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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 are sincerely grateful to the reviewer for their contribution. Many of their comments significantly improved our study. We have carefully analyzed and addressed all their comments below.

In the paper "Reactive nitrogen (NOy) and ozone responses to energetic electron precipitation during Southern hemisphere winter" by Arsenovic et al, model studies for the years 2002-2010 incorporating the impact of different populations of precipitating particles (medium-energy electrons, low energy electrons, solar proton events, and all) are analysed. Medium-energy electrons (MEE) and solar protons (SP) are included by

prescribing ionization rates, low-energy electrons (LEE, representing the auroral input) are input as an influx of NOy at the model top (1 Pa). Results show generally best agreement with observed NOy and ozone loss when all precipitating particles are included. Medium-energy electrons only contribute a small amount to the stratospheric and mesospheric NOy budget, which is dominated by low energy electrons; however to reproduce the stratospheric ozone loss, both low energy and medium energy electrons are necessary, though the reason for this is not clear. Energetic particle precipitation is considered part of the solar forcing of the climate system, and the LEE parameterization used here is recommended for the CMIP6 model experiments (see Matthes et al 2017). A comparison of model results using this parameterization with observations of NOy and ozone as presented here is therefore of great interest, e.g., for the interpretation of the CMIP6 runs, as is the determination of the individual contributions of the different particle sources as provided here by analysing model runs incorporating MEE, LEE and SP combined and individual. However, I have two concerns which must be considered before publishing. Both will need some changes to the text but no further analysis, so are probably minor, though important in my opinion:

1. In my opinion, the comparison of the individual contributions of MEE versus LEE is the most important result of the paper, as the LEE impact only has already been studied in earlier model studies (e.g., in Sinnhuber et al 2018, see also next comment). Maybe you could emphasize this point more by including this in the title of your paper? However, stating that you compare MEE and LEE contributions is misleading, because the upper boundary of your model – 1 Pa, about 80 km – does not represent a sharp boundary between LEE and MEE. Rather, the MEE impact as defined by you (electrons with 30-300 keV) extends from 100 km to possibly below 70 km altitude, while the LEE impact (aurora) probably extends from around 90 km upwards. There are three obvious difficulties with separating the individual impacts of LEE and MEE here: a) there is some overlap of MEE and LEE between 90-100 km; b) MIPAS observations, on which the upper boundary conditions for LEE are based, only scan the atmosphere up to 68 km altitude, so implicitly include contributions from both MEE and LEE which cannot

be separated; c) the upper boundary of your model does not include the whole altitude range impacted by MEE (from 100 km down to probably below 70 km), rather it cuts around 80 km (1 Pa), which might be even below the maximum of the MEE impact (I would assume that this is in the range 80-95 km altitude, though this is not really clear yet). So what you actually investigate are the individual impacts of electron precipitation to above and below your model top, which is interesting in itself, but not quite the same as the individual contributions of MEE and LEE with the definition provided in your paper. I don't see how you could do the analysis better, and considering that 1 Pa is rather conventional as upper boundary for state-of-the art CCMs, this is a very interesting investigation anyway. However, you only provide a lower limit for the MEE impact, and you must discuss this in some detail.

We have added two sentences in second paragraph, describing altitudes of auroral and radiation belt electrons: Radiation belt electrons (energies > 30 keV) impact chemistry below 90 km in the atmosphere (Turunen et al 2009).... [LEE] Their peak impact is above 90 km in the thermosphere (Turunen et al 2009). Methods, 3rd paragraph added sentence: "As mentioned before, LEE precipitate above 90 km and MEE precipitate between 70 and 90 km altitude (Turunen et al 2009). However, because of our model top at 80 km, here we consider electrons that precipitate below 80 km as MEE and electrons that precipitate above model top as LEE."

2. There is one publication (Sinnhuber et al 2018) discussion results of model experiments using the same upper boundary condition for LEE NOy as used here, considering the same period of time (2002-2010), and using a model with the same top height (1 Pa = 0.01 hPa) with probably very similar model dynamics (both based on ECHAM5). Results of this model experiment are compared to NOy and observed ozone loss, and generally show a good agreement. So you really must emphasize what is new in your paper compared to Sinnhuber et al 2018. In my opinion this is: a) you use a different setting of the upper boundary condition, incorporating fluxes instead of VMR, and b) you investigate the individual contributions of NOy above and below your model upper

boundary. Concerning the first point, you should compare your model NOy and ozone loss to the results of Sinnhuber et al 2018, to investigate whether there is an obvious difference between including NOy flux or VMR. My impression being that results are probably very similar for NOy, but mesospheric ozone loss seems more realistic when using flux over the model top boundary compared to VMR.

