Response to Reviewer #1

We would like to thank you for your critical reading of our manuscript and your constructive suggestions and comments.

Authors: As outlined in the initial paragraph and reiterated throughout your esteemed report, you have suggested removing the mean particle theory model from the manuscript. However, in our investigation, this model yields reliable and valid results, as the diffusion coefficient utilized within it is both suitable and consistent with the coefficient employed in the Monte Carlo model. This alignment indicates that the ion heating process predominates over other forces, as highlighted in various sections of the manuscript. Specifically, the wave-particle interaction effect is shown to be dominant over external forces such as gravity, polarization electric field, and mirror force in certain regions, while it competes with these forces in others. It is acknowledged that, without the correct diffusion coefficient, the mean particle model would indeed become overly simplistic and not advisable for use. Therefore, we advocate for retaining both the model and its associated results within the manuscript.

Detailed comments:

Line 18: …"use velocity and altitude diffusion coefficients…" My understanding is that the authors mean ... use velocity and altitude **dependent** diffusion coefficients. Same in lines 20 and 23.

*Authors: Yes, we mean that "velocity and altitude **dependent** diffusion coefficients", corrected.*

Line 70: remove "…because there are several events at lower altitude.", the reason that the locally observed fields were unlikely to be the source of heating was that they were of too low amplitude. In the subsequent paper (Waara et al. 2011) it was essentially found that this case study was an exception.

Authors: thank you, we removed the sentence.

Line 74: I would add: Waara et al. (2011, 2012) provided a statistical study of ion heating and related wave activity. They provided average values of diffusion coefficients…

Authors: thank you, it has been added.

Line 78: "They expected the relation…" this sentence is a bit unclear. Looking up the reference I suppose the authors mean that "The electric to magnetic field spectral density ratios were found to be close to what is expected for Alfvén waves."

Authors: thank you, you are right, we have included your correction.

Line 88: As suggested elsewhere, I would remove the comparison with the mean particle theory.

Authors, please, as we mentioned before, we prefer to keep it.

Equation (8): Spell out explicitly what this equation means, I.e. to my understanding the saturation of the diffusion coefficient due to the finite wave length of the waves. I think it would also be prudent to cite Bouhram et al. here again, as they were first with introducing the finite wavelength effect.

Authors: we have added the following: "equation (8) indicates the diffusion coefficient is dependent on altitude and velocity. However, as ions are heated and move to higher altitudes, their gyroradius may approach the perpendicular wavelength of the electromagnetic turbulence, and when the ratio ($k_\perp v_\perp/\Omega_i$) exceeds 1, the heating rate becomes self-limited. Bouhram et al. (2004) derived an alternative form for the altitude and velocity dependent diffusion coefficient and interpreted their results in terms of finite wavelength effects."

Line 164: Once again, I do not think the comparison with an older very simple model adds anything to the paper.

Authors: again, we prefer to keep it.

Line 218 section 3.2

This is the interesting part of the paper. Unfortunately it is also the least complete. It is very unclear what the observational data they Cooper with is, and how this study is related to what was already reported in Bargouthi et al (2016). This must be made much clearer. I started reading Barghouti et al. (2016) but it is not really my job to sort this out in a clear fashion. So what I have found is that Nilsson et al. (2013) divided the diffusion coefficients after region. This was extended in Barghouthi et al. (2016) to cover a larger altitude interval as well as different diffusion coefficient combinations, including the maximum and minimum values used in the present paper.

Thus the observations shown are as I understand from Nilsson et al. (2013). The minimum and maximum values around that are from Barghouthi et al. (2016). This should be made clear.

Authors: we have mentioned that observations are obtained from Nilsson et al (2013) and Barghouthi et al (2016). Also, we present figure 3 and provide the comparison between the results of the models and the corresponding observations in certain mentioned regions.

Line 233, Figure 3: Minimum and maximum values appear to be interchanged. Highest temperatures are seen for the dotted line, this must correspond to the maximum case and vice versa.

Authors: sorry for this mistake, we have interchanged them.

Note that Barghouthi et al. (2016) is missing from the reference list.

Authors: No, it is in the reference list, line 309.