

Interactive comment on “Climate Impact of Polar Mesospheric and Stratospheric Ozone Losses due to Energetic Particle Precipitation” by Katharina Meraner and Hauke Schmidt

Anonymous Referee #1

Received and published: 26 July 2017

In this paper, simplified model experiments are carried out to investigate the impact of ozone loss induced by energetic particle precipitation on atmospheric temperatures and dynamics from the mesosphere down to the surface. The topic is highly relevant at the moment, as energetic particle precipitation is recommended as part of the solar forcing for the upcoming CMIP-6 model experiments (Matthes et al., ACP, 2017). The results therefore are of great interest, and the paper is also very clearly structured and well written. However, there are three points which need to be addressed before the paper can be published in ACP: a) the setup of the model experiments does not reflect the temporal and spatial structure of the direct and indirect particle impact as it is known from observations; b) some observation of the temperature response of

Printer-friendly version

Discussion paper



the winter-time stratosphere to geomagnetic activity exist (e.g., Lu et al., JGR, 2008; Seppaelae et al., JGR, 2013) but are not used here to compare the results of this model run (actually the observed amplitude is much larger than the results shown here). This comparison needs to be included as it provides ground truth to estimate how realistic the modeled response of the troposphere is; c) the estimation of significance using a t-test is not applicable to the high-latitude Northern hemisphere winter, where due to the occurrence of strong sudden stratospheric warmings the underlying distribution is bimodal.

These as well as a few more minor points are discussed in more detail below.

Page 1, lines 11 to page 2, line 8: the impact of energetic particle precipitation on the middle and lower atmosphere has been investigated since the 1970th, and a lot more has been published than referenced here. In particular there are two recent review papers which summarize the state of the art (Sinnhuber et al., Sur Geo, 2012; Mironova et al., Space Sci Rev, 2015), as well as reports on observations of a) the temporal and spatial structure of the indirect effect in different trace species (e.g., Hendrickx et al., JGR, 2015; Fytterer et al., JGR, 2015; Sinnhuber et al., JGR, 2016; Friederich et al., ACP, 2014); b) the temporal and spatial structure of the indirect effect in NO_y (e.g., Funke et al., JGR, 2014a, b) and ozone (e.g., Fytterer et al., ACP, 2015; Damiani et al., GRL, 2016; Kazutoshi et al., ACP, 2017), c) the impact of the indirect effect on stratospheric temperatures and winds in the Northern hemisphere winter and spring (e.g., Lu et al., JGR, 2008; Seppaelae et al., JGR, 2013), and d) the response of tropospheric weather patterns to geomagnetic activity (e.g., Seppaelae et al., JGR, 2009; Maliniemi et al., JGR, 2014). Observations provide the ground truth your model study has to compare to, so should be summarized here.

Page 3, lines 22-25, description of model experiments with reduced ozone loss: the scenarios differ quite substantially from what is known about particle induced ozone loss from observations of the direct and indirect impact. They are very much simplified, and of course there is justification for carrying out very simple model studies. How-

[Printer-friendly version](#)[Discussion paper](#)

ever, you should be aware how they differ from reality (as provided by observations), and discuss this carefully. Direct impact, mesospheric ozone: the direct impact has been shown to occur in sporadic events which are mostly short-lived (one day to a few days), but can occur in a periodicity related to solar rotation (27 days, 13.5 days, 18 or 9 days). It is restricted clearly to geomagnetic latitudes corresponding to the auroral oval (about 60-75° geomagnetic latitude). Implying this impact onto the whole polar cap should lead to an overestimation of this impact (see e.g., Hendrickx et al., JGR, 2015; Fytterer et al., JGR, 2015; Sinnhuber et al., JGR, 2016; Friederich et al., ACP, 2014). The indirect effect has been observed in every winter where observations in polar night have been available (Funke et al., 2014a,b). The impact of ozone is characterized by a downwelling negative anomaly starting in the upper stratosphere in mid-winter, and moving downwards to below 30 km in spring; it is restricted to the polar vortex (e.g., Fytterer et al., ACP, 2015; Damiani et al., GRL, 2016; Kazutoshi et al., ACP, 2017). Amplitudes are generally less than 20%, however it should be pointed out that observations show the difference of years with high to years with low geomagnetic activity; as the indirect effect occurs in every winter, see above, this is something different to the model experiments, which compare years with high activity to years with no activity, something that in reality doesn't happen even during deep solar minimum.

