Ann. Geophys. Discuss.,
https://doi.org/10.5194/angeo-2020-29-AC2, 2020

Annales Geophysicae Discussions
Open Access

Interactive comment on "Helium in the Earth's foreshock: a global Vlasiator survey" *by* Markus Battarbee et al.

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Received and published: 4 August 2020

Response to referee comment 1 by Jeffrey Broll

The submitted work investigates the helium foreshock in simulation and MMS/HPCA observation. (Throughout, all mentioned helium is doubly ionized.)

We wish to thank Dr. Broll for his helpful assessment of our manuscript.

The main simulation of interest is 2D3V hybrid-Vlasov ions and massless-fluid electrons, with setup very similar to previously published work - low-β fast solar wind, 45° cone-angle, supercritical. This is as appropriate for the study as is feasible, but I must wonder what acts in place of electron pressure gradients for the cross-shock potential. I suspect/guess (cf. I think Gosling+ 82, "Evidence for

spec. refl. ions upstr. from the $Q_{||}$ bow shock", and Gosling + 89, "Ion refl. and downstream thermalization at the $Q_{||}$ bow shock", it's been a while) that some deviation from the specular reflection ovals should be expected if the main shock ramp jump is smaller or less sharp, in particular - this is possibly a shortcoming of the reviewer, but some demonstration of the cross-shock potential at an interesting point or two would have been nice to see...

We agree with the referee that a cross-shock potential field exists at astrophysical shocks, and that it has an effect on ion reflection, in particular at the quasi-perpendicular shock (it has been studied very little at the quasi-parallel shock front). The magnitude of the cross-shock potential, discerned from observations of quasi-perpendicular shock crossings, is estimated at 10% to 30% of the shock ram energy (Schwartz 1988). In the Ohm's law sense, the cross-shock electric field arises from a combination of the Hall current, electron pressure gradients, and drag due to the small population of gyrating ions at the shock front (Bale 2008). The majority ($\sim$ 60-70%) of the observed macro (i.e. ion)-scale electric field in the Normal Incidence frame is due to the Hall term $J \times B/ne$ (Eastwood 2007), which we already include in our model. Yang (2009) state that at the shock ramp, the shock-normal electric field is dominated by the ion Lorentz term and the Hall term, with the electron pressure term of negligible importance. Thus, hybrid models which neglect the electron pressure term do successfully model the majority of the cross-shock electric field, and only neglect one small portion of it.

Nevertheless, the role of cross-shock potentials on ion reflection is an interesting topic of study, and a study into different parametrizations (e.g. adiabatic or isothermal electrons) in a Vlasiator simulation is already underway (but not ready for submission yet). We will add discussion regarding this term to the manuscript, in particular to the section discussing specular reflection ovals.

The data used are appropriate for the study (although more could have been gotten out of burst HPCA data as in the cited HPCA papers).

We agree that burst HPCA data might be of great interest for many foreshock studies and thank the referee for the suggestion. We however fear that the low amount of helium ions detected by the instrument would likely require rebinning of the data in order to get statistically relevant results. Also, our survey of available burst mode intervals did not come across any foreshock crossings without an IMF discontinuity (a requirement which limited our options significantly).

However it's not clear if the figures couldn't be better labeled - Figures 3's caption is traveling backwards in time.

Could the referee please clarify this issue? We reviewed the figure and could not locate a problem, though of course would be happy to fix any errors.

Further, the suprathermal proton and helium population seem to go above HPCA's energy range on 30 Dec 2018 so perhaps some kind of either Maxwellian or κ fit to averaged foreshock E/Z slice would increase confidence that the suprathermals were being counted far enough up.

Thank you for pointing out this issue regarding suprathermal ion energy ranges. A kappa-fit might indeed give some extra information regarding this, but the fine time detail, the low amount of energy bins, and the extent of statistical noise leads us to not taking this approach. Further, as the color bar shows, the phase-space density range is highly logarithmic, and densities are strongly dominated by the lowest included suprathermal energy bin(s). We shall add discussion to this effect to the manuscript.

This issue and the different suprathermal definitions (which could be explained in the text for those not fluent in Vlasiator nuances) seem to make comparison harder than necessary.

We admit that the difference in suprathermal definitions is unfortunate. The data amounts generated by Vlasiator necessitate reduction of data stored to disk and full VDF information is available only at select locations (as shown later in the manuscript).

MMS HPCA data could probably be converted into a Vlasiator-style suprathermal densities but that would not allow energy-time-spectrogram plotting without accurate estimation of GSE-coordinate motion of the spacecraft, and would result in a presentation method unlike those used in other spacecraft studies. We performed visual evaluation of HPCA distribution functions using the available binning and concluded that the energy threshold method used here is not wholly unreasonable. We will, however, amend the text to draw attention to this discrepancy and that some particles which would be likely included in the Vlasiator-style suprathermal population now fall below the energy threshold as seen by the spectrogram energy bin just below the threshold (and also at energies $<$100eV).

The analysis lost me a bit right around the very large $T_{\parallel}/T_{\perp}$- could the resolution be hindering something like firehose growth that would bring that down? This could be an issue; checking Gary's microinstabilities book (7.2.1 and fig. 7.1) and assuming β sane, I'd guess that you could be missing some physics there. I think that given the nature of the work this is to be expected, but it could be mentioned for completion's sake.

Thank you for an interesting point. Within the foreshock, the parallel plasma beta rises from the solar wind value of 0.7 to values ranging from 1 to 10 or even somewhat beyond in the field-aligned beam portion at the edge of the foreshock. At the same time, temperature anisotropies $T_{\perp}/T_{\parallel}$ decrease to as little as 0.3. This parameter range indeed begins to be firehose instable. However, the beam particles streaming along field lines away from the shock are (along with the frozen-in magnetic field) also efficiently convected laterally due to ExB drifts and enter the deeper foreshock region, where plasma parameters are in the stable region. One may theorize that non-uniform driving (e.g. including statistical noise) with a good spatial resolution might drive firehose microinstabilities right at the foreshock edge, and we are happy to add discussion to this effect to the manuscript.

Having picked those nits I think this work should be published after some minor-

to-moderate complaints are addressed. In general there are some Vlasiator-specific nuances that are not common enough knowledge (at least for this reviewer) and possibly could be made so without detracting from the simulations section.

Thank you for the kind suggestions. We shall strive to improve the clarity of the manuscript in these respects.

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