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Interactive comment

Interactive comment on "Non-locality of the Earth's quasi-parallel bow shock: injection of thermal protons in a hybrid-Vlasov simulation" *by* Markus Battarbee et al.

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We wish to thank the referee for the helpful review.

The manuscript analyses Vlasov numerical simulations including test particle runs of the quasi-parallel Earth's bow shock and ion acceleration there. The concept of "shock non-locality" is introduced and it is shown that the non-locality has little direct effect on particle injection. Instead the injection takes place in a larger region surrounding the shock but at the same time local magnetic field distortions at the shock are important for the injection.

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Discussion paper

Abstract promises a novel method for spacecraft data analysis. It is not clear what is the novel method, why and how it should be applied and what would be the outcome. This needs to be clarified.

Thank you, we will clarify that the proposed new analysis is calculating non-locality from a combination of three plasma measurements.

Simulation initialisation:
- Why 5° tilt in the magnetic field?

In this study, we chose to focus on the quasi-parallel bow shock. The 5 degree run was chosen as it provided a large region where global curvature effects were captured within the quasi-parallel bow shock.

- Is 43eV solar wind plasma temperature motivated by the velocity resolution?

This is correct. In order to ensure the shock dynamics are properly modelled, the incoming solar wind distribution must be adequately resolved by the velocity grid. This is verified by ensuring there is no numerical heating as the distribution propagates from the inflow boundary to the shock.

- There should be a proper discussion why simulating the system with a simulation having the spatial resolution larger than the characteristic ion inertial and ion gyroradius scales is appropriate to address the problem of ion injection where most of the ion reflection can occur on ion kinetic scales. This points needs to be clearly addressed as it may affect the general conclusions of the paper.

We thank the referee for the feedback. We intend to improve this discussion in the paper with the following reasoning. Our simulations choose to emphasize the global dynamics due to, e.g. curved bow shock reformation as ULF waves impinge upon it. We acknowledge that there may be additional ion effects at smaller kinetic scales, pending further study and resource expenditure. A convergence study would be a very expensive undertaking, but we intend to further analyse these effects in future studies. To our knowledge, there doesn't exist a study yet which would invalidate the dynamics seen with this resolution.

It is not clear why authors have chosen to do the test particle approach if the Vlasov code is supposed to follow the full distribution function. This point needs to be clearly explained.

On line 210-213 we state: "Following the evolution of distribution functions does not allow for tracing of particle histories. In order to evaluate injection probabilities, particles need to be tracked as they meet the bow shock and interact with it, ultimately either returning to the upstream or being transmitted to the downstream." We will reword this for added clarity, highlighting how we actually use these test-particles as a method of tracking the evolution of a small portion of the VDF.

The concept of non-locality is introduced which does not include magnetic field. The motivation is that it provides poor results while at the same time paper mentions that magnetic field structures are very important for the injection. All this makes the motivation for the non-locality concept very unsatisfactory and it is not clear what authors mean motivating their selection by having poor vs good results. Magnetic field data is one of the primary datasets in the shock analysis and it is unclear why one would want to exclude it from the shock definition. In general, it is not clear why authors want to introduce a new concept.

We acknowledge that magnetic field measurements are often used in spacecraft. We will clarify this section, stating that (in agreement with analytical studies of quasi-parallel shocks resulting in little magnetic field compression), the magnitude of magnetic field at the quasi-parallel shock showed multiple successive enhancements and rarefications. As our proposed method depended upon the measurement reaching a conclusive downstream state, the magnetic field magnitude as such was insufficient. We do, however note that the magnetic field does have a role in the calculation of the shock-normal magnetosonic Mach number, so the magnetic field is not ignored.

In Figure 4 100eV case of test particles is shown. It should be motivated why this particular case is shown and not for example the case of Maxwellian distributed particles.

The Maxwellian case appears very similar to the presented 100 eV case, so we shall replace the figure. We also note that the evolution of all performed test-particle distributions can be examined in supplementary movies B and C.

Figure 5 results and discussion are not fully consistent and should be significantly improved. For example, showing injected particle results (column 1 and 2) one makes conclusion that particles with energies below solar wind drift energy are loosing energy on average and particles above are gaining. This results is inconsistent with that the figures shows most of the injected particles have high energy. Such high energy particles if they start at solar wind energy and then during some part of the orbit have energies below the solar wind energy then on average at low energies the energy gain and loss should be equal. If the statement made in the manuscript is true then why there are no low energy injected particles (while there are still a lot of low energy particles at r<0 and they all show negative energy gain.

We wish to thank the referee for pointing out this error. We did additional analysis of our results, and found that when binning the particle energization changes, the script made an erroneous assumption that energy changes per time step would be very small. Due to each energy change being recorded at the end value, the plot emphasized energy gains at high energies and losses at low energies.

