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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.

Anonymous Referee #2

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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.

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.

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Discussion paper

Simulation initialisation: - Why 5° tilt in the magnetic field? - Is 43eV solar wind plasma temperature motivated by the velocity resolution? - 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.

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.

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.

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.

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.

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

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. 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.

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

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.

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".

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

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

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

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. '

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.

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