We added sentence in the last paragraph in the introduction: Sinnhuber et al (2018) showed impact of this LEE parameterization in their EMAC model on NOy and ozone, they they used another method of prescribing LEE and they don't consider MEE. We added Sinnhuber et al (2018) citation in methods "Matthes et al (2017) and Sinnhuber et al (2018) also implemented..." Comparison of Sinnhuber et al. (2018) is given in results, section 3.1: "Sinnhuber et al (2018) showed similar results in EMAC and KASIMA models and overestimation of NOy in 3dCTM model in southern hemisphere compared to MIPAS observations." In the last paragraph of section 3.2 we added: "In the study of Sinnhuber et al (2018) the three analyzed models (3dCTV, KASIMA and EMAC) generally show good agreement with the satellite observations."

3. Page 2, lines 9-10: the recommendation is either as influx through the model top or as volume mixing ration in the upper model boxes. Please clarify.

We have changed this sentence as suggested: "For climate models that have an upper lid below the thermosphere, a prescription of LEE either as NOx influx through the model top or as volume mixing ration in the upper model boxes is recommended (Matthes et al 2017)."

4. Page 2, lines 16 to 23, description of the MIPAS-bases upper boundary condition: my understanding is that MIPAS scans the atmosphere up to 68 km altitude, so any parameterization based on MIPAS data has to be extrapolated to the upper mesosphere above âĹij70 km. Evidence (from previous usage of this upper boundary condition) seems to be that this works really quite well, but nevertheless it must be a restriction of the method if applied to around 80 km altitude (1 Pa) as you do, and you must dis-

cuss this. Also, because the upper boundary condition is based on observations of the mid-to lower mesosphere, it is not a parameterization of LEE only as stated here – it must be a mixture of LEE and MEE effects, as these are not clearly separated in the observations in any way – it is NOy reacting correlated to changes in the geomagnetic indices, possibly with some time-lag, in both cases.

Regarding the MIPAS scanning the atmosphere to 68 km, we added the following text: Methods, 3rd paragraph: "Although MIPAS scans the atmosphere up to 68 km altitude, the applicability of this parameterization above 70 km has been validated by comparing to MIPAS Middle and Upper atmosphere observations (scanning up to 100 and 170 km, respectively)." It is true that MIPAS shows the mixture of LEE and MEE NOy, but it is impossible to clearly distinguish between them and this approximation is inevitable. However, NOy coming from MEE has a very small contribution to total NOy compared to LEE (as we showed in this paper), so this approximation should not have a big impact on ozone loss and thus induced changes.

5. Page 2, lines 22-23: it has already been shown in Sinnhuber et al 2018 that this upper boundary condition works really well in reproducing mesospheric and stratospheric NOy. This was actually done for exactly the same period of time as investigated in your paper (2002-2010), and using a model also based on ECHAM5 (EMAC), so you shouldn't generalize so much here (this already has been done), instead focus on what really is new in your paper: a) showing that the parameterization also works well if used as a flux through the upper boundary, and b) investigating the individual contributions of NOy above and below your model upper boundary. Please clarify.

We have removed line "It is therefore important to demonstrate that the particle impact is well represented in chemistry-climate models." from the introduction and added: "Although Sinnhuber et al (2018) showed impact of this LEE parameterization in their EMAC model on NOy and ozone, they they used another method of prescribing LEE and they didn't consider the MEE. Here we present results from a state of the art chemistry-climate model that employs the new Funke et al (2016) parameterization of

LEE together with the previous representations of other energetic particles." to the last paragraph in the introduction.

6. Page 3, line 9: A note on the definition of MEE and LEE here. As pointed out already above, the MIPAS upper boundary will probably be affected by MEE as well, as it is based on observations below 70 km altitude. On the other hand, AIMOS data in the vertical range of your model probably do not include all MEE effects even in the definition used here (30-300 keV electrons), as 30 keV electrons will have an impact on the atmosphere above your model boundary (1 Pa âĹij 80 km?) as well. Actually I would expect MEE to have the largest impact on NO in the altitude region 80-95 km. Actually I think that your model experiments with and without EEP NOy above and below your model lid are very interesting; however, I also think that you should clarify that this is not exactly the same as separating auroral and MEE electrons, due to the altitude of your model lid, and because there must be a vertical area where both overlap (probably between 80-100 km), and contributions are separated neither in AIMOS, nor in the MIPAS-based upper boundary condition. So please clarify this here.

