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

Katharina Meraner and Hauke Schmidt

katharina.meraner@mpimet.mpg.de

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We thank the reviewer for the assessment of our work and the help to connect it better to previous studies. Below we reply point by point, first showing the reviewers comments in italic and blue followed by our response. To avoid confusion, we refer to graphics shown in this document as Fig. and graphics shown in the paper manuscript as Figures.

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 the winter- ime 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 occurence 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 Noy (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.*

We followed the suggestion of the reviewer and included a summary of the observational record on the ozone loss due to EPP as well as the impact on the stratospheric temperature and zonal wind to the introduction. We included a large number of the suggested references.

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. However, 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.

We agree with the reviewer that our description of the experiments was too brief. We added two paragraphs to Section 2.1, also taking into account the comment of Reviewer #2. We still believe that our experimental design is justified, because this allows us a clear signal-to-noise ratio and long simulation periods in order to gain as much insights in the processes governing the climate impact of EPP. But we now added a discussion on how our experiments differ from the observational record. In particular, the lack of a vertical propagation of the signal, the shift of the polar vortex and the EPP estricted to the auroral oval are now discussed. We would also like to thank the reviewer for providing such an extended list of references, from which several are now cited in the manuscript.

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.*

We analyzed the probability density functions (pdfs) for the variables shown in the paper. Fig. 1 shows exemplary the pdfs for winter polar mean (left) temperature averaged over (60 – 90N) and (right) zonal wind at 60N between 1 – 10 hPa. This corresponds to Figure 2 in the paper. Although the distribution is not smooth, it resembles more a normal distribution than a bi- modal distribution. We don't think that the notion of two distinct states, with and without an SSW is correct. There is a spectrum of major SSWs of very different peak intensities and durations which smoothly transitions into winter states without major SSWs which however often include minor SSW events of again different characteristics. This is also reflected by the fact that different SSW definitions may identify different cases. We think it is sufficiently justified to use Student's t-Test as it has been done in many earlier publications, e.g., by Seppälä et al. (2009), Arsenovic et al. (2016) and Gray et al. (2012). We decided to stick to the Student's t-test.

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

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. Thank you for pointing us to this paper. We added a comparison to this paper in Section 3.1.

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). We added the absolute values of the change in heating rates to the manuscript and compare it with the model study of Sinnhuber et al. (2017).

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.

We followed the suggestion of the reviewer and redid Figures 2-4 from the paper separately for SSW and no-SSW (see Fig. 2-4 only for no-SSW). For each SSW event the according season was marked as "with SSW". If a SSW occurs in February or November, also the next season was marked as "with SSW" (i.e., for February MAM and for November DJF). This method ensures the consistency of each season and prevents an influence of early or late SSWs on the next season. In total, we obtained 74 (71) winters without SSW for piControl (strato-O3). Comparing Fig. 2-4 to Figures 2-4 of the paper, we see that they are very similar and our conclusions still hold. For strato-O3 the signal, especially in the late winter, even weakens. We understand that the inclusion of SSW winters in the context of EPP forcing is somewhat problematic as such events, depending on their time of occurrence, may in reality or coupled chemistry models (not in our idealized setting) influence the forcing (i.e., polar ozone depletion) itself. On the other hand, discarding SSW winters might actually remove a big part of the signal, as a forcing may also change the timing of SSW occurrence (Gray et al., 2013). The QBO dependence of solar UV effects on the polar winter stratosphere, as shown e.g., by Labitzke et al. (2006), is strongly dependent on SSW occurrence. Additionally, see above, we don't think that the notion of a bimodal distribution of winter states is correct. Therefore, we strongly prefer to keep the figures showing all years (SSW+no- SW). But we added information on the changes in temperature and zonal wind if only no-SSW seasons are considered.

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.

It is true that our results match qualitatively very well the results (for temperature) obtained from reanalysis data (Lu et al, 2008, Seppälä et al., 2013). Whereas for the zonal wind response, the two studies differ from each other. Seppälä et al. (2013) showed a strengthening of the polar vortex with enhanced equatorward planetary waves, whereas Lu et al. (2008) showed a weakening of the polar vortex. We added this information to the manuscript.

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

We checked this for only winter without a SSW and still found a warming in the summer upper mesosphere (see Fig. 2). This suggests that the signal is not an effect of

SSWs. While we find this strong change very interesting, we think that further analysis is beyond the scope of this paper.