Page 4, lines 5-9, determination of statistical significance: using a t-test implies a distribution of temperatures which is random around a mean state. However, in the Northern hemisphere polar winter, this is obviously not the case: years with sudden stratospheric warmings are not outliers of the mean atmospheric state distribution, they belong to a different distribution: the distribution of temperatures do not approach a normal distribution (as student's t-distribution), but is bimodal, with one mode for the years without, and one mode for the years with warmings. Therefore, you can only use the t-test separately for years with and without warmings (if the distribution of those years is indeed symmetric, which maybe you should check before doing a statistical test); it is definitely not applicable, and therefore meaningless, for the whole sample of winters with and without warmings.

Page 4, line 25-27: I eventually understood what you did there, but the sentence was difficult to follow. Maybe you can clarify it.

Page 5, lines 12-13: there is one publication in ACPD at the moment which shows the same impact on heating rates (Sinnhuber et al., 2017) using a slightly different approach to yours. The results seem comparable, and I would encourage you to discuss/compare those results to yours.

Page 5, line 28: a change in the heating rate of 10% as for your stratospheric ozone experiment means a change of 0.1-0.2 K/day (see Figure 1). Observations and also the model study by Sinnhuber et al., ACPD, 2017, imply that this change in the stratosphere is not sporadic, but persists for several weeks, implying a warming during mid-winter of a few K. That is actually not a small change, and also in line with observations of the temperature response due to high geomagnetic activity in the high-latitude upper stratosphere (e.g., Lu et al., 2008; Seppaelae et al., 2013).

Page 5-8, discussion of statistical significance: a t-test is just not applicable if you combine years with and years without SSWs, see my comment above. I think you should study the change in years with and without warmings separately; then you can provide a robust measure of the significance. Also, this would make the results more comparable to the observations shown in Seppaelae et al., 2013, for the stratospheric response, as they also analyze years without warmings.

Page 8, line 4: the impact in the winter-time high latitude upper stratosphere temperatures you show in Figure 2 has a similar structure to observed temperature and wind field changes for years with high geomagnetic activity (Lu et al., 2008; Seppaelae et al., 2013). However, the amplitude of the warming is much smaller (about one order of magnitude?) than in the observations. This comparison to observations needs to be discussed here.

Page 8, line 8: the interhemispheric coupling is evident in both the meso-O3 and the strato-O3 experiments as a "statistically significant" change in the summertime upper

[Printer-friendly version](#)[Discussion paper](#)

mesosphere. However, this is more likely an affect of SSWs?

Page 8, line 25-30: The patterns and amplitudes you observe here should be compared to observations (Seppaelae et al., 2009; Maliniemi et al., 2014). However, as the amplitudes of your stratospheric warming appears to be much lower than observed, I would expect the impact on the troposphere also to be low compared to observations. Another point: Seppaelae et al., 2009 show that the impact on surface temperatures is different, with larger amplitudes, when years with SSWs are not considered. You should separate years with and years without warmings here as well. Can you reproduce their result regarding the impact of warmings? Again, a t-test is not applicable if you use years with and without warmings.

Page 11, 11: "Our results suggest that the climate impact of an ozone loss due to EPP is small" considering that the impact of particle precipitation in your analysis is masked by the strong variability implied on the Northern hemisphere winter atmosphere by sudden stratospheric warmings, and your results of the stratospheric impact strongly underestimate the observed response of the stratosphere, you can not draw this conclusion at this point.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-507>, 2017.

[Printer-friendly version](#)[Discussion paper](#)