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Atmospheric Chemistry and Physics Discussions

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

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We thank the reviewer for their constructive suggestions that contributed to improving the quality of this paper. We have carefully analyzed and addressed all comments below.

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 NOy produced in the upper mesosphere and lower thermosphere. They criticize the alternative method of prescribing mixing ratios as possibly

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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 NOy 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.

We agree that the flux depend on dynamical boundary conditions of the model. However, when the downwelling in the model is weak, the NO that entered through the model top will be constrained to the top model level(s) and it would not reach lower altitudes where it can impact HOx and ozone. In case of prescribing mixing ratios on model levels, this is not the case. By prescribing the mixing ratios, in case when the model downwelling is weak, NOy would still be prescribed on the lower levels where it can impact HOx and ozone, while in reality it wouldn't.

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.

We chose 2005 for high and 2006 - 2010 for low geomagnetic activity because we want to include two satellite datasets for our study and ozone measurements of MLS instrument start at year 2005. Moreover, it is true that MIPAS' coverage is not as good as in the other years, but the lack of observations appears only in September. This paper focuses on JJA period which is well covered with MIPAS in year 2005 and choosing this period allows us to use both satellite instruments (MIPAS and MLS) in the same manner. To clarify we added in the first paragraph of section 3.1: "Even though year 2003 on average has higher Ap, here we choose year 2005 as the geomagnetically active year. This allows us to compare modeled NOy and ozone using two different satellite datasets MIPAS and MLS (which is available only since 2005). MIPAS data

are unavailable from September 2005 to the end of the year, but our main period of interest is JJA, which is well covered by the observations."

3. The pronounced mesospheric minimum of the NOy 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.

SP events should be visible as NOy enhancements, but unfortunately due to the limited storage space, we don't have a higher temporal resolution for NOy as we do for ozone.

4. Why does the SP event of June 2005 does not show up in Fig 3?

SP of June 2005 was one of the minor events (M4; ftp://ftp.swpc.noaa.gov/pub/indices/SPE.txt). Although even smaller event is seen on Figure 3 (M3, end of July), it is possible that the June event was lost due to the dynamic variability.

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.

Please see the attached plot (Figure S1). Although there is up to 10% less CO in the year 2005 compared to the 2006-2010, the difference is not statistically significant. Our study focuses on chemical effects of EPP- Follow-up study will focus with temperature and dynamics caused by EPP. We added in the following sentence in the Conclusions, second paragraph: "Future work is required to address the roles of indirect changes in temperature and dynamics in the EPP-induced stratospheric ozone variation."

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.

We changed the colormap of the Figure 2 and reduced the number of plotting levels to make the plot clearer. We did the same change for NOy in Figure 4 to keep the plotting

style consistent.

7. The enhancement of NOy by MEE (Fig4) outside the polar vortex needs some discussion of the photochemical lifetime expected outside the polar vortex, in sunlight.

We added the following sentence in the second paragraph in the section 3.3: “Around 0.01 hPa, EPP produced NOy increases from 50 ppbv at around 60° S, where NOy lifetime is decreased due to the sunlight, to more than 500 ppbv at the pole, in the polar night.”

8. A main result of the paper is the impact of MEE, essentially via HOx, on ozone which the authors estimate to be of the same order as NOy produced by LEE. 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.

We addressed the issue about proton contamination of electron channels during SP events. In section 3.1 in 4th paragraph we added: “During strong SP events protons can contaminate the highest electron channel, so this channel is excluded from the AIMOS dataset (Yando et al 2011). Although some degree of contamination is still possible in the lower channels, protons are not the sole cause of the increased NOy in this SP event. Namely, SP events are often associated to large coronal mass ejections that form a shock in front of them. Once the shock hits the Earth if often leads to a geomagnetic storm which leads to acceleration of electrons of > 30 keV energies. Therefore, increased MEE precipitation often happens very shortly after SP event because the shock and the geomagnetic storm are related to the same coronal mass ejection driver (Asikainen and Ruopsa 2016).” By excluding SP events we would not get valid comparison with satellite measurements (Fig 2 and Fig 3) in which all EPP are

present. Because Fig 4 shows difference of experiment (ALL, LEE and MEE), where we have SP, and REF run, which also contains SP, we can focus on electron influence only.

9. 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

Thank you for the corrections, please see the answers below.

1. “or chemistry transport models”

Corrected.

2. “auroral only?”

