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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.

Anonymous Referee #2

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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:

– 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.

– 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.

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.

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 $\sim$70 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

discuss 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.

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.

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 $\sim$ 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.

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.

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.

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?

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?

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?

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

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

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

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

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