Author response to referee #2

The authors wish to thank Anonymous referee #2 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).

> *paper often refers that the results support the model by Karlsson et al 2015, but that paper states "An unambiguous answer to the question of how magnetosheath jets and plasmoids are formed is not available, and it is outside the scope of this paper to provide it." It would be very helpful to be more specific on what exactly in Karlsson et al 2015 paper the current study supports. It should be very clear what jet formation model the current study supports and clearly show evidence for that.*

Indeed, Karlsson et al. [2015] base their hypothesis of jets being caused by SLAMS on previous simulation and observational studies, as well as observed similarities between SLAMS and paramagnetic plasmoids in the magnetosheath. Because evidence in favour of this mechanism was found in both spacecraft observations [Raptis et al., 2020] and simulations Palmroth et al. [2018], we investigated the transmission of compressive foreshock structures of varying amplitudes of magnetic field enhancement through the bow shock in Suni et al. [2021] and found that a majority of the jets found in these four Vlasiator simulation runs are connected to such foreshock structures. Though the majority of these structures were found to not fulfill the SLAMS criteria, we nonetheless concluded that we had found evidence for the part of the Karlsson et al. [2015] hypothesis that concerns the transmission of structures from the foreshock to the magnetosheath. Raptis et al. [2022b] observed a connection between foreshock waves, compressive structures, bow shock reformation, and magnetosheath jet formation directly using MMS spacecraft observations, further supporting this formation mechanism. We also found that SLAMS and the weaker, more frequently forming structures that are associated with the formation of the majority of jets are part of a continuum of foreshock structures of varying magnetic field enhancement. It should also be noted that SLAMS and their role in bow shock reformation have been studied in Vlasiator simulations [Johlander et al., 2022].

We will revise the manuscript to de-emphasise the aspect of the Karlsson et al. [2015] hypothesis that specifies SLAMS as a possible origin of jets, and instead refer to the results of Suni et al. [2021] and Raptis et al. [2022b].

> *It seems that the current manuscript is updating on the earlier results and shows that the previous division of jets in FCS and non-FCS type of jets is physically not appropriate. Just to remind that Suni et al 2021 was concluding that FCS and non-FCS jets have different sources, while this manuscript shows that it is not true. Therefore, it is unclear why the manuscript wants to distinguish between FCS and antisunward jets if their properties are similar. The conclusion section is written in a way that the separation between FCS and antisunward jets is not mentioned, and it would be appropriate to rewrite the whole paper also in the same way. FCS separation can be mentioned in the introduction or discussion in terms of putting in perspective the current manuscript to the earlier work but it is not appropriate from the point of view of discussing the main results of the paper. If authors want to keep FCS notations, then it has to be defined in a way that it includes antisunward jets.*

In Suni et al. [2021] we found that FCS-jets and non-FCS-jets are different in some of their properties, such as magnetosheath penetration depth, but we did not study these differences in detail and did not conclusively find that there must be fundamental differences between FCS-jets and non-FCS-jets. In this study, we find that because antisunward jets appear to form the same way as FCS-jets, the separation into FCS-jets and non-FCS-jets as defined in Suni et al. [2021] does not produce a clear separation of formation mechanisms.

We will revise the manuscript to emphasise that categorising jets based on whether they are connected to FCS or not and what way they propagate are simply two different ways of categorising jets, with the former focusing on the origins of the jets with respect to a specifically defined structure, and the latter on the properties of the jets. We will also clarify that the set of FCS-jets appears to not contain all the jets that are associated with foreshock structures of enhanced dynamic pressure. The ones not contained in the set are the antisunward non-FCS-jets, which are apparently connected to foreshock structures that are too weak to fulfill the FCS criteria as defined in Suni et al. [2021] and this study. In the discussion, we will specify that in terms of formation mechanism, antisunward non-FCS-jets and FCS-jets appear to be the same, and that the separation method in Suni et al. [2021] produced an artificial divide between them.

> *As a follow-up on the previous point, the abstract is too long.*

We will shorten the abstract by removing statements that require references.

> *The methodology part requires improvements.*

We will attempt to remedy this by expanding the model and methods section to describe in more detail the technical aspects of the simulation. Additionally, we will elaborate on the timing analysis method in the jet classification section.

