

Interactive comment on “Reactive nitrogen (NO_y) and ozone responses to energetic electron precipitation during Southern Hemisphere winter” by Pavle Arsenovic et al.

Anonymous Referee #3

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A straightforward paper which is timely for the discussion of the topic in the community.

I have only few concerns being major, and otherwise recommend the paper for publication if my concerns have been clarified.

1. The authors use a flux boundary condition for including the NO_y produced in the upper mesosphere and lower thermosphere. They criticize the alternative method of prescribing mixing ratios as possibly inconsistent. I do not agree with this general statement. In my opinion, this depends on the dynamical boundary conditions of the model. At the ground you may introduce an influx of some species essentially via a turbulent flux, but in the mesosphere the influx of NO_y

- is by an advective term. Setting the influx from the parameterization, you may have an increase in concentration even when having upwelling which is physically impossible.
2. The chosen periods (2005 for high and 2006 - 2010 for low activity) should be explained. 2003 (at least 50% higher Ap) and 2008-2009 could have been a better choice. As the Halloween storm occurs in late 2003, this event should not interfere for your study. In addition MIPAS' coverage in 2005 is not as good as in other years. As MLS data are not available for 2003, MIPAS ozone data could be used.
 3. The pronounced mesospheric minimum of the NO_y concentration in the REF runs during SH mid winter needs an explanation. The mentioned SPEs should show up in the middle mesosphere in the whole SH winter. Perhaps you could provide a figure with a higher time resolution as you have done for ozone.
 4. Why does the SP event of June 2005 does not show up in Fig 3?
 5. Please provide an additional figure with an mesospheric transport tracer as for example CO (active - ref years), for comparison to exclude or evaluate dynamical effects.
 6. Yet technically, but nevertheless important for the understanding of possible effects in the lower stratosphere, the colors in Fig 2 do not really allow to decide where small values are significant. Please use a different color table.
 7. The enhancement of NO_y by MEE (Fig4) outside the polar vortex needs some discussion of the photochemical lifetime expected outside the polar vortex, in sunlight.
 8. A main result of the paper is the impact of MEE, essentially via HO_x, on ozone which the authors estimate to be of the same order as NO_y produced by LEE.

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This is important for the understanding of EPP effects, but this result needs in my opinion more substantiation. The fact that MEE in 2005 mostly come with SPEs (Fig. 3) reminds me that there were some discussions about crosstalks of the detectors for the different particles especially in the MEE energy range. For example, Anderson et al. 2012, exclude electron fluxes during SPEs in their analysis because of possible contamination. Please try to extend your analysis when excluding SPE periods.

The authors should also improve the grammar of their paper with a special emphasis on the use of articles.

Minor comments are marked in the commented pdf attached.

Please also note the supplement to this comment:

<https://www.atmos-chem-phys-discuss.net/acp-2018-1123/acp-2018-1123-RC2-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-1123>, 2018.

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