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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of lightning-NO on Eastern United States photochemistry during the summer of 2006 as determined using the CMAQ model" *by* D. J. Allen et al.

Anonymous Referee #2

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The manuscript by Allen et al. is potentially suited for publication in ACP, but requires major revisions as explained in detail below.

Major concerns

1. The paper (title, topic, results and figures) is in large parts similar to Allen et al., JGR, 2010. The authors have to clearly announce this in the introduction and have to clarify what is new/different in this study and in how far a separate publication is justified.

2. The authors announce a "new lightning-NO parameterization" (17703/15). This new scheme has to be set in relation to existing lightning parameterization schemes (see e.g. Tost et al., "Lightning and convection parameterizations", ACP, 2007.)

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Interactive Discussion

Discussion Paper

The authors have to demonstrate the (superior?) performance of their new parameterization scheme, e.g. by adding modeled flash rate distributions from other schemes to Fig. 2 exemplarily.

3. Our knowledge on lightning NOx is still highly uncertain and inconsistent (compare e.g. Beirle et al., ACP, 2010, which is not compatible to a LNO production of 500 moles/flash). The authors indeed mention some discrepancies and shortcomings in the text, but from reading abstract and conclusions only, one might get the impression that by just adding the LNOx to the model, everything works out fine

But this is not the case:

a) The fact that upper tropospheric NOx is underestimated by CMAQ might indeed be due to missing LNOx (of course, by adding an upper tropospheric source, a bias can be reduced). But the bias might as well be caused by a wrong/insufficient implementation of chemistry and/or deep convection in the model; the latter should also be discussed by the authors. It is evident from Fig. 17 that there is something completely wrong in the upper troposphere, which can not be fixed by just tuning the LNO source.

b) The NO2 comparison done by the authors (section 3.1) is not conclusive:

b1) Three OMI NO2 products are considered, but only the results for DOMINO are given as numbers in the text; for DP-GC, I assume that the addition of LNO even leads to worse agreement, if AKs are considered! Numbers for all products have to be given in the text, and the CMAQ avgK columns should be also added to Fig. 4 for better comparison.

b2) All number are based on temporal and spatial averages over large scales. The mean LNO2 contribution (0.31e15 molec/cm2) is of the same order of magnitude as the uncertainty of the stratospheric estimation (0.15-0.2e15, Boersma et al., 2007), which is potentially systematic, i.e. is not eliminated by the temporal averaging. Consequently, the observed difference of model and satellite must not be over-interpreted (compare 17714/4-5).

b3) The authors admit that the addition of LNOx does not improve the correlation of daily mean NO2 between model and OMI, despite the large-scale averaging. This is of course rather disappointing. It would be quite interesting to investigate if there is at least a correlation of the observed daily NLDN flash rates (prior to the OMI measurement) and the observed OMI columns? If this is not the case, there is no indication at all that OMI columns are affected by lightning, and quantitative conclusions are meaningless.

Thus, the authors have to

- clearly admit the still existing uncertainties w.r.t LNOx, also in abstract and conclusions,

- discuss the a-priori choice of 500 moles/flash (it is at the upper end of the estimates given in Schumann and Huntrieser, but still can not fix the bias in the UT! In addition, it was found by Jourdain et al., 2009, by comparing model to TES O3 data, while, in this study, the authors clearly state that "LNO algorithms should not be evaluated by how much they improve biases between modeled and measured UT O3", 17718/8-10),

- clarify, how far the comparison in 3.1. actually tells us anything on LNOx.

Further comments:

The discussion of rural vs. urban regions is interesting in itself, but somehow off topic within this study. I recommend to shorten this discussion in 3.1, and especially in the abstract (17701/14-21).

17701/8: Add "Assuming a LNO production of 500 moles/flash, . . .".

17704/10: The DOMINO product is currently updated, see Boersma et al., AMTD, 2011. Please check how far the changes affect your conclusions.

17704/21: When investigating lightning NOx, the selection of cloud free pixels probably introduces a systematic bias, as lightning is generally accompanied by clouds. Please

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comment on that.

17706/28: Add a reference to Kain-Fritsch parameterization.

17713/18: Replace the ";" by a ",".

17714/19: How large would these biases be?

17715/11: Replace "belief" and give a reference.

17725/8: Add "Assuming a LNO production of 500 moles/flash, . . .".

Figures: The authors should improve the choice of colors in their figures; avoid having the same color+line style for two different data sets as in Fig. 4. The purple in Fig. 17 is hard to recognize.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 17699, 2011.

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