Author response to referee #1

The authors wish to thank Anonymous referee #1 for their very insightful and thorough comments on the manuscript. We will take the comments into account when revising the manuscript. In this document we provide responses to each of the referee's comments (formatted as italics in indented paragraphs).

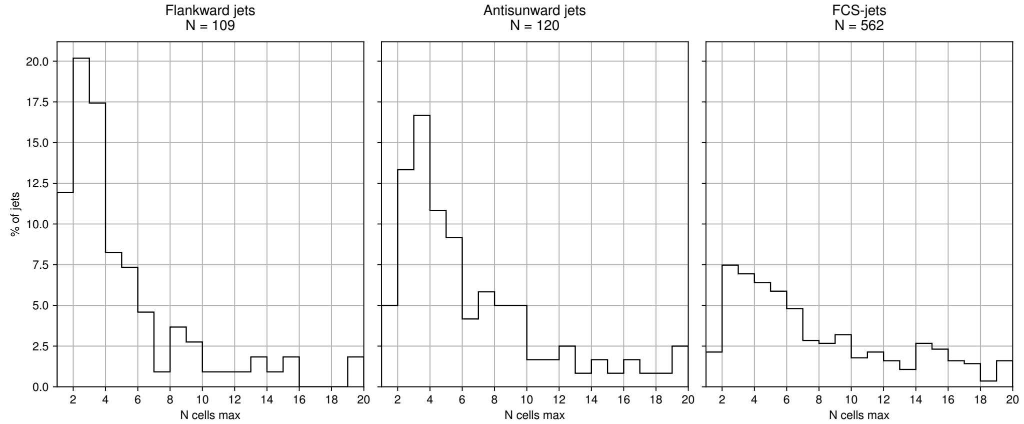
> *In my opinion, the main question is: "What physical processes lead to the formation of jets in the simulations discussed?". The answer to this question remains unclear. In lines 299-301, the authors write: "The intrusion of solar wind plasma into the magnetosheath resembles what is expected by the SLAMS theory of jet formation by Karlsson et al. (2015), but here this applies to structures of dynamic pressure and magnetic field enhancement compared to the magnetic field enhancement that defines SLAMS." Karlsson et al (2015) do not present a "theory". Instead, they simply cite some SLAMS related computational works and Jovian magnetosheath observations. When Karlsson et al (2015) cite the work by Karimabadi et al. (2014), they say "SLAMS have been proposed to penetrate the bowshock and convect into the terrestrial magnetosheath during certain circumstances". This argument, however, is not directly linked to the formation of jets (dynamic pressure enhancements), observed by Karimabadi et al. (2014) in their 2D simulations and claimed to be caused by the ripple formation mechanism without providing arguments to support that claim. In fact, a more recent 3D computational study by Omelchenko et al. (2021) established a connection (cited in this manuscript) between the magnetosheath turbulence and formation of jets capable of penetrating the magnetosheath and impacting the magnetopause. Interestingly, their 3D jets look very similar in characteristics (density, magnetic field, speed, but not geometry) to the 2D jets demonstrated by Karimabadi et al (2014). Moreover, these jets do not seem to be related to SLAMS that normally originate in the foreshock, further away in the sunward direction. As far as the study by Karlsson et al (2015) is concerned, they simply hypothesize that "a small-amplitude foreshock SLAMS may encounter a corrugation in the bow shock (due to either a large- amplitude SLAMS or an HFA) and cross the bow shock". In this manuscript, the authors must support their conclusion that the nature of simulation jets "agrees with the formation mechanism proposed by Karlsson et al. (2015)" (line 308). For that, the authors must prove that these jets are in fact SLAMS, caused by the steepening of sunward propagating ULF waves – and not another type of plasma structures, e.g., ones that result from the turbulent action of magnetic field, as proposed by Omelchenko et al. (2015), whereby high-speed jets effectively represent "parcels" of upstream solar wind plasma capable of deep penetration into the magnetosheath. This extra analysis needed may be facilitated by comparing the characteristics of jets discussed in these simulations with those of SLAMS or high-speed jets (HSJs) observed experimentally. A mere classification of numerical "jets", carried out in this manuscript, is not sufficient for proving the ability of these simulations to reproduce data obtained in relevant satellite observations.*

