

Ann. Geophys. Discuss., referee comment RC2
<https://doi.org/10.5194/angeo-2020-87-RC2>, 2021
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Comment on [angeo-2020-87](#)

Anonymous Referee #2

Referee comment on "Foreshock cavitons and spontaneous hot flow anomalies: A statistical study with a global hybrid-Vlasov simulation" by Vertti Tarvus et al., Ann. Geophys. Discuss., <https://doi.org/10.5194/angeo-2020-87-RC2>, 2021

This manuscript presents statistical results of transient features present in the foreshock of a global hybrid Vlasov simulation, namely the so-called cavitons and Spontaneous Hot Flow Anomalies (SHFAs). Results on formation, propagation, evolution, and properties are presented. The work is very thorough, however, some of the results require further work to be more compelling. If addressed I believe that this work would be suitable for publication.

Major comments

Throughout there is very little explicit comparison of the properties of the transients compared to the foreshock in general, let alone the ambient foreshock at the transient's location. Instead mostly only values in the pristine solar wind are used for comparison. However, understanding how the structures differ from their surroundings is of vital importance and needs to be incorporated into the work throughout. This affects numerous aspects of the work, including:

- Are the choices of properties and thresholds for detection of the transients suitable? How does a 20% decrease in density compare to the variability in density associated with the foreshock ULF wave field? Is plasma beta a sensible parameter to use to distinguish between cavitons and SHFAs (I would have thought a temperature criterion would have been more appropriate) and how does a value of 10 compare to the typical foreshock and its variability?
- In Figure 2, how do the density of suprathermals and temperatures of cavitons and SHFAs compare to typical foreshock conditions? Are the velocities in these structures significantly different from the ambient?
- In Figure 3, are the correlations presented simply extensions of the overall foreshock or do they constitute distinct populations?

These are important considerations in fully understanding the context of the results presented.

I also have concerns over the results surrounding the suprathermal ions. The method employed of distinguishing between core and suprathermals uses the velocity and temperature of the pristine solar wind. This seems unsuitable for transients associated with flow anomalies, as the authors concede on line 200, and thus many of the results are likely mischaracterising the solar wind and suprathermal ions in these structures. I would suggest the authors reprocess the data separating out regions in phase space using a distance condition in velocity space (based on the temperature in the pristine solar wind) either from the bulk or peak phase space density.

Related to the above, many conjectures around how the solar wind beam vs. the suprathermals are affecting the moments of the distribution are made, however, no velocity distributions are presented within the manuscript. It is known that the distributions within foreshock transients can evolve from multicomponent to single component plasmas, whereas the authors posit only the former.

Finally, the results with relation to the "nose angle" (which may be better described in the manuscript as meridional angle or solar zenith angle throughout) need to be understood in terms of the θ_{Bn} angle that the transient is magnetically connected to, since this largely controls the physics of the foreshock. This may aid in the interpretation of the results.

Minor and specific points

Lines 20-21: "before it is deflected by the magnetopause" This could do with rewording, since the bow shock also deflects the solar wind and the pressure gradients present throughout the magnetosheath (between bow shock and magnetopause) act to deflect the plasma around the boundary.

Line 23: "far back into the upstream." This is not true for the entire region of the shock connected to the IMF, as the sentence suggests, only in the quasi-parallel case. Please reword this sentence, for example, removing the word "far".

Line 59: "SHFAs evolve" I would say they are "thought to evolve" since this point requires further evidence in general and the results of the manuscript show it be the case only for some SHFAs.

Line 188: "SHFAs tend to be more depleted than cavitons" This could simply be an effect of the plasma beta condition so needs further comment.

Figure 4: PDFs would be more helpful to readers than CDFs to see the regions where the transients actually form, rather than cumulatively from the bow shock up to some region where a certain proportion form. Some of the cumulative numbers can remain in the text, however.

Table 1: Minima and maxima of probability distributions are not robust statistics, the 25th and 75th percentile would be more appropriate columns to use. This would also remove potential confusion between the minimum and maximum value for each a particular transient used in the left column, which is appropriate.

Figure 4: The label states these are counts, but they are proportions