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.

We added a comparison of the surface temperature response to observations. In addition to the analysis of the full sample we analyzed the impact restricted to winters without SSW (see Fig. 4). A more detailed description on how this subset is calculated is given in the comment “Page 5-8”. We obtained larger amplitudes in the surface temperature for winters without SSW. However, till much smaller than in Seppälä et al. (2009) and Baumgaertner et al. (2011). The cooling over Northern America agrees qualitatively with the aforementioned studies, but we obtained no warming over Eurasia. We added the behavior for winters without SSW to the manuscript.

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 con-

clusion at this point.

We rewrote the whole paragraph and encourage now more research to clarify the effects of EPP. We think it is important to point out that the climate impact of EPP is not as clear as often thought.

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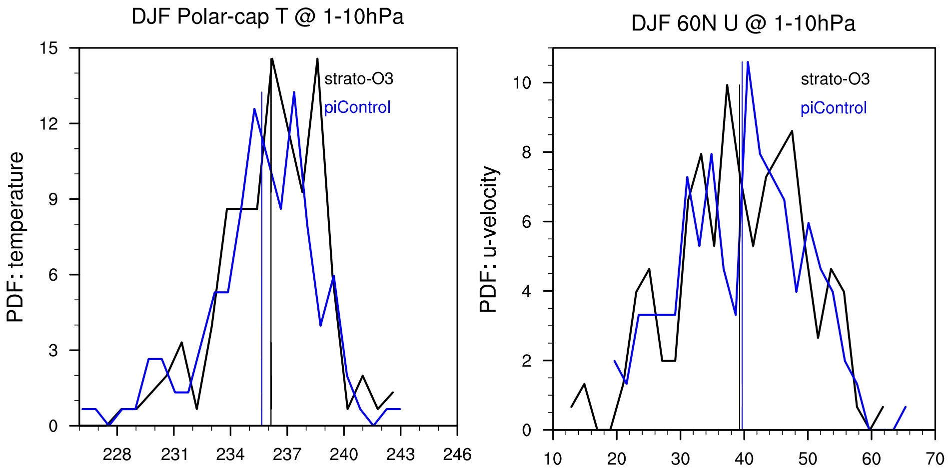

Fig. 1. PDFs for winter polar mean (left) temperature averaged over 60 – 90N and (right) zonal wind at 60N between 1 and 10 hPa. Two experiments are depicted: (black) strato-O3 and (blue) piControl.

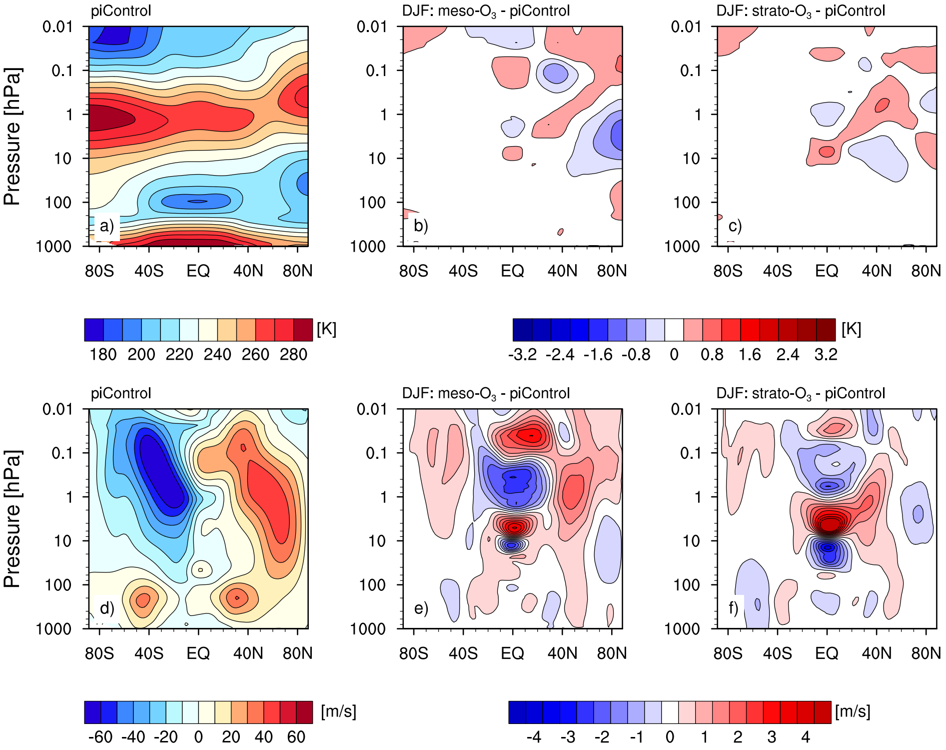

Fig. 2. Same as Figure 2 (in paper) but only for winters without SSW.

