Ann. Geophys. Discuss.,
https://doi.org/10.5194/angeo-2020-29-AC1, 2020

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

Markus Battarbee et al.

markus.battarbee@helsinki.fi

Received and published: 4 August 2020

We thank Dr. Sibeck for his helpful comments in improving our manuscript.

This paper presents a comprehensive analysis of novel results from the Vlasiator model for proton and helium acceleration in the Earth's foreshock. The paper is clear and (in general) well written, the conclusions are substantiated by discussion of simulation results and comparison with observations. For the community interested in shock physics, the paper will be very important. For the general space plasma physicist., the results will be fairly important. Some of the results reported in the paper include (1) different edge locations for the proton and helium foreshock, (2) the manner in which the ratio of helium to proton

density varies with location in the foreshock, (3) the nature of proton and helium distribution functions as a function of location, (4) the manner by which helium is heated in the foreshock, (5) the effects of the IMF orientation on foreshock boundary structure, and (6) the nature of waves/turbulence in and around the foreshock. I only have a few comments/questions.

Thank you for the helpful assessment of our key points and the significance of our study.

Lines 50-61. I did not find this review of past work as clear as it could be. I have no objection to each sentence but I think it can be presented more carefully. The authors could systematically go through each region of space, or each type of distribution function, showing they are covering all possibilities. A table noting regions, types of distribution functions, and composition ratios would help. Could the authors just tell what is seen first, and then give explanations? Or could they state expectations and then tell what past work has seen? It would be cleaner than the mixture of observations and interpretations. Having a table would also be something useful that the authors could refer back to when summarizing their work towards the end of the paper, especially if they can check off each observation and state that their model predicted it.

Thank you for the suggestion, a table might indeed help. We also noted the difficulty of comparing previously published He^{2+} observations from different parts of the foreshock. A table was previously not included in order to not veer into review territory, but we will restructure this section and, if at all feasible, introduce a table and reference it in latter parts of the manuscript.

Change:
1. the suprathermal He^{2+} fraction $\rightarrow$ the ratio of He^{2+} to H densities with suprathermal energies

Thank you, that is a good formulation.

2. High energy field aligned beams near the foreshock edges show significant He/H ratios, whereas lower energy beams deeper within the foreshock exhibit intermediate proton distributions and lower He/H ratios

Agreed.

3. Still deeper (?) within the quasi-parallel shock, He distributions are nongyrotropic partial rings whereas H distributions are ring beams and density ratios return to solar wind levels.

Agreed.

4. Diffuse ions are found WHERE?. The ratio of suprathermal He to suprathermal H ion densities is similar to that for the solar wind composition.

Indeed, the typical location of diffuse ion populations (throughout the deep foreshock) should be mentioned.

Lines 100-102. The authors chose to simulate very rare solar wind conditions. There were only 85 hours of solar wind velocity between 700 and 800 km/s and densities less than 3 cm^{-3} during the 17250 hours in the two-year period of 2012 and 2013 (0.5% of all conditions). Could the authors please add a paragraph to the conclusion stating what they expect the results for more typical solar wind conditions to be?

We are happy to add discussion regarding the simulation parameters. Despite the large speed and low density, the plasma beta (0.7) and Alfvenic Mach number (7) of the simulation are descriptive of quite typical solar wind conditions, and thus we do not expect these results to be atypical. A fast solar wind speed ensures efficient initialization of our simulation, which is computationally expensive, and thus allows for a longer simulated extent of well-formed foreshock dynamics.

Line 150 says the simulation finds $N_{\mathbf{alpha}}/N_{\mathbf{proton}} > 2$ deep in the foreshock. Is that consistent with the summary above? What is the explanation for it? If the

paper tells this somewhere and I have missed it, please strengthen the discussion to make it clear. I would have guessed that deep within the foreshock is a region of diffuse ions and I have read above that density ratios for diffuse ions are similar to those in the solar wind, not twice as great.

