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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 #1

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General comments: This paper uses a " new lightning-NO parameterization " in the CMAQ model in order to assess the effect of lightning on several features of the atmospheric composition (NO2 and O3 columns; wet deposition of nitrates). Starting with a " classic " scheme of lightning parameterization proportional to convective precipitation like in Allen and Pickering 2002) developed by Koo et al., 2010 in CMAQ, the authors uses NLDN observations to adjust flash rates in the model. This new version of the model is used to evaluate the impact of lightning NO over photochemistry of USA during 2004 and 2006.

My major concern deals with the poor amount of new results in this paper that is much

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devoted to model development. There is however an interest for the CMAQ community with important model improvement. However most of the conclusions are derived (1) from the comparison of previous version of the model (without lightning) with the implementation of the lightning NO scheme (summer 2004) which hopefully gives better results than unrealistic simulations without lightning NO parameterization, and (2) after unconvincing evaluation of the model respect to in situ and satellite observations (NO2 and O3, summer 2006). The author should insist on the evaluation of the model respect to in situ and satellite observations. They may have a thorough discussion on the biases between the model and observations as they do well in Sec 3.5. It is indeed difficult to give general conclusions on the influence on lightning NO on tropospheric chemistry (Sec 3.3 Sec.3.4 and Sec.3.5) when the model is high biased. Moreover I find a lot of similarities in the methodology and the scientific objectives in a recent study of the authors, using another model (GMI): Allen et al JGR doi:10.1029/2010JD014062 2010. The only new results concerns in this paper concerns the discussion of urban versus rural sites for tropospheric NO2 columns in Sec3.1 and the impact on deposition of nitrogen species in Sec 3.4. I don't think this paper should be published in ACP unless important modifications and improvements.

Specific comments:

Abstract Lines 8-12 I am not sure the authors can give such conclusions on lightning NO contributions to NO2 tropospheric columns and O3 mixing ratios when the model is high biased in reproducing observations (Same remark for Sec 3.1, 3.2 and 3.4)

Introduction p 17703 lines 25 to 32 What is the interest of such sentence? Of course it is expected to have lower errors in a simulation with lightning-NO than without, as a simulation without lightning-NO is not realistic. It would have been interesting to see how this new parameterization, compared to Koo et al., 2010 can better reproduce measurement during INTAS, not in comparison to a study without lightning NO from Napelenok et al. (2008)

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Sec 2.3.1 Lines 10-14 p 17709 Similar lightning constraint approach from ground based and satellite observations has already been developed in previous studies that should be mentioned in the paper (Sauvage et al., 2007 with OTD-LIS; Jourdain et al., 2010 with NLDN). Moreover the author uses a very similar approach in a previous paper (Allen et al. 2010) with another model by constraining flash rates with OTD-LIS climatology and this should have been mentioned in the paper.

lines 24-30 p17709 Description is unclear. " closely as possible " Please clarify. " to avoid very large flash adjustments " some precision should be added. Is this something happening a lot of times? How many adjustments are outside 0.1 and 10?

lines 1-12 p 17710 Why are you using a different method to determine the percent of emissions for year 2004 and 2006? This is not explained and confuses the manuscript. The different methods should be tested for the same period (e.g. summer 2004) in order to evaluate the consequences on lightning NO simulations

Sec 2.3.2 p 17711 Could you please explain the important correlation differences between diurnal and daily variations in summer (and winter)?

lines 4-5 I don't see such a good agreement between the model and observations. Coefficient of determination would be more useful in such analysis (Rˆ2, not R) and is indeed quite low, except in summer for diurnal flash rate. Please clarify.

line 25-28 What is a "stronger synoptic forcing"?

Sec3 A discussion should be realized on sensitivity test dealing with unrealistic simulations without lightning NO emissions (e.g. please see and add reference of Kunhikrishnan et al 2004) at least in order to interpret non linear processes such as for ozone

Sec 3.1 It is really hard to follow this section. The interest of this section is also very difficult to see as the methodology and the conclusions are very weak or confused, especially the ones on lightning NO contribution. The main interest of this section

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concerns the comparison between rural and urban regions and should be highlighted. Fig.4 and Fig.5 demonstrates strong differences between model (with lightning NO) and DOMINO NO2 columns and it is hard to derive strong conclusions from a sensitivity test with and without lightning when the model is such biased compared to observations.

p17712 The domain used for averaging is big. Is there a strong geographical variability between the model and satellite to average on such a big domain?

p17712 lines 3-4 What is the interest of such a sub-section if the comparison is not rigorous, as claimed by the authors? How conclusions on lightning-NO contribution can be deduced if the comparison is not rigorous, and when there is such a poor agreement between the model and the satellite retrieved column (except for mid July mid August with DP-GC mean columns)?

Figure 5 Why the authors use the comparison between 3 different models column representations and the DOMINO columns only? Why not the others products?

Figure 6: p17714 lines 15-16 “addition of lightning NO decreases bias” A lower bias is at least expected when adding lightning NO. What is the new result of this sensitivity test?

Sec 3.2 It is hard to derive conclusions on lightning NO contribution when the model gives better results without than with lightning NO emissions. Fig.9 shows indeed high biases between the best version of the model compared to observations. The strength between model and satellite given in Fig.9 are very weak (Rˆ2 does not exceed 0.6).

Figure 10: It is not because adding lightning in the model increases ozone by 6 to 12 ppbv, that the authors can conclude lightning NO production adds 6-12 ppbv to upper tropospheric ozone. Indeed the model does not match with satellite observations so it is hard to believe the simulations, and second non linear processes should be taken into account in such sensitivity tests (LNOx or NO LNOx) as ozone production (destruction)

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is not a linearly dependant of NOx (see the discussion in Kunhikrishnan et al GRL 2004).

Sec 3.3 Same remark than before on the conclusions related to the lightning NO impact on tropospheric column. The conclusions should be less affirmative.

Sec 3.4 Fig.14 clearly shows that adding simulation with lightning NO improves a bit comparison with observation (for West America). However the comparison between the model and observations are very low (Rˆ2) and it is hard to conclude on the effect of lightning NO on the deposition of nitrogen species.

Sec 3.5 I find this section the most interesting in the comparison and discussions between observations and model biases.

Minor comments

p 17704 line 10 " NASA OMI Aura Validation Data Center NO2 time series product ": please add reference p17704 line 23 " that include negatives " I don t understand the sentence, please clarify. What is the incidence of using a NO2 column product that excludes scenes with cloud fraction higher than 30% (NASA time series) and another with cloud fraction higher than 50% (DOMINO)? An equal threshold value should be used both for NO2 and O3 retrieval product for comparison with model simulations (50% as mentioned for O3 in p 17705 line 50%)

Sec 2.3 p 17707 Please describe the time period of the simulation (2006 and 2004). It is difficult to understand when start and end each simulation, whether or not a spin up is used.

Sec 2.3 p 17706 line 27 Please add reference for the Kain-Fritsch parameterization and the WRF model, and after for the BEIS inventory, SMOKE . Line 13-15 p 17707 " prior work has shown CMAQ underestimates " Which work? without lightning NO emissions? Please clarify and add reference.

References Please check references. There is a clear need to add references to men-

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tion previous scientific work and studies in the paper. Some of the references cited in the text do not appear in the Reference section.

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