

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

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We thank the reviewer for the assessment of our work and the useful suggestions for improvements. Below we respond point by point, first showing the reviewer's comments in blue and italic 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.

The manuscript presents the response of the atmosphere and surface temperature to

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the introduced permanent decrease of the ozone concentration in the mesosphere and upper stratosphere simulated with the MPI-ESM model. The forcing was designed to mimic the ozone depletion by hydrogen and nitrogen oxides formed by the precipitating energetic particles. The subject of the manuscript is appropriate for ACP because it addresses widely discussed during the last decade question about possible influence of the energetic particles on the atmosphere, ozone and surface air temperature. The manuscript is well written, the most of relevant publications are cited, the figures are clear. However, the manuscript does not look mature because the bold conclusions cannot really be supported by the presented results. It seems obvious for the authors because in the summary they formulate why the results are not convincing and what to do to make them better. Therefore, I cannot recommend publication in the present form.

Main Issues:

1. The experimental design is too simplified. It resembles the ozone loss due to EPP obtained from the observations and models however substantially differs in the time evolution and distribution in space. Application of realistic ozone depletion scenarios could lead to very different results. If the authors do not know the implications of the chosen scenario (as it is said in the summary) what potential readers could learn from the paper? There are several aspects of the problem such as shift of the vortex from the pole and intensified ozone influence on solar radiation heating or interaction of the propagating disturbance with internal variability modes like PJO. These effects are automatically taken into account in the models considered all relevant to EPP processes, but they are missed if too simplified approach is applied. The simplest way to avoid the problem is to eliminated connection with EPP. Actually, the introduced ozone depletion scenario in the upper stratosphere is closer to the influence of halogens.

We agree with the reviewer that our description of the experiments was too brief. How-

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ever, its simplistic nature is intended and, we think, useful. We added two paragraphs to Section 2.1, also taking into account the comment of Reviewer #1. Earlier studies (see introduction for references) consider a mix of stratospheric and mesospheric ozone losses. The sole impact of a mesospheric ozone loss due to the direct EPP effect as suggested by Andersson et al. (2014) remains unclear. Additionally, a stratospheric warming due to EPP was identified in reanalysis data (Lu et al. 2008; Seppälä et al. 2013), whereas model studies obtained a stratospheric cooling either of dynamical origin (Baumgaertner et al. 2011) or of radiative origin (Arsenovic et al. 2016). In this sense, we believe that our experimental design is justified, because a) we can separate the climate impact of stratospheric and mesospheric ozone loss due to EPP; and b) the simplified approach allows us to gain insights in the processes governing the climate impact of EPP. Prescribing complex ozone reductions that vary in space, inter-seasonally and interannually, or simulating the ozone reduction interactively, might enable more realism but doesn't facilitate the identification of potential mechanisms. We think a reader can learn from our study that a) a significant climate impact of a mesospheric ozone change as suggested by Andersson et al. (2014) seems unlikely; and b) the interplay of dynamical cooling and radiative warming is complex and the climate impact of stratospheric ozone losses due to EPP is not as clear as often thought. In our simulations, we obtained a radiative warming in November and January. But in December, when the polar night is shortest and, hence, the radiative warming is strongest, a dynamical cooling is found. Therefore, additional research is needed to clarify the role of wave reflection for the dynamical feedback and for the coupling mechanism between stratosphere and troposphere. Furthermore, we now added a discussion on how our experiments differ from the observational record. In particular, the lack of downward propagation of the signal, the shift of the polar vortex and the EPP restricted to the auroral oval are now discussed.

2. I found interesting a large disagreement between the results of 80 and 150-year

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long runs. I guess, this phenomenon should be understood and explained with more details. I am not convinced that it is just the results of inter-annual variability. If so all modeling community is in a huge trouble. Did the authors check the presence of any model drift?

We agree with the reviewer that this large disagreement is interesting. Following your recommendation, we show different quantities that one might assume to influence EPP signals if they were drifting (Fig. 1). We do not find any drift in the model. The maximum difference (highest value – lowest value) in the sea surface temperature is 0.2 K for piControl and 0.17 K for strato-O3. This agrees with the internal variability in global mean surface temperature estimated by Sutton et al. (2015) for CMIP5 pre-industrial control experiments. We added a sentence to the manuscript and stated that no model drift is found.

3. The authors frequently discuss not statistically significant responses. I have noticed that almost all results presented in Figure 2 and 4 are not significant. It is rather interesting why the applied model is not sensitive to 20% decrease of the ozone in the polar upper stratosphere. There were several publications (mentioned in the introduction) claiming significant response of the atmosphere to the observed ozone depletion in the last decades of 20th century and the ozone depletion scenario is close to what is used in the manuscript. Some discussion of this issue is necessary.