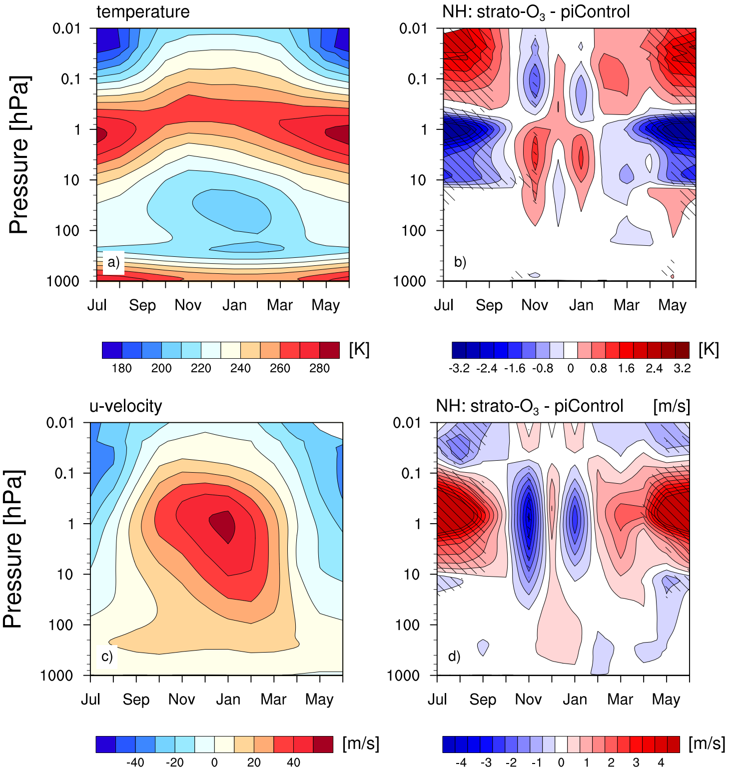

Fig. 3. Same as Figure 3 (in paper) but only for seasons without SSW.

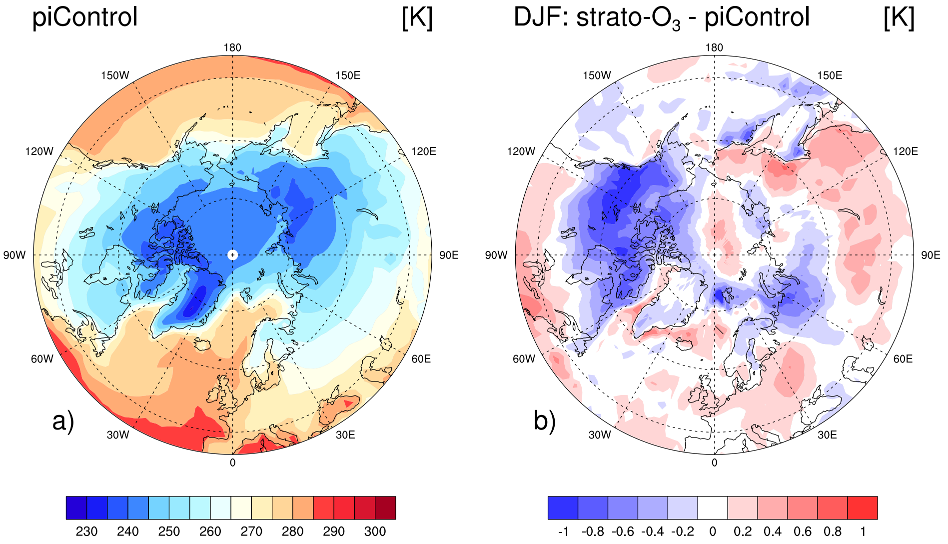

Fig. 4. Same as Figure 4 (in paper) but only for winters without SSW.