Response to Reviewer #2

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

The article by Barghouthi and Halaika focuses on the physics of ion outflow at the magnetosphere of the Earth and discusses different approaches for modeling this process. By comparing the approaches between them, as well as against observations, the authors conclude on the limitations of each simulation method and the importance of specific parameters which may control the validity and applicability of each method. Generally, the paper is useful and contains original results. Even though there are language and presentation issues, the approach and study concept are straightforward to understand. It is also a potentially useful paper for anyone working on the topic of ion outflow.

Authors: Thankyou

On the other hand, the presentation quality of the study is quite low. The authors take too many things for granted (which only experts on outflow may understand), basic introductory materials are missing, e.g. about the outflow theory, what physical processes are involved and how these map to the different part of the equations presented etc. While in some cases references are provided, these are not enough, and I will give more examples below. Furthermore, the study lacks a clear motivation statement. E.g. what is the main reason that this comparison is done? Has this never been done before, is it driven by a necessity to demonstrate the performance and applicability of the Barghouthi model, or is it still unclear which factors (equation terms) control the outflow results? Finally, there are many language issues, e.g. long sentences, sentence parts without articles or written as statements in a conference presentation. I only give selected examples of such language issues below, it is impossible to keep track of them all. I suggest a more careful proofreading.

Authors: we have published numerous articles, as listed in the references, and many other specialists have contributed extensively to this field of research. Topics such as introductory material, ion heating, ion outflow, Mean Particle Theory, Monte Carlo simulations, and the Barghouthi model have been widely discussed in various publications. In line with our approach, we avoid repeating previously published information and instead provide the key concepts while referring to the relevant references.

When it comes to the scientific results (mostly Section 3), the main problem I see there is that comparsion between models and observations is under discussed. Despite multiple claims that MC simulations and data agree well, I do spot several key disagreements which need discussion. These missagreements don't falsify the study, but understanding them will also be even more revealing for the theoretical models and their limitations. Furthermore, claims of good/excellent agreements between models and data are not bases on quantitative claims.

Authors: sometimes, a quantitative comparison isn't possible, so we focus on making qualitative comparisons instead.

Below I provide selected comments on parts of the paper that justify my summary evaluation above. Overall, I see that all issues are resolvable and the paper can certainly be published after these are resolved. Most comments are minor but adding many minor comments together sums up to a moderate/major revision.

Detailed/specific comments

1) There are many minor or major language issues, e.g. just in the abstract:

Abstract, line 10: Earth magnetosphere -->Earth's magnetosphere
Authors: corrected
Abstract, line 11: We present altitude profiles for mean perpendicular◊ add "the" before "mean"
Authors: done
Abstract, line 12: using Barghouthi model --> using the Barghouthi model
Authors: done
Abstract, lines 15-16: in which parameter an agreement is obtained?
Authors: we add, (mean perpendicular energy $W_{\perp}$, mean parallel energy $W_{\parallel}$, and mean total energy W_{total})
Abstract lines 16-17: What kind of wave particle interaction is referred to here? What external forces refers to? A lot of terminology is used in the abstract, but in a kind of vague way
Authors: we include, electromagnetic waves, external forces (gravity, polarization electric field, and mirror force)
Abstract, line 17: "produce high energies": I assume high energy particles? Can you indicate numerically what high energy means? what particles are we talking about?
Authors: high energy O^+ and H^+ particles, see figures 1, and 2.
Abstract, line 18: "not reasonable": The way this expression is placed in the sentence is not correct and it's unclear what is not reasonable. I suggest to break the long sentence into smaller ones.

Authors: not reasonable, much higher than observations (Barghouthi, 2008), we break the sentence into sentences.

Abstract comments: Generally, it is not clear what outflow parameters are compared, or what excellent agreement means. There is also no coherence in the text, e.g. "we can claim that the wavelength of the electromagnetic wave existed in those regions": You do not introduce anything about an electromagnetic wave (what is this wave?) . There is no statement of an open question in the beginning sentences of the abstract, it is unclear in the end what exactly is the goal of the study. Within the abstract various terms and concepts are introduced or mentioned which add lots of confusion.

Authors: we cannot include everything in the abstract, we have mentioned, electromagnetic waves and we provide its wavelength that produce good agreement with observations, we have mentioned the name of the regions, particles, parameters,….

