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Atmospheric Chemistry and Physics Discussions

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 #2

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General comments:

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

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

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 introduc-

tion) 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.

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.

Minor issues:

1. Page 2, line 2: if –> of

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).

3. Page 3, line 23-25, line 31: The ozone depletion scenario is too simplified.

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?

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?

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.

7. Page 5, line 4: The maximum of the ozone VMR is normally around 6 hPa for this location. What ozone profiles were used?

8. Page 5, line 33: I guess, Langematz et al. (2003) showed the same.

9. Page 6, line 9: 75N is not really representative (see above).

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).

11. Page 8, line 15: statein –> state in

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