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The Local Bow Shock Environment during Magnetosheath Jet Formation: Results from a Hybrid-Vlasov Simulation

High speed jets in quasi-parallel shocks, *submitted to Annales Geophysicae,*

The manuscript analyzes plasma structures identified by the authors in their previous simulations.

Major concerns:

1. 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.

2. 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).
3. 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.
4. 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.
5. 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.

6. 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.

Minor concerns:

1. 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.
2. Line 44: replace “is host” by “is a host”
3. Line 82: replace “E.g.” by “For instance,”
4. Line 87: replace “simulation runs” by “two-dimensional simulation runs” (it is important to emphasize 2D at the very beginning to avoid further confusion)
5. 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.
6. Line 108: mention that you are using GSM axes
7. Line 129: provide a definition of “magnetosonic Mach number” to avoid confusion. Are you using the magnetosonic speed, v_ms = sqrt(va^2+vs^2) so that M_ms = v_sw/v_ms?
8. Line 131: rephrase “the position space simulation cells” (what does it mean?)