It is true that Karlsson et al. [2015] simply make a hypothesis based on references to previous studies and observed similarities between paramagnetic plasmoids and SLAMS. Palmroth et al. [2018] and Raptis et al. [2020] found evidence in favour of the hypothesis in a simulation and in MMS spacecraft data, respectively. Based on this evidence, and the fact that Vlasiator simulations have been found to accurately model foreshock processes [Palmroth et al., 2015, Turc et al., 2023], SLAMS and bow shock reformation [Johlander et al., 2022], and magnetosheath jets [Palmroth et al., 2021] based on comparisons with spacecraft data, we further studied jets in Vlasiator simulations in Suni et al. [2021] and found that a majority of the jets were associated with foreshock structures of enhanced dynamic pressure and magnetic field. While the majority of the foreshock structures did not fulfill the criteria for SLAMS, some did fulfill the SLAMS criteria. The results of Suni et al. [2021] suggested that SLAMS and weaker foreshock magnetic field enhancements are part of a continuum of foreshock compressive structures of varying amplitudes, and thus we concluded that jets can form through a mechanism that is similar to the one hypothesised by Karlsson et al. [2015], even if the structures in question are not actually SLAMS. Raptis et al. [2022] found direct spacecraft evidence for a connection between foreshock wave evolution, compressive foreshock structures, bow shock reformation, and the formation of magnetosheath jets. It should be stressed, however, that these results do not disprove either the bow shock ripple mechanism of Hietala et al. [2009] or the magnetokinetic mechanism of Omelchenko et al. [2021], they only show that the majority of jets in Vlasiator are caused by foreshock compressive structures interacting with the bow shock and magnetosheath.

The manuscript will be revised to focus the discussion about formation mechanisms of FCS-jets and antisunward jets on the results of Suni et al. [2021] and Raptis et al. [2022]. In addition, we will refer to previous studies that have found agreement between Vlasiator and spacecraft observations.

> *The dynamic pressure structures ("jets") shown in the Figures (e.g., Figs. 1,7) are "dot- like". It is unclear if the simulations resolve them at all. How many computational cells do these "jets" spread over? The manuscript appears to claim that these structures are SLAMS. However, SLAMS are known to be characterized by large-amplitude magnetic perturbations (> 3 IMF strength) that initially propagate in the sunward direction in the foreshock. I do not see any proof of this mechanism of formation in the Figures presented. Do these simulations see SLAMS in the foreshock? In fact, these "dot-like" jets, if not properly resolved numerically, may be numerical artefacts, or LOCAL nonlinear effects associated with wave steepening/breaking (not to confuse with steepening of PROPAGATING wave fronts which may lead to formation of SLAMS).*

The black and red dots in Fig. 1 and the grey crosses, blue triangles and orange dots in Fig. 7 mark the weighted centers of jets as defined in Section 3.1, not their actual spatial extents. Approximately 12% of flankward jets, 5% of antisunward jets, and 2% of FCS-jets have a maximum size of only 1 cell (see Fig. 1). Excluding single-cell jets does not change the result of our analysis.


Flankward jets
N = 109

Antisunward jets
N = 120

FCS-jets
N = 562

Figure 1: Histograms of maximum jet sizes in units of simulation cells divided by category. Jets larger than 20 cells exist as well, but the x-axis has been limited to the range [1,20] for better visibility of the distribution of small jets.

It was not our intent to imply that these structures, or any other structures in the magnetosheath, necessarily originate from SLAMS. The text as a whole will be revised to de-emphasise any perceived role of SLAMS in the formation of these simulated jets and to emphasise the role of foreshock compressive structures instead.

