS: Statistical properties. *Journal of Geophysical Research: Space Physics*, 125(11):e2019JA027754. e2019JA027754 10.1029/2019JA027754.

[Vuorinen et al., 2019] Vuorinen, L., Hietala, H., and Plaschke, F. (2019). Jets in the magnetosheath: IMF control of where they occur. In *Annales Geophysicae*, volume 37, pages 689–697. Copernicus GmbH.