It is true that most signals in Figures 2 and 4 are not significant at the 95% level. Nevertheless, it makes sense to analyze if the signals could have a physical explanation and not be purely accidental. Additionally, we want to emphasize that even if DJF averages are not significant, this can be different for individual months, as we show in Figure 3. Graf et al. (2007) and Langematz et al. (2003) used observed ozone changes to analyze the role of ozone for climate change. In both studies, the ozone is mostly reduced in the lower stratosphere, in contrary to the upper stratosphere in our study. Additionally,

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they used a rather short simulation period (10 years in Graf et al. (1997) and 20 years in Langematz et al. (2003)). Analyzing different simulation periods we obtain mesospheric warming and cooling of apparent significance. However, also compared to observational records of temperature and zonal wind responses due to EPP (Lu et al. (2008) and Seppälä et al. (2013)), the amplitude of our responses are smaller. We now added a comparison to the above mentioned studies.

4. Section 3.1: The use of 75N should be better motivated if the authors would like to wire these results with ozone depletion due to EPP. If the ozone depletion occurs inside polar vortex then 75N is not representative because huge ozone influence on solar heating rate outside polar night area will dominate over very small longwave effect. It should be also considered that in the Northern hemisphere the vortex is not stable and tends to move from the pole out of the polar night area.

Figure 1 is only an illustrative example of polar ozone heating rates and it is not thought to be representative. At other latitudes the polar night would be, of course, shorter or longer. Additionally, we agree with the reviewer that the length of the polar night exposure of an air parcel depends on altitude and the actual dynamics (e.g., movement of the air parcel). The pure radiative response to ozone loss should be a warming in mid-winter and get weaker towards early and late winter. However, our Figure 3 shows a warming in November and January/February, but not in December. The December cooling is of dynamical origin. We now discuss the missing shift of the polar vortex in Section 2.1 and added the above mentioned information to Section 2.2.

Minor Issues:

1. Page 2, line 2: if → of. Done.

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2. Page 2, line 4: Langematz et al. (2003) showed tiny direct LW warming (Fig.7) , but the resulting stratosphere is cooler (Fig.8). Graf et al., (1998) showed the response in the lower stratosphere (70 hPa). Thank you for pointing this out. We changed the sentence to: “During polar night reduced ozone slightly decreases the infrared cooling of the polar stratosphere resulting in a net (small) stratospheric warming (Graf et al., 1998; Langematz et al., 2003). However, both studies prescribed an ozone loss in the lower stratosphere.”

3. Page 3, line 23-25, line 31: The ozone depletion scenario is too simplified. We extended the description of the applied ozone losses and discuss now differences to observed changes. See also reply to major comment 1.

4. Section 2.2: The radiation code is not described. The references do not provide satisfactory information about the treatment of solar (e.g., spectral range coverage, spherical) and infrared (e.g., LTE treatment) radiation. The standard version of the RRTMG does not include wavelengths shorter 200 nm and therefore the heating rate in the mesosphere should be heavily underestimated due to the absence of Lyman-alpha line and Schumann-Runge bands. How it is treated in Psrad? The solar and infrared radiation is treated in Psrad in the same way as in RRTMG. Hence, wavelengths shorter than 200 nm are not included. However, the absorption of ozone takes primarily place in three spectral regions: Hartley band (200 – 310 nm), Huggins band (310 – 350 nm) and Chappius band (410 – 750 nm) (Brasseur and Solomon, 2005). All of those bands are considered in RRTMG and, hence, also in Psrad. The Schumann-Runge bands are of great importance for the mesosphere, but primarily due to absorption of molecular oxygen (and not ozone). In this sense, we underestimate the total heating rate in the mesosphere. However, in our setup we compare two radiative transfer calculations. The difference between both calculations is not (at least not strongly) affected by the underestimated total heating rate. We extended the description of Psrad in the manuscript.

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5. Page 4, lines 16-18: *I do not understand what means “separately . . .and then combined”. Why CO2 is not in the input list. Is it not included in Psrad?* We rewrote this sentence to make it clear that the optical properties are calculated for shortwave and longwave separately, but then combined to estimate the total heating rate. CO2 and O2 are set to fixed values invariant with height in Psrad. We added this information.

6. Page 4, line 24: *Actually, the length of the polar night depends on the altitude and at 80 km it could well be shifted by one month relative to the surface. In Figure 1 this effect is absent, which affects the results in the mesosphere.* Thank you for pointing this out. Please see also comment to major point 4. We added a discussion on the representativeness of 75N to the manuscript.