We are in the process of fixing our analysis and completely redoing figure 5 and the associated interpretation.

Similarly, it is not clear how the current simulations results contradict the re-

sults from the Johlander et al. Firstly, it is not clear if SLAMS are observed in the current simulation and if they are do they have similar properties as in the observations? Secondly, when comparing with Johlander et al., it would be good to do the comparison in an adequate way, so that one understand how one should translate the results from the Vlasiator case to another cases such as Johlander et al. For comparison with those results one would need to look at solar wind ions that have different kinetic energy in the shock frame and see the differences in the injection rate. The authors should guide the reader where and how this can be seen.

We acknowledge that the comparison was very brief. In Johlander et al, Fig. 5, low-energy particles were found to be likely to reflect from SLAMS, whereas fast particles passed through them. In the quasi-parallel region, structures such as SLAMS merge into the bow shock, and thus, there is some merit in comparing how particles interact with SLAMS vs how they interact with the bow shock. Our results showed that particles with a large energy in the solar wind frame were more likely to be injected, but we acknowledge that a high solar wind frame speed can result in both faster and significantly slower shock-frame particles. We will perform additional tests based on the shock-frame energy and revise the results accordingly.

Figure 6 requires several clarifications. The largest structuring of the injection probabilities is seen in the dependance on the impact position angle. Instead of trying to resolve the physics of the large injection rate variations authors suggest how to smooth these variations which suggests that authors themselves maybe do not trust the numbers. This needs to be clarified.

We indeed expect that panel to not depict any underlying erratic dependence on shock-normal angle, but rather, to be indicative of how our particle injection time window was not long enough to encompass a sufficient amount of shock reformation cycles. We will clarify this reasoning. Due to the large spatial extent and the large amount of test-particles, we still believe our statistics are sufficient to probe other properties of particle-shock-interactions.

In an ideal world, we would use a longer period of time for our test-particle study, but such a simulation set is not available at this time.

Another unclear point is how shock non-locality is defined for a particle that starts at one position and gets injected at another position (in general valid for all particles). From which time and position are the given shock non-locality values. Similarly, it is not clear at which time instant is measured the bow-normal angle.

We acknowledge that the statement on lines 289-290: “For each test-particle, we evaluate these properties at the first time the particle reaches a point in the simulation space that fulfills the solar wind core heating ($T_{core} > 4T_{sw}$) criterion.” was somewhat hidden. We shall clarify this point in the text and the caption.

Minor things:
L.8 fix the language of the sentence.

Thank you for this correction.

l.45 The work of Johlander et al. 2016 does not make the mentioned assumptions in the manuscript but shows that SLAMS can contribute to the injection.

The referee is correct, the assumptions of the test-particle study in Johlander et al (2016) differ in that they investigate a SLAMS instead of a planar shock front. We will correct this mistake.

l.59 It is a bit confusing in which reference frame particle gains energy in the definition of the energization. For example, a particle reflected from a shock can have lower energy in the shock reference frame than the solar wind particles (e.g. when reflected from SLAMS) but it would not be "part of the incident thermal distribution".

We thank the referee for making this important point, and we will add it to the manuscript.

l.165 the division of the core distribution is unclearly described.

We will reword this to the following: “The Vlasiator distribution function is separated into core and suprathermal parts ($n_{p,core}$ and $n_{p,st}$). Each velocity space cell is evaluated as belonging to the core distribution, if it is inside a sphere centred at u_{sw} = ($-$600, 0, 0) km/s and with a radius of 690 km/s. Cells outside this sphere are considered as belonging to the suprathermal distribution.”

l.368 this should be illustrated and quantified, adding by the figure

We will reword this for clarity – this in fact was referring exactly to Figure 5, which shows changes in particle energy as measured in the simulation frame. We will also amend the caption of Figure to this effect, and if necessary, change the text according to our re-done analysis.

l.411 What do you mean by "high-fidelity"?

We will replace “high-fidelity” with “noise-free”.

Figure 1: please use slightly thinner lines, the structure of the pink line cannot be resolved in the figure due to the thickness of the line.’

We did not intend Figure 1 to be used for evaluating the mesoscale bow shock shape, instead highlighting this in Figure 2. Nevertheless, we will redo Figure 1 with a smaller line width to show the details already in this image.

Table 1: Why comparison is done with suprathermal densities? I assume that from Vlasiator one can estimate the flux of reflected particles and thus have a good estimate of the injection rate.

Within the foreshock region, the suprathermal density indeed is a close measure of reflected particles, with only possible very minor contamination from the solar wind core during flow deflection events. The datasets of Vlasiator are too large (several terabytes) to store all data at every time step, thus we use reduced measurements such as the described split into core and suprathermal portions of the distribution function. We have added a description to explain that the suprathermal density is a good measure of reflected particles, adding the note that it includes all particles which have been reflected, even a long time ago, and are currently in the upstream.

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