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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lightning NO_x emissions over the USA investigated using TES, NLDN, LRLDN, IONS data and the GEOS-Chem model" *by* L. Jourdain et al.

Anonymous Referee #2

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General comments:

This study analyzes the effect of lightning produced NOx onto upper tropospheric ozone over the U.S. based on TES satellite observations and the GEOS-CHEM model. The model was run in several different configurations to demonstrate the effect of lightning on upper tropospheric ozone. Both satellite and ground-based observations of lightning activity were used to rescale model-generated lightning (and hence NOx-production) fields.

The paper is generally well written and the use of model simulations and observations is appropriate to address the scientific questions posed. In my view the study has two

major innovative aspects: Firstly, the authors were able to improve the representation of lightning in GEOS-CHEM by scaling the model generated lightning to real lightning observations from OTD/LIS and NLDN. This step was very important for the purpose of this study since the standard lightning parameterization of the model, which is based on Price and Rind, has serious flaws. The second innovative aspect is the use of TES ozone observations to study the effects of lightning NOx and the demonstration that these measurements are clearly suited for this.

The paper draws a number of important conclusions that deserve being published: - It is convincingly shown based on both TES observations and the model simulations that lightning has an important effect on upper tropospheric ozone over the US in summer supporting the results of previous studies.

- TES ozone observations are sufficiently sensitive and vertically resolved to demonstrate the effect of lighting on upper tropospheric ozone.

- The Price and Rind parameterization places too much lightning into the tropics and too little into the extratropics (not really a new conclusion) but a regional rescaling using satellite or ground-based lightning observations is possible and improves the results (a fairly new conclusion).

- Ozone enhancements tend to be significantly larger in the observations than in the reference model simulation. A scaling of the NO production per flash by a factor of two from 260 mol NO / flash (used in the reference) to 520 mol NO / flash, consistent with other recent studies, clearly reduces the bias between observed and model simulated ozone.

- Attributing upper tropospheric ozone to either NOx from lightning or to NOx convectively lifted from the PBL is very challenging since both processes are depositing NOx in the upper troposphere in a highly correlated way. As a consequence, the correlation between model and TES ozone is only reduced significantly when both sources are switched off in the model but it changes only little when only one of the two is missing.

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Interactive Comment

Probably this important finding should be better stressed in the paper.

Specific comments:

The study has a few minor issues that should be properly addressed before publication:

1. The analysis of the sensitivity of the model results to the representation of lower stratospheric ozone is important but these results are not sufficiently well discussed and have too little weight in the paper, in particular in the conclusions section. Changing the model setup to a more realistic representation of lower stratospheric ozone obviously leads to upper tropospheric ozone changes that are of the same order of magnitude as a doubling of lightning NOx. Based on the results of this study I would even conclude that a good representation of lower stratospheric ozone (and of cross-tropopause fluxes) is essential before the role of lightning NOx can be properly assessed. The present study clearly suffers from the imperfect representation of the stratospheric ozone contribution and a further conclusion should therefore be that the GEOS-CHEM model should be improved in that respect.

2. The simulation Sbase corresponds to a global lightning NOx production of 6 Tg N/yr. The simulation SNLDN appears to be about 40% higher than this and the simulation Slighx2 is again 2x higher than SNLDN and thus 2.8 times higher than Sbase. Since the simulation Slighx2 appears to provide the best results in terms of upper tropospheric ozone, should it not be concluded that a global lightning production of about 2.8 times the value of 6 Tg N/yr, i.e. about 17 Tg N/yr, would be more consistent with the TES ozone observations, at least in the GEOS-CHEM model? I think this point at least deserves some discussion.

3. Section 3.2.1 is not convincing and needs to be improved significantly. Figure 8 shows that the probability of having a non-zero daily lightning activity in a particular grid box in GEOS-CHEM when NLDN observes lightning activity is of the order of 60%. From this it is concluded that the ”daily pattern of convective activity occurrence is correctly reproduced by the model”. I doubt that this is a good measure for showing

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this. Think of a model that produces lightning in every grid box on every day. Would such a model not perform even better in terms of this measure? I think that another score factor should be used which also takes into account the agreement for grid cells with no lightning. By the way, Price and Rind (1992) had published a very nice way for judging the spatial agreement between model and observations in terms of lightning activity.

4. The title contains many abbreviations and probably some readers will not be familiar with all of them. Of course, the authors have used an impressive number of different datasets in their analysis but I do not think the title is a good place for stating this fact. What about ”The effect of lightning NOx on upper tropospheric ozone over the USA investigated using TES satellite observations and the GEOS-CHEM model” or ”Lightning NOx emissions over the USA constrained by TES ozone observations and GEOS-CHEM simulations”.

Technical corrections:

- Last sentence of abstract: Add ”ozone” after GEOS-Chem because otherwise it might not be clear what you are comparing.

- p1131, line 7: Nox -> NOx

- p1131, line 9: I don not really see why 0.28 TgN is ”consistent” with the much higher value of Hudman et al. of 0.45 TgN. - p1133, Lines 9-12: This sentence could be dropped.

- p1134, line 12-14: This was already explained in section 2.4

- p1134, line 24: ”This error is typically of about” -> ”This error is typically about”

- p 1139, line 10: The units should be mol NO / flash not NO mol / flash.

- p1140, line 4: ”increase” -> ”increased”

- Last sentence in conclusions section: Change to ”.. sondes reveal that stratospheric

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influences are”

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