> *What is the spacecraft frame, and how it distinguishes from other frames?*

The term “spacecraft frame” is a remnant of the fact that the timing method was developed for observations by real spacecraft. In the case of our simulations the VSCs do not move and thus the spacecraft frame is identical to the simulation frame. We will change v_{sc} to v in the revised manuscript to clarify this.

> *What exactly is spacecraft timing done - does one times pressure increase, pressure maximum, or just correlates pressure time series? There is no example showing how well the timed series align and how good is the plane wave assumption. In simulations no nice planar wave structures are seen.*

The dynamic pressure time series are cross-correlated with each other to give time lags that are then translated into velocity based on the separation of the VSCs. Indeed, many of the jets studied in these simulations are similar in size to the VSC separation and cannot be considered planar. However, the requirement that the smallest of the maximum cross-correlation coefficients (we get one maximum for each cross-correlated pair of time-series) should be at least 0.8 (the maximum possible being 1) ensures that the dynamic pressure enhancements observed by different VSCs are similar to each other in shape and environment and are thus most likely part of the same structure, giving a reasonable estimate of the propagation velocity even though the structures are not planar. We will explain this in the revised manuscript.

> *There is no discussion or explanation of what is the physical meaning of estimating vsc and vtr, and what are the expected differences.*

v_{sc}, now renamed v, is derived from observations of dynamic pressure enhancements as temporal structures at a limited number of stationary points in space, which is similar to how spacecraft constellations observe in reality.

v_{tr}, on the other hand, is based on the motion of a spatial structure defined as a collection of spatially connected simulation cells whose dynamic pressures exceed the jet threshold.

These velocities are expected to be virtually identical in situations where the dynamic pressure enhancement is spatially large enough to be observed by all three VSCs and its shape is not complex and does not change significantly as it propagates. If the enhancement is spatially very small (on the order of 1 simulation cell), the fact that the tracking method considers only cells whose dynamic pressure fulfills the jet criteria while the timing method considers any dynamic pressure enhancement can lead to differences in the velocities.

The observation that the median v_{tr} is smaller than the median v in Figure 2 in the manuscript is probably due to the elongation of jets after they form at the bow shock, causing a significant change in shape and thus a difference in the propagation of the weighted center of the jet (v_{tr}) and the peak of the dynamic pressure enhancement (which is what v is based on).

We will explain this in the revised manuscript.

> *Recent studies, such as Raptis et al 2022, show that simply looking on moments is not enough, it can be too simplified and misleading picture in understanding jets as plasma can contain different populations in the same location. Vlasov simulations have the great advantage of having the full distribution function and allowing to resolve the different populations in distribution functions. At least for the case studies it would be important to include the distribution functions of jets, to support the usage of moments.*

While our simulations have VDFs in every spatial cell during runtime, it is unfortunately unfeasible to save all VDFs in the output data files as this would increase the file size from $\sim$2 GB to $\sim$2 TB. Due to this, VDFs are saved for only certain cells. Of the non-FCS-jets investigated in this study, only one happens to overlap with a cell whose VDF is stored. We have analysed this VDF and found that indeed, slightly after the jet starts overlapping with the VDF-carrying cell, which is at the bow shock, the reduced 1D distributions show evidence of two maxima in the plasma distribution function. Some FCS-jets overlapped with VDF-carrying cells deeper in the magnetosheath. In these cases, the reduced 1D distributions show bumps on their tails, but at no point did we see unconnected populations in the distribution functions. However, studying the possible effects of multiple populations on moment calculations is outside the scope of this study.

We will revise the manuscript to mention that these multiple populations could be an issue when calculating moments under the assumption of a single population, with a reference to Raptis et al. [2022a].

> *l.13 propagate > propagating*

We think “propagate” is correct here.

> *l.156 It is confusing to have VSC as a notation for virtual s/c as later one uses also v_{sc} for the velocity of the dynamic pressure pulse.*

We will change v_{sc} to v, for the reasons described above, which should hopefully clear up any confusion.

Figure 5: red, orange, and pink are too close colors to be distinguishable. Valid also in other figures.

We will change the colormaps and contour colours in figures 1,3,4, and 5 for better visual clarity.

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