We believe this to be a feature of helium existing in more distinct structures (partial rings and gyrating clumps) than protons in the deep foreshock (as expected based on observations), and the fact that the diffuse population is removed from our simulation domain. This is mentioned section 4, but we shall add an initial note of this feature at this point.

Line 181-182. When the authors present two case studies of observations they should tell where the spacecraft were located and present a plot showing the locations of the magnetopause and bow shock, the IMF lines, and the locations of the spacecraft. This will help in the comparisons and in the reader's comprehension.

This is indeed a good suggestion, we shall add the plots of spacecraft locations and environmental conditions to Figures 3 and 4. Positions were already listed in section 3.1 along with verbal descriptions of the IMF, but sometimes a picture does say more than a thousand words.

Line 182. Actually it is probably the foreshock moving past the spacecraft and not vice-versa and the authors should make this clear.

We shall amend the wording to clarify the possibilities here. We did attempt to find crossings where the foreshock edge movement would be as slow as possible (no IMF discontinuities), so that it would be at least partially spacecraft movement instead of the edge sweeping over the spacecraft, but likely both effects are at play.

In general (1). Where are the spontaneous hot flow anomalies reported and simulated to occur within the quasi-parallel foreshock? [Zhang et al., JGR, 118, 3357,

2013; Omidi et al; JGR, 119, 9823, 2014]

SHFAs have been previously investigated within Vlasiator in Blanco-Cano et al. (2018) and a statistical study is under preparation (presented at EGU 2020, Tarvus et al.). A full study of helium dynamics in response to SHFAs would warrant a whole study of its own. Within the scope of this current study, we can state the following. As SHFA are identified via a flow deflection and an abundance of hot ions, we would expect to see SHFA only where there are plenty of beam-like, gyrating or intermediate suprathermal ions (diffuse ions will not significantly influence flow deflection). Our simulation shows that SHFAs are formed very close to the bow shock and deep within the foreshock, and the ratio of He^{2+} to H densities with suprathermal energies is greater in this region than in the solar wind, likely due to the abundant diffuse proton population having been excluded. However, visual inspection of a number of SHFA-flagged regions shows that these do not show the suprathermal ion ratio rising much beyond 2. This is in agreement with an abundance of energetic non-diffuse protons within SHFA. Improvements to the numerical method or better analysis approaches may indeed merit a further study of alpha-particles within SHFAs, but we do not wish to investigate it in any more detail in this study.

In general (2) Do the authors find foreshock compressional boundaries with density and magnetic field strength enhancements like those reported by Omidi et al. [JGR, 118,823, 2013]? If so, where do these boundaries lie compared to those for the patterns for waves and suprathermal composition ratios?

The presence of foreshock compressional boundaries and their dependence on shock Mach numbers in Vlasiator simulations were investigated in Turc et al. (2018). As we do not expect them to be significant from an alpha-particle point of view, we have not discussed them in the manuscript, but can comment the following: Brief visual analysis of foreshock wave compressionality in the presented simulation indicates waves are compressional in the regions where Figure 1a shows well-structured (red-and-blue-striped) wave fronts. The positions at the bow shock where these two regions connect

may in fact be associated with the theorized "optimal" θ_{Bn}-connection, but the complicated wave interactions further in the flanks of the foreshock make this hard to discern. We would also like to point out that especially in the region $X < 0$ at the flank we see strong compressional features aligned with the IMF, akin to the canyons and ridges which were seen in Blanco-Cano et al. (2018) and which were associated with caviton and SHFA-type structures. These IMF-aligned ridges and canyons would, if advecting past a spacecraft, appear like single structures such as cavitons and SHFAs.

I caught a few typos/corrections.
1. Author list. Stephen
2. Line 15. The → to
3. Wilson III → Wilson
4. Line 36 dynamical → dynamic

Thank you! These will be corrected.

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