> *The authors emphasize that the jets they study "are required to form at the bow shock" (line 124). For this purpose, they use three different ways to define the bow shock numerically (line 126). However, all*

> *these definitions use instantaneous simulation information. For instance, the authors state that they cannot use the density boundary because it fluctuates due to shock reformation (lines 133-134). In my opinion, using the other (temperature and Mach defined) boundaries as proxies for finding the shock location is not a safe physical approach for jet classification either. A more physically sound approach would be using time-averaged (slowly fluctuating) shock boundaries, e.g., similar to ones demonstrated by Ng et al. (JGR, Hybrid Simulations of the Cusp and Dayside Magnetosheath Dynamics Under Quasi-Radial Interplanetary Magnetic Fields, 2022). The authors should further discuss the uncertainty of their approach.*

Both the method used in this study and the one in Ng et al. [2022] have merits and value, however, we want to study the transmission of transient structures through the bow shock and into the magnetosheath, so using instantaneous boundaries is necessary to correctly identify the contact between upstream and downstream structures. The reason that the density-based boundary is not used for this purpose is because the density of the upstream structures increases as they approach the bow shock, causing them to sometimes fulfill the density bow shock criterion and thus be counted as magnetosheath plasma according to the density criterion before they merge with the existing magnetosheath. This bow shock reformation has been studied in Vlasiator before [Johlander et al., 2022].

We will revise the manuscript to address this and compare and contrast our approach with that of Ng et al. [2022].

> *In the last paragraph of Section 2.1, the authors do not provide enough physical and numerical information about the setup of 2D Vlasiator simulations they analyze in this manuscript. At the very least, they should provide the (1) domain size in solar wind proton inertial lengths, (2) mesh resolution (numbers of cells in each dimension), and (3) simulation magnetopause to obstacle radius ratio (which characterizes the simulation dipole strength chosen). For the reader to understand how realistic these simulations are, the authors should also discuss how they scale their ion inertial length scales to RE distances shown in the Figures.*

Vlasiator uses an unscaled dipole, and the values of the plasma and magnetic field properties in the simulation are in SI units.

We will add this information to Section 2.1 along with descriptions of the domain sizes. The real space and velocity space resolutions can be found in the caption of Table 1. For reference, the real space resolution will also be reported in units of d_i in the solar wind.

> *Lines 160-165: It is not clear what the authors did here. VSCs are immobile points, aren't they? If so, their frame of reference coincides with the simulation frame of reference. Therefore, it is unclear what v_{SC} ("the propagation velocity in the spacecraft frame") represents in this numerical analysis. Does it make sense to measure it with respect to any point in the simulation (e.g., VSCs)? Isn't it just v_n? Please revise this paragraph for clarity.*

The references to a "spacecraft frame" are because the timing method used was developed for use with observations by real spacecraft. In our study the VSCs are indeed stationary and thus the spacecraft frame is identical to the simulation frame. We will change v_{SC} to v to hopefully clear up any confusion. The reason why v_n on its own is not used, but instead a "corrected" velocity is calculated, is that v_n does not account for the plasma bulk velocity perpendicular to the propagation direction of the assumed plane wave. The corrected velocity v does take it into account.

> *Section 5 (Conclusions) needs to be revised in accordance with my previous remarks, especially those regarding "the formation mechanism proposed by Karlsson et al (2015)", who simply hypothesized about the origin of jets. There is no so evidence in this manuscript that the magnetosheath structures discussed are the SLAMS discussed by Karlsson et al (2015). The conclusion should also make clear which "jets" the authors refer to, given the nomenclature adopted in this manuscript (e.g., FCS-jets vs non-FCS jets). In regard to the simulation jets, the Conclusion mentions: "These properties indicate that they might form behind a part of the bow shock that locally and temporarily changes from quasi-parallel to quasi-perpendicular". This is not consistent with the SLAMS related explanation pointed out by Karlsson et al (2015). The statements in the Abstract and Conclusions sections must be clear and consistent.*

The sentences "These properties indicate that they might form behind a part of the bow shock that locally and temporarily changes from quasi-parallel to quasi-perpendicular" refers to the proposed explanation of the formation of flankward jets only. While this local change of the bow shock is proposed to be related to foreshock ULF waves, these ULF waves are not necessarily compressive in the way required for the formation mechanism found in Raptis et al. [2022].

