

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 #3

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The authors have developed a “new lightning nitrogen oxide algorithm” in the CMAQ model and have used it to evaluate the impact of this source of NO_x on tropospheric photochemistry over the USA during summer 2004 and 2006.

I do not think this paper should be published in ACP unless major improvements are done.

Major comments:

The paper does not provide a substantial contribution to the research on the source of NO_x by lightning, even if this paper is certainly interesting for the CMAQ community.

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1)Indeed, the lightning–NO algorithm has already been introduced in CMAQ in Koo et al. (2010). Here, it only differs in that the emissions are scaled locally on a monthly basis using NLDN flash rates. The influence of this scaling on the results in term of NO₂ mixing ratios is not really discussed in the paper.

2)In addition, one goal of the paper (cf. Introduction) is to investigate whether introducing a source of NO from lightning in the CMAQ model would lead to better simulate NO₂ concentrations in the upper troposphere. Of course, by adding a source of NO in the upper troposphere, we expect a decrease in the bias between measurements and model.

3)Furthermore, the evaluation of model performances presented in the paper is not conclusive.

- For instance, the authors write that “Over the United States, LNO_x is responsible for 20%-25% of the tropospheric nitrogen dioxide (NO₂) column”. I do not think the authors can state this. Indeed, the CMAQ model underestimates NO₂ columns and NO₂ mixing ratios in the upper troposphere as shown by the comparison with OMI and aircrafts measurements. Furthermore, the authors do not take into account the source of NO by aircraft in their model. (In addition, owing to the non-linearity of the chemistry, comparing a simulation with NO from lightning and another that does not include NO from lightning, does not give the contribution of the lightning NO source to the NO_x field). I think the authors should change their title.

- The remark in the abstract “most of the differences between modeled and satellite retrieved urban to rural ratios are likely a consequence of the horizontal and vertical smoothing inherent in columns retrieved by OMI” should not be a result of the study. Indeed, it is expected. To overcome this problem, all the comparisons between model and satellite products should be done by taking into account the OMI averaging kernel. This is not always the case in the paper (figure 4 for instance). In addition, the authors use different OMI products, the reading of their features is a little bit tedious. It would

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be helpful to summarize the information in a table. I think that errors on OMI columns should be added and used in the discussion.

- By adding a source of NO from lightning, the authors scale up the ozone in the simulation of 2006 and increase the bias between the model and the observations. I agree that it is not necessarily due to the treatment of the lightning NO source in the model. But the authors do not investigate enough the reason for the discrepancies on ozone. They could give an estimation of the ozone bias in the fields (from GEMS and GEOS-CHEM) used for the boundary conditions. I would recommend improving the boundary conditions.

To conclude, I think the authors should find what we can really learn from their study and build the paper around that instead of presenting a list of not very conclusive results.

Possible ideas: Do you find any influence of lightning events in OMI data? Percent of NO₂ column due to lightning (roughly: LNO_x –noL) vs NO₂ column (LNO_x) (when OMI avgk applied) would give ideas on the influence of lightning on the OMI column and if it is larger than the OMI errors. If yes, you could maybe focus on events before generalizing to United States and the whole summer?

Specific comments:

Abstract:

P17701, L26 please state here which uncertainties in the chemistry you will investigate in the paper.

Introduction:

I think the introduction section should be better organized. The overall context and prior work with the CMAQ model are mixed. The goals of the study should be better defined.

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P17702, L25 TES stands for?

P17703, L25 we expect to better represent NO mixing ratios in the upper troposphere when NO from lightning are included (see major comments above).

Section 2.1:

P17704 L23, “negatives” could you clarify? Why do you exclude pixels with cloud fraction higher than 50% for the NASA dataset and pixels with cloud fraction higher than 30% for the other dataset? you would need to say how much this has an impact on the difference between the two NO₂ columns. You maybe need to say here how you compare NO₂ columns from CMAQ with the different NO₂ products from OMI. A table summarizing the features of the different OMI products would be helpful.

Section 2.2:

P17706, L1, change to “The TES instrument is an infrared Fourier transform spectrometer with a spectral resolution of 0.1 cm⁻¹ and a spectral range from 650-2250 cm⁻¹ (Beer et al. 2001)” You can mention Worden et al. (2007) along with Nassar et al. (2008).

Section 2.3: Could you give a reference for SMOKE version 2.6? Could you describe how long the simulations last, when they begin and end? Why the boundary conditions are constant in time for the 2004 simulation? This should introduce errors in your simulations. Please explain why it is acceptable to use these boundary conditions.

Section 2.3.1: Why do the authors do not scale the flashes to NLDN on a daily basis?

Section 2.3.2: The title should be changed; it is not the evaluation of LNO_x but the evaluation of Flash rates.

P17711, L1, these results are shown in the paper? what should we conclude from L1-L3 ? Simulated and observed daily variation of flash rate in summer do not agree, this has to be kept in mind when analyzing NO₂ columns.

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P17711, L27 what is this stronger synoptic forcing in 2004 ? the agreement is better in Aug 2004 but not in July 2004. Please could you clarify what we can learn from this section and would be important to understand the NO₂ comparisons?

Section 3.:

P17712, L7 can you explain why do you perform a simulation airLNO_x? Your simulation LNO_x do not have aircraft NO emissions?

Section 3.1:

Figure 4: I have a problem with this figure. You say that the comparison is not rigorous because you did not adjust the model output with the averaging kernel. I think you should not show this comparison between the model and OMI without adjusting the model results. You can not compare CMAQ and domino either because they not are at the same horizontal resolution, I understand that you did not map CMAQ for this figure onto the DP-GC grid.

Figure 5: why do you use the DOMINO product for this figure and not the other products?

P17714, L3 "Clearly, care must be taken when drawing conclusions with respect to biases between modeled and satellite-retrieved columns". I think you should remove this sentence and rigorously compare model and OMI products (please see comment on figure 4 and major comments).

Section 3.2:

P17716 | 23 I think it is of importance that you better understand the overestimation of the ozone in the model. Figure 9 shows that by adding NO from lightning you scale up the ozone in your simulations and increase the bias between the model and the observations.

P17718 L5 can you explain, why you think you can use fixed boundary conditions?

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Section 3.5: This section provides interesting results. But, it would be interesting to know how the HO_x in CMAQ compares with HO_x measured during the INTEX-A campaign.

Some of the references in the text are missing in the list.

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

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