7. Page 5, line 4: *The maximum of the ozone VMR is normally around 6 hPa for this location. What ozone profiles were used?* We used ozone profiles averaged over the late 20 th century provided by the general circulation and chemistry model HAMMONIA. In this profile the maximum ozone VMR is also around 6 hPa. The strongest heating occurs around the stratopause, which agrees e.g., with Brasseur and Solomon, (2005). We adjusted the sentence accordingly.

8. Page 5, line 33: *I guess, Langematz et al. (2003) showed the same.* We added Langematz et al. (2003) to the references.

9. Page 6, line 9: *75N is not really representative (see above).* Please see the comment to major point 4.

10. Page 8, line 5: *75N is not really representative (see above). This result disagrees with Langematz et al. (2003, see their Figure 7 and 8).* Langematz et al. (2003) showed a dynamical cooling in the polar winter stratosphere but expected also a warming from the radiative transfer modeling. In contrast, Lu et al. (2008) and Seppälä et al. (2013)

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showed a warming in the polarwinter upper stratosphere due to EPP in re-analysis data, but the magnitude is much stronger (5 K) than in our simulations. Furthermore, we found a small dynamical cooling in December, which is caused – as in Langematz et al. (2003) – by a reduction of waves entering the stratosphere. In this context, we discuss the differences to Langematz et al. (2003). We added the comparison to Lu et al. (2008) and Seppälä et al. (2013) to the manuscript.

11. Page 8, line 15: *statein → state in.* Done.

Additional references:

Sutton, Rowan, Emma Suckling, and Ed Hawkins. “What Does Global Mean Temperature Tell Us about Local Climate?” *Phil. Trans. R. Soc. A* 373, no. 2054 (2015): 20140426. doi:10.1098/rsta.2014.0426.

Brasseur, Guy P., and Susan Solomon. *Aeronomy of the Middle Atmosphere: Chemistry and Physics of the Stratosphere and Mesosphere*. Springer Science & Business Media, 2005.

Lu, Hua, Mark A. Clilverd, Annika Seppälä, und Lon L. Hood. “Geomagnetic Perturbations on Stratospheric Circulation in Late Winter and Spring”. *Journal of Geophysical Research: Atmospheres* 113, Nr. D16 (2008): D16106. doi:10.1029/2007JD008915.

Seppälä, A., H. Lu, M. A. Clilverd, und C. J. Rodger. “Geomagnetic Activity Signatures in Wintertime Stratosphere Wind, Temperature, and Wave Response”. *Journal of Geophysical Research: Atmospheres* 118, Nr. 5 (2013): 2169–83. doi:10.1002/jgrd.50236.

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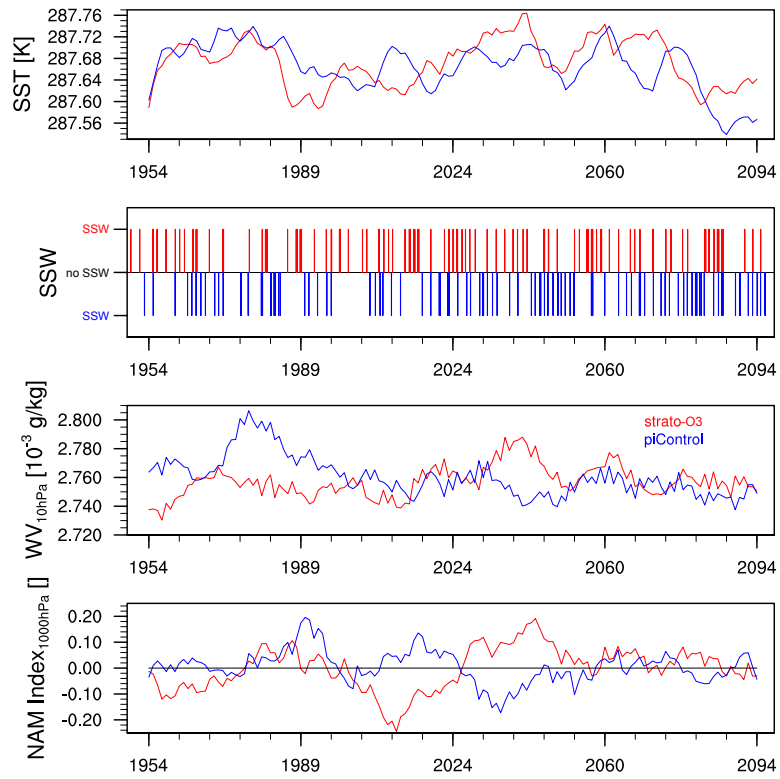


Fig. 1. 10-year running mean of global SST; occurrence of SSW; 10-year running mean of global water vapour [10 hPa] and 10-year running mean of NAM index [1000 hPa] for piControl (blue) and strato-O3 (red).