We added to Methods section in 3rd paragraph: "As mentioned before, LEE precipitate above 90 km and MEE precipitate between 70 and 90 km altitude (Turunen et al 2009). However, because of our model top at 80 km, here we consider electrons that precipitate below 80 km as MEE and electrons that precipitate above model top as LEE."

7. Page 3, line 30-31: please state the vertical resolution (not the same as the vertical spacing of the limb-scans) of MIPAS NOy, and make some statement about the applicability of NOy above the top altitude of the MIPAS nominal limb scans.

The vertical resolution of NOy data is 19 pressure levels between 10 and 0.01 hPa (10, 7, 5, 3, 2, 1.5, 1, 0.7, 0.5, 0.3, 0.2, 0.15, 0.1, 0.07, 0.05, 0.03, 0.02, 0.015 and 0.01 hPa). We have edited the sentence to: "Since it provides the entire NOy budget in the upper atmosphere (on 19 pressure levels between 10 and 0.01 hPa), we used this dataset to validate simulated NOy." Regarding the second part of the comment, in the last paragraph of Methods section, we added: "The top altitude of the MIPAS nominal limb scans is 68 km, but it also contain information on the NOy above, though with low vertical resolution." And in Methods section in the 3rd paragraph: "Although MIPAS scans the atmosphere up to 68 km altitude, the applicability of this parameterization above 70 km has been validated by comparing to MIPAS Middle and Upper atmosphere observations (scanning up to 100 and 170 km, respectively)."

8. Page 4, lines 12-26, discussion of Figure 2: the comparison of the different model results to the observations is actually quite difficult, because a) the observations do not cover the whole year, b) NOy vmr is plotted with a logarithmic scale using shades of red – it really is quite easy to overlook a factor 2 difference in that way. I guess you had a good reason to choose 2005 as sole "high Ap" year. However, you could improve on b), maybe by using a color-scale with more contrast, probably also by overplotting the 1, 10, 100 and 1000 ppb contours of MIPAS on the different model scenarios. That would really help to distinguish differences in timing and magnitude, at least qualitatively, in a more comprehensive way.

We address the lack of data by adding the following sentences in the first paragraph in the 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." Regarding the Figure 2, we changed the colormap and reduced the number of plotting levels to make the Figure clearer.

9. Page 4, line 19 and line 20: the model and upper boundary condition discussed in Matthes et al 2017 and Sinnhuber et al 2018 are actually the same (though possibly a newer version of the upper boundary condition), so this sounds like a contradiction – NOy is underestimated in Matthes et al 2017 and overestimated in Sinnhuber et al

2018?

We have corrected the sentence in question: "Sinnhuber et al (2018) showed similar results in EMAC and KASIMA models and overestimation of NOy in 3dCTM model in southern hemisphere compared to MIPAS observations."

10. Page 4, line 25-26, "This difference is coming from increased MEE precipitation . . ." but is this real (compared to observations) or does it rather suggest a problem of the ionization rates used? Possibly due to proton contamination of the electron detectors during strong SP events?

We have added to the 4th paragraph in the section 3.1: "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)."

11. Page 5, lines 7-8: the possible contribution of MEE to mesospheric NOy during SPEs has already been investigated (using AIMOS data) in Wissing 2010 (also in Funke et al 2011?). However, if I remember correctly, the rather large impact discussed in Wissing 2010 might in part be due to proton contamination of the electron channels. How sure are you that this is not the case here?

We have addressed this comment in the previous answer.

12. Page 5, lines 22-24: how do your results compare to the model results of Sinnhuber et al 2018?

We added citation to Sinnhuber et al (2018) in this sentence.

13. Page 6, line 31: how does the ozone loss compare to Reddmann et al 2010?

The comparison with Reddmann et al (2010; Figure 14a) is not possible because they show different time period (single year 2003, where the Ap index is very high) and they show vertical propagation in time, while we discuss averaged JJA period.

14. Page 8, line 2: the transport of large amounts of NOy, I assume.

Although NOy produced by LEE gets transported in the downwelling circulation, LEE affect the chemistry by producing the large amounts of NOy.

15. Page 8, line 18-19: though the reason is not quite clear.

We added the sentence: “Future work is required to address the roles of indirect changes in temperature and dynamics in the EPP-induced stratospheric ozone variation.”

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