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Comment

## ***Interactive comment on “Will climate change increase ozone depletion from low-energy-electron precipitation?” by A. J. G. Baumgaertner et al.***

**Anonymous Referee #2**

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### General comments

The paper compares the effects of the low energy electron precipitation (EEP) on nitrogen oxides and ozone for the present day and future atmospheres. This question is relevant to the scope of ACP. This problem has not been considered in the published literature and, therefore, is novel. From the experiments with CCM EMAC the authors concluded that in the future atmosphere the enhancement of the nitrogen oxides due to EEP will be more pronounced over the southern hemisphere during winter-spring season leading to additional ozone depletion in the upper stratosphere between 60 and 70 deg. south. Applied scientific method and assumptions are valid however the

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model set-up is not always clearly outlined. The attribution of the obtained effects to the increase of the Brewer-Dobson circulation is not sufficiently supported by the presented results. In general, the paper looks a little bit sketchy and I would suggest performing more careful analysis to make the author's conclusions more convincing. The authors properly credited to related publications and clearly indicated the originality of the work. The title reflects the content of the paper. The abstract provides a concise and complete summary of the obtained results. The paper is well written and structured however the analysis of the results can be and should be more detailed (see "specific comments"). I think the paper can be published in ACP after major revisions.

### Specific comments

#### 1. Brewer-Dobson circulation.

The authors just say in the introduction that BD circulation consists of "...poleward transport in the middle and upper stratosphere, with rising air in the tropics and downwelling in the polar regions." However it is well known that the horizontal mixing also plays important role possibly leading to a dilution of EEP NO<sub>x</sub> (which could mean smaller magnitude but large horizontal coverage). The MLT circulation differs from the BD circulation and its behavior in the future climate is not considered in the introduction. This issue is very important for the considered problem because it provides initial downward propagation of the EEP NO<sub>x</sub> from the thermosphere. I think it is important to discuss these issues in the introduction and also extend the analysis showing the simulated changes of the atmospheric circulation using proper quantities (e.g. stream function, TEM circulation, passive tracer, methane, carbon monoxide and so on). Figure 6 in the present form (annual mean ozone change) is not very instructive for the illustration of the circulation changes because the ozone changes in the middle to upper stratosphere are controlled mostly by halogen loading and stratospheric cooling. From the Figure 6 it is really difficult to say anything about the downward penetration of the mesospheric air inside polar vortices.

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## 2. Experimental set-up

The description of the experimental set-up is not clear. The text explaining SST does not agree with Figure 1,2 captions (the reference periods are different), which makes it difficult to understand. I think that the discussion of SST choice can be shortened and figures 1-2 can be eliminated. It would be enough to say which data set is utilized, which years are used for the climatology and just mention that there is no El-Nino/La Nina bias in the SST fields. I also do not understand why two individual years are shown in Figure 3. In the description the authors said that AMIP and ECHAM5-MPIOM climatological SSTs are applied, and then the comparison of two individual years (2000 and 2100) does not make much of a sense. It is also important to justify the choice of AMIP SST for 2000 instead of ECHAM5-MPIOM SST from 20th century experiment. Some discussion of the ECHAM5-MPIOM SST biases is necessary. Nothing is said about halogen loading and ozone precursors for the future atmosphere. The model initialization is not properly discussed. If the future run is initialized from present day atmospheric state then 3 years spin-up is probably not enough to reach quasi-equilibrium, because the mean ages of the air can substantially exceed 3 years.

## 3. More issues to consider.

In addition to the BD circulation changes there are another process which can lead to different response. For example, the magnitude of EEP NO<sub>x</sub> and ozone depletion may depend on chlorine loading, stratospheric temperature, ozone mixing ratio. I am not sure how important these factors are but it will be very important for the readers to be convinced that the BD circulation change is the only reason. I recommend also to show/discuss the results for the northern hemisphere winter and for the mesosphere. It can give additional information about the processes.

## 4. Compensation

The authors stated that "... an additional ozone loss of up to 0.5 ppmv ... would be compensated by an ozone enhancement ...". It is important conclusion, but I cannot

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really understand what the authors mean by this. At the moment it reads like there is some additional ozone depletion due to EEP NO<sub>x</sub>, but it is compensated by other factors. Well, if it is compensated, then there should be no ozone depletion in Figure 8. Please, clarify. I think an additional figure (similar to figure 6 but for the experiments with EEP NO<sub>x</sub> and for the proper season) and discussion would be helpful.

#### 5. Future ozone

Discussing Figure 6 the authors explained the ozone increase in the upper stratosphere by the cooling of the stratosphere due to greenhouse gas increase (page 9903, lines 5-8). Does the halogen loading play any role? Does the model include the changes of ODS in the future?

#### Minor comments and technical corrections

1. Please, use the same name for the model (ECHAM5/MESSy or EMAC)
2. Page 9897, lines 1-2: I recall there was a paper by Marsh et al. (JGR, 2007) about the effects of low energy electron precipitation. Is it relevant to the present paper?
3. Page 9897, lines 18: Please, define “opening of the subtropical jets”
4. Page 9897, line 23: Not necessarily. The efficiency of NO<sub>x</sub> cycles is not increasing steadily with the pressure.
5. From page 9898, line 23 to page 9899, line 7: Simple set of abbreviations does not help to understand the model. I guess this paragraph can be eliminated. For example what the reader can learn from “. . .RAD4ALL (radiation code). . .”?
6. Page 9899, line 18: what kind of emissions from boundary layer are included?
7. Page 9901, lines 18-20 : From the Figure 4 I would say the highest NO<sub>x</sub> production should be seen in November-December over the winter hemisphere.
8. Figure 5: Figure is wrong because there is no experiment with NO<sub>x</sub> off.

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