Yes, Baumbaertner et al (2009) parameterization deals with low energy electrons (auroral) which are prescribed as NO influx through the model top.

3. “reference uses just one model”

Corrected. We removed “several”

4. “give reference”

Corrected. We added Rozanov et al 2012

5. “no new paragraph. Please combine”

We removed the last sentence of the first paragraph so separation is now better. The first paragraph describes new LEE parameterization, while the second presents the aim of the paper.

6. “ionization by . . . is”

Corrected.

7. “I do not see how the approaches really differ. This may depend on the implementation: could you please specify?”

The first approach is prescribing NO in the model domain between 0.09 – 0.01 hPa and the second is prescribing influx through the model top. We added “instead of fluxes through the model top” to clarify.

8. “Why this periods? 2005 is not the best year for MIPAS coverage. I propose to check 2003 (esp. first half) and 2008 - 2010.”

We have already addressed this comment above.

9. “in the MIPAS observations”

Corrected.

10. “Somewhat frustrating for the reader that the authors do not try to answer by themselves.”

This is true, but this issue deserves a study on its own, which is beyond the scope of this paper.

11. “style”

This is not clear, but we’ve changed the sentence to: ” The modeled NOy mesospheric anomaly peak is absent and enhancement of 10 ppbv. . .”

12. “alone”, “the”, “was”

Corrected.

13. but you are showing diffs between high and low years too.??

We removed “and not by on/off experiments as done here”

14. “specify”

The difference is very small and not statistically significant. We changed “small” to

“does not show a statistically significant”

15. “compared”

Corrected.

16. “Please explain missin June-16 event”

We have addressed this comment above. This event, because of its small magnitude was probably lost in the dynamic variability. However, the absence or presence of the SP event in question will not change the conclusions of the study.

17. “mean”

Corrected.

18. “combine sentences”

Combined.

19. “Do you really mean equatorward”

We changed this to “Between 30-35° S. . . “

20. “the”

Corrected.

21. “unclear. Please be concise”

We changed this sentence to: “However, increased MEE precipitation coincident with SP events may be a significant contribution to the observed NOy amounts.”

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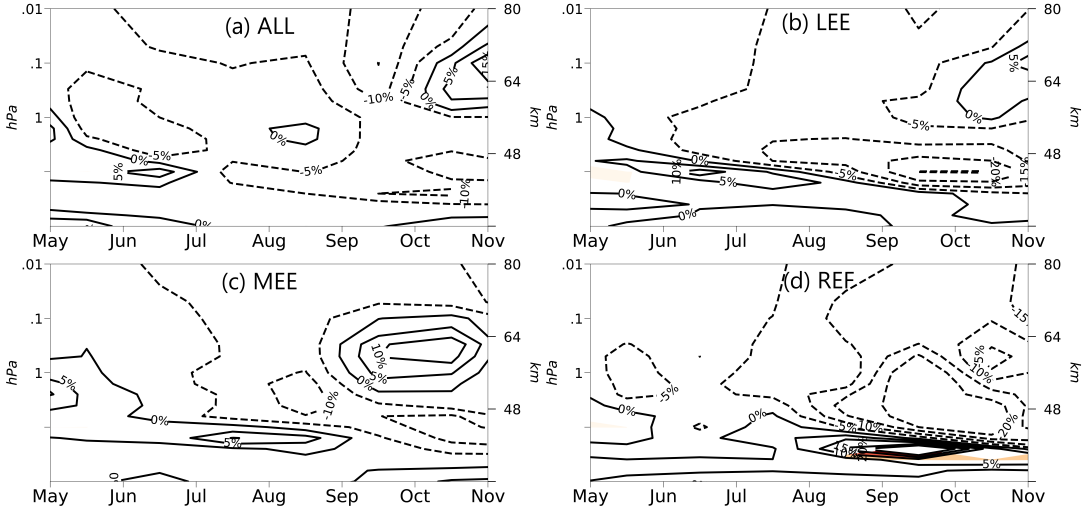

Figure S1: Monthly mean CO volume mixing ratio in % for the Southern Hemisphere (> 70° S average) calculated as difference of the year 2005 and the average 2006 – 2010. (a) ensemble mean of ALL simulations; (b) ensemble mean of LEE simulations; (c) ensemble mean of MEE simulations; (d) ensemble mean of REF simulations. Contour levels are: -20, -15, -10, -5, 0, 5, 10, 15 and 20 %. Colored regions are significant at the 99 % confidence level (calculated using a Student t-test).

Fig. 1.

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