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Interactive comment

Interactive comment on "Resolution dependence of magnetosheath waves in global hybrid-Vlasov simulations" *by* Maxime Dubart et al.

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We thank the reviewer for their careful examination of our manuscript and their constructive comments. Please find below our response to your concerns.

"Major comments: Plasma β is one very important parameter to two instabilities. If different solar wind speed, solar wind temperature, and IMF field strength are used in the simulation, the plasma β in the magnetosheath will be very different. So my concern is that the concluded spatial resolution very likely depends on the magnetosheath plasma β or solar wind parameters. Therefore, it is necessary to justify that the conclusion is true for all the possible solar wind parameters or how the

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conclusion depends on the solar wind parameters. Otherwise, the importance of this work to future simulations will be very limited."

This is an excellent point we will add to the discussion. While running an entire new set of simulations with different solar wind parameters would be too expensive, we plotted the growth rate of the proton cyclotron instability with three different β (the reviewer can find the figure attached to this document). An increased plasma β gives a higher value of the maximum growth rate γ_{max}, but does not change significantly the value of the corresponding wave vector k. Our conclusions remain that the low resolution run does not resolve a large enough spectrum of wave vectors to allow the proton cyclotron instability to develop. A higher beta value would slightly improved runs with resolution between 300 and 600 km. We will add this to the discussion in the revised manuscript.

"Under this certain solar wind condition, the authors conclude that $\Delta r \approx 440$ km = 0.6 di would be adequate. However, I am not convinced by this number which is based on the growth rate profile. For example, where the growth rate is calculated may not be the source region. So I wonder whether the authors can run several more cases, e.g.,with Δr around 400 km, to show that the results are indeed close enough to the case with $\Delta r = 300$ km"

We thank the reviewer for pointing this out. We agree that this conclusion, purely made based on the growth rate profile, is probably too specific, since many other parameters, such as the source region of the wave in the global simulation, may affect it. As mentioned earlier, running additional Vlasiator simulations is difficult because of their large computational cost (a few million CPU hours for a 400 km run similar as those presented in this study). In the revised manuscript, we will reformulate our conclusions as follows:

The currently available runs allow us to conclude that the wave modes of interest here are properly resolved at a resolution of 300 km = 1.32 di. The growth rate profiles suggest that larger cell sizes, between 300 and 600 km, may still be sufficient to resolve those wave modes in the simulations. This hypothesis could be tested in running additional global simulations with a range of spatial resolutions. Such a parametric study is however not currently achievable because of the large computational costs of global Vlasiator runs at these relatively high resolutions.

Incidentally, we have realised that there was an error with the value of the resolution in term of ion skin depth in the manuscript. The values should be $\Delta r = 300$ **km** $= 1.32\ d_i$**,** $\Delta r = 600$ **km** $= 2.64\ d_i$**,** $\Delta r = 900$ **km** $= 3.96\ d_i$ **and** $\Delta r = 400$ **km** $= 1.76\ d_i$**, as the ion skin depth in the solar wind is 228 km. This does not however affect our conclusions. We will correct this in the revised manuscript.**

"Minor comments: Please rephrase “magnetosheath waves” in the abstract and conclusion as there are not just mirror mode and AIC waves in the magnetosheath."

We will rephrase this formulation in the revised manuscript.

"Why the position of quasi-perpendicular bow shock in Figure 1c is more outward than Figures 1a and 1b?"

We thank the reviewer for raising this discussion. As can be seen on Fig. 7, due to the AIC waves not being resolved, the temperature anisotropy is greatly increased in the 900 km run. The lack of mechanism to reduce the perpendicular

temperature leads to an increased pressure behind the shock, "pushing" it further away. We will add this to the discussion in the revised manuscript as an additional argument toward the importance of setting the resolution to 300 km.

"In Figure 2f, there are signals along two blue lines. What are they?"

We believe it to be due to some numerical artefacts. The low resolution and the fact that these features appear in the perpendicular direction make us doubt that this would be related to Alfven waves. We currently apply a Hanning window filtering in both time and spatial dimension. We are planning to add other filters and see if these features disappear.

"In Figure 5, it would be better if there are similar panels of other two runs for comparison."

We will add them in the revised manuscript.

"There are some typos such as line 221, 'were the data were taken' → 'where the datawere taken' and line 225, 'more efficient tp' → 'more efficient to'".

We thank the reviewer for picking up these typos, we will correct them in the revised manuscript.

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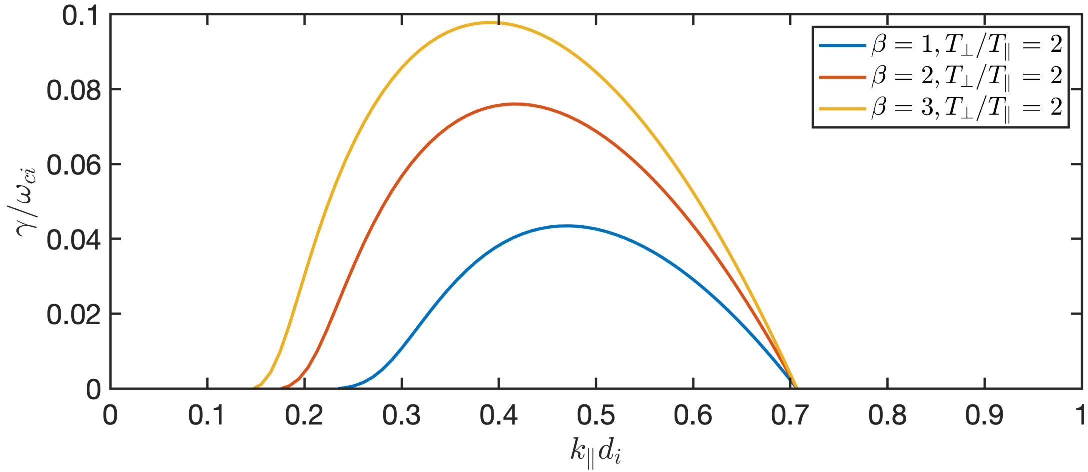

Fig. 1. Growth rate of the proton cyclotron instability for three different beta plasma, computed with HYDROS

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