Authors' response to referee comments on "Atmospheric odd nitrogen response to electron forcing from a 6D magnetospheric hybrid-kinetic simulation" by Häkkilä et al.

The authors would like to thank all the Referees for the review process. We are grateful for the Referees' insightful and constructive comments and we appreciate the time taken to review the revised manuscript. We also appreciate the Referees' acknowledgements of the improvements made to the manuscript in the revision.

Please find below our answers (in blue) to the comments (in black).

Response to the comments of Referee #1

I'd like to thank the authors for responding constructively to the comments from the other reviewers and myself. I think the manuscript has improved considerably through this and I don't have major concerns regarding the publication anymore.

I have to admit, that I'm only halfway satisfied with the replies to my original first major concern, the vagueness of the motivation. I understand that the manuscript presents a novel approach. But when the authors write about "considerable uncertainties in our knowledge of the polar MLTI". I would like to see these uncertainties to be spelled out and argued why this novel approach has the potential to reduce these uncertainties. There is the helpful reference to Sarris et al. (2023) on questions concerning plasma-neutral interactions, and in the paragraphs following this statement it is becoming clear that this study has a focus on NOx production from auroral electrons which are unsatisfactorily represented in existing approaches. I also appreciate the argument that "solar wind parameters can be observed earlier than e.g. the geomagnetic activity" provided newly in the Discussion section. However, I still think the authors miss the chance of providing a clear statement on the potential of their new approach to answer existing questions. Which questions would that be?

Thank you for pointing out that the motivation of the study still remained unclear. As the Reviewer states, we are focused on the production of NOx from auroral electron precipitation, and how this precipitation is presented in atmospheric modelling. In addition to presenting a new method of deriving the auroral electron precipitation, we focus on whether this more accurate characterisation of auroral electron forcing could solve the inadequate production of NOx in current atmospheric models. We now state this motivation more explicitly in the Introduction of the manuscript, and have modified the Discussion section accordingly.

Response to the comments of Referee #3

The manuscript presents a novel approach to combine magnetospheric modelling with whole-atmosphere modelling to investigate the auroral production of odd nitrogen. The magnetospheric model drives the precipitating electrons, and the whole-atmosphere model WACCM is used for the production of the odd nitrogen species, NO in this case.

The authors have addressed all my concerns and clarified the respective points which has improved the manuscript considerably. Thus it is a worthy contribution to Ann. Geophys., I have only a few minor points left that should be addressed before publication.

General comments

1. I would still like a short note about the single WACCM model run used in the analysis. Even though the authors refer to the "SD" part, the interesting part is where the model is free-running. It is not clear or even obvious that multiple runs will produce identical results as the authors suggest.

Thank you for the comment. We have now included a brief discussion on the use of single WACCM runs in this study in the Discussion section of the manuscript.

2. Appendix B4: To my understanding, the equations given by the authors describe the integrated particle flux [cm-2 s-1 sr-1], not the integrated energy flux [eV cm-2 s-1 sr-1]. I assume both can be used for the normalization here, but I suggest to correct the terminology.

Thank you for pointing this out. We did use the integrated energy fluxes for the described purpose, and indeed forgot the energy term in the integral expressions. We have corrected the equations accordingly.

Specific comments

L. 25: Maybe "complex interactions" instead of "dynamics"?

We have made this change in the revised manuscript.

L. 114–115: A short note about how typical these solar wind conditions are would be helpful.

We have added a brief mention to such conditions being found during the fastest of the solar wind high-speed streams and how frequently such events typically occur.

L. 124–127: This description is a bit long and convoluted with repeated expressions. I suggest to rewrite to be a bit more concise.

We have rephrased these sentences in a more condensed and hopefully clearer way.

L. 193: How many model levels are used for NOEM input?

NOEM is used to set the NO concentration at the top-most model level of WACCM, i.e. at one level.

L. 302–310: The authors list only the numbers of the SH, please add the NH relative and absolute changes too. That would strengthen the authors' argument later in the manuscript, that in the NH the background levels are higher, so that the relative changes are smaller.

We have now included more NH NO_x impact numbers, both relative and absolute changes, in Section 3.2 when discussing the magnitude of the impact. For some specific examples of peak impacts we still only include the SH values.

L. 364–370: The authors list only the numbers of the SH, please add the NH relative and absolute changes too.

We have now included more NH ozone impact numbers, both relative and absolute changes, in Section 3.2 when discussing the magnitude of the impact.