As mentioned above, we will revise the manuscript to de-emphasise how the Karlsson et al. [2015] SLAMS hypothesis relates to what we see in our simulations and to emphasise the results of Suni et al. [2021] and Raptis et al. [2022] instead.

> *The Abstract contains many insignificant details which obscure the main results of this work. The Abstract also contains information that must be supported by references, for instance: "A jet generation mechanism that has been widely discussed in observational and numerical studies is steepened Ultra Low Frequency (ULF) waves interacting with the bow shock. However, other formation mechanisms have also been proposed". For clarity, such statements should be avoided in the Abstract.*

We will revise the abstract to remove statements requiring references.

> *Line 44: replace "is host" by "is a host"*

We will reformulate this sentence to "The dynamic quasi-parallel magnetosheath exhibits many kinds of transient phenomena".

> *Line 82: replace "E.g." by "For instance,"*

We will implement this change as requested.

> *Line 87: replace "simulation runs" by "two-dimensional simulation runs" (it is important to emphasize 2D at the very beginning to avoid further confusion)*

We will implement this change as requested.

> *Last two sentences in Section 1. It is not clear what types of "jets" are being studied in this manuscript and what makes "non-FCS jets" different from "FCS jets". This must be discussed before the reader gets to see results from the statistical analysis. Also, this manuscript lacks comparisons with observations. That must be mentioned/explained.*

In Suni et al. [2021], we found that FCS-jets have different properties (at least in terms of magnetosheath penetration depth) than non-FCS-jets, but we did not study their differences in detail and did not conclude that there must be a fundamental difference between FCS-jets and non-FCS-jets. In the present paper, we find that there is no fundamental difference between the properties of antisunward non-FCS-jets and FCS-jets. The manuscript will be revised to de-emphasise any implied fundamental differences between FCS-jets and non-FCS-jets and clarify that categorisation based on connection with foreshock structures is just one of many possible ways of categorising jets.

The lack of comparisons with observations is due to the fact that categorising jets based on propagation direction requires multiple points of observation for the timing analysis, which in the case of spacecraft observations would mean spacecraft constellations in tight formation. Combined with the difficulty of ascertaining through spacecraft observations whether a jet forms at the bow shock or not means that suitable spacecraft observations to compare to are very rare. It should however be noted that Palmroth et al. [2021] did compare jets found in the very same simulations studied here to MMS observations, finding that they are quite similar in their properties. The manuscript will be revised to explain the lack of comparisons with spacecraft observations and that previous studies have found jets in Vlasiator to be realistic.

> *Line 108: mention that you are using GSM axes*

The simulation coordinate system is GSE, but because our simulation runs have a dipole that is aligned with the GSE z-axis, it is equivalent to GSM in this case. The lack of dipole tilt and the relation between GSM and GSE will be mentioned in the revised manuscript.

> *Line 129: provide a definition of "magnetosonic Mach number" to avoid confusion. Are you using the magnetosonic speed,* $v_{\mathrm{ms}} = \sqrt{v_a^2 + v_s^2}$ *so that* $M_{\mathrm{ms}} = v_{\mathrm{sw}}/v_{\mathrm{ms}}$*?*

This is indeed the definition we are using. The definition will be explicitly mentioned in the revised manuscript.

> *Line 131: rephrase "the position space simulation cells" (what does it mean?)*

By "position space" we mean the 3-dimensional space in GSE x, y, z coordinates. We will reformulate this to "the simulation cells in position space"

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