Main article:

Line 35: of the ion --> of the ions

Authors: corrected

Line 37-38: Sentence needs rewrite, maybe break it in several smaller sentences. Also its unclear how Monte-Carlo and diffusion theory are combined, maybe add few words? E.g. "Monte-Carlo simulations are performed using test particles and predefined electromagnetic fields. The way diffusion theory is combined with Monte-Carlo simulations is…". Maybe it is clear for experts in the field but for other readers, numerous unexplained terms and concepts are introduced without background. This will also help understanding text in follow-up paragraphs.

Authors: We are addressing this to the experts in the field and have included the relevant references along with their key findings. Adding more details would only lengthen the manuscript, and we risk being criticized for repeating information already covered in previous publications.

Introduction: general comment is that by the end of the introduction, no open question is posed. E.g. it is clear that models and theory will be compared, but what is the motivation behind that? Is there still some doubt on which models are best to use? To find the applicability and limitations of each approach? To explore aspects in data that remain unexplained and may require combinations of model? What are the open questions?

Also I need to clarify that since I am not an expert on the topic of the outflow, I would have appreciated some more introductory comments on the topic, that could help readability e.g. in sections like 2.1. For instance, 1-2 sentences on what the polarization electric field is, what are the external forces, what do we refer to when we talk about wave-particle interactions. External forces, for instance, are defined for the first time in line 204, while this could be done in the introduction or section 2.1. WPI is a very broad term. What is the driver/physics of wave activity, what is the topology of the waves (e.g. present at low altitudes, high altitudes?). Without this information, readability of the manuscript would be enhanced and the reach to non- experts can be increased. What is the physics behind the diffusion coefficient (D)? For models not considering WPI, what does D represent?

Authors: reviewer # 1 mentioned that the manuscript is too long, we cannot include all details, we provide main idea and the major scientific contributions related to the topic of the manuscript.

Lines 157-159: Break this long sentence into several ones, to improve language and readability.

Authors: done

Section 2.2: Similar to a comment above, can you briefly describe how the Monte-Carlo implementation of your model works? Is this a test-particle approach?

Authors: yes, it is a test particle approach, it has been described in different publications, Barghouthi 1997, 2008, Barghouthi et al 1998, 2011, 2016, 2016, …

Figures 1-4: Can you add a legend on the Figure? Lines are explained in the caption, but it may be useful to have this figure with a legend in case its used in a review article, presentation etc.

Authors: We prefer not to do that, as it would make the figures too crowded.

Line 207: Check language – the first sentence after "i.e." is written like it is part of a bulleted list in a presentation. Use simpler writing with smaller sentences. E.g. "This means the velocity diffusion, D(r), coefficient is altitude dependent."

Authors: yes, corrected

Lines 213-217: Break this long sentence into several ones, to improve language and readability.

Authors: done

Section 2: I believe it would be useful to have some parametric demonstration that shows at which parameter (or set of parameters) the two approaches (theory and MC simulation) start to deviate. Obviously, the two theories agree well in the auroral region and not in the polar wind region. In line 209-211 it is explained that the poor performance of the mean theory in the polar wind region is due to the effect of the external forces, included in the MC approach only. However external forces are included also in the case of the auroral region. This means that there should be some relation between external forces and the quantification of wave particle interaction, (e.g. a ratio?), across which mean theory becomes irrelevant. Woud it be possible to discuss the results in such a way?

Authors: We have highlighted in several sections that the wave-particle interaction is especially strong in the auroral region, where it outweighs the external forces. In contrast, in the polar wind region, the wave-particle interaction is weaker, allowing external forces to have a more pronounced impact.

Section 3: This section needs more discussion. There are many claims of excellent or good agreement, but there is no quantitative way to define what that means, besides a non-objective visual comparison of curves in Figures 3 and 4, especially when it comes down to comparison with data. E.g. in the top panels of Fig 3, at altitudes of 6-8 km, the particle energies are in the range of observations, however the shape of the altitude energy profile E(h) is different than what seen in observations. The slope of increasing energy is much steeper in the simulation/theory curves that in the data. It is also unclear why theoretical curves stop below 10 km. In the bottom panels of Fig. 3 the E(h) profiles are more similar, so I would say that the bottom profiles are closer to be "in excellent agreement" than the top ones. The deviations in the top profiles need discussion.

Same applies for the left panel of Figure 4 (steep theory profiles compared to data), whereas the right profiles show better agreement with observations.

Authors: in the figures 3, and 4, every panel is in different region of earth magnetosphere.