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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of convective transport and lightning NO_x production over North America: dependence on cumulus parameterizations" *by* C. Zhao et al.

C. Zhao et al.

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Reviewer 1

General Comments:

(Q): The manuscript details the use of a 3-D regional CTM to study the impacts of two different convective schemes on upper tropospheric trace gas distributions. Model results are compared with both satellite observations and aircraft observations obtained during the INTEX-NA campaign to gain insight into the performance of the different convective parameterizations. This work addresses convective parameterizations, a key area of uncertainty in regional scale modeling. Very few papers have used chemical measurements to evaluate convective parameterizations though the impact of convec-

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Interactive Discussion

Discussion Paper

tive processes on trace gas distributions is well documented. In that respect, this work is very timely and should serve other modeling groups well as they seek additional observational constraints to better refine the representation of convective processes in large scale models. The discussion of lightning NOx production and its dependence on convective parameterization will also be of interest to readers of ACP and the use of the C2H6/C3H8 is very interesting with a number of potential applications. A strength of this paper is that the authors try to go beyond simply describing differences caused by the convective parameterizations and actually explain why some of these differences occur. Overall I think it is a very good paper that is suitable for publication in ACP. I do think there are areas where clarification or additional details are needed, particularly in the discussion of lightning NOx. Those are outlined below.

(A): We thank the reviewer for a detailed review. Both text and figures are revised as the reviewer suggested.

Technical Comments:

(Q): p. 2290, Lines 20-21 - "The model divergence on lightning NOx..." - I believe the authors are referring to the difference between the models using the two convective schemes, but because divergence can refer to a physical quantity, this needs to be clarified.

(A): Now we clarify the sentence in the abstract "The model divergence on lightning NOx mostly is above 12 km" as "Simulated lightning NOx production difference (due primarily to cloud top height difference) is mostly above 12 km"

(Q): p. 2291, Lines 20-22 - May want to add to this sentence that there remains a great deal of uncertainty over which parts of a flash are productive of NOx.

(A): Now we change the sentence "NOx is thought to be produced during the return stroke stage of a cloud-to-ground flash and the leader stage of an intra-cloud flash" to "NOx is thought to be produced during the return stroke stage of a cloud-to-ground

ACPD

9, S2017–S2022, 2009

Interactive Comment

flash and the leader stage of an intro-cloud flash but there remains a great deal of uncertainty in the mechanism of NOx production in lightning flashes".

(Q): p. 2292, Lines 5-6 - Should cite where the 1-20 Tg N/yr range comes from.

(A): Changed.

(Q): p. 2292, Lines 22-25 - "...the model difference can be attributed to mostly the difference of the convective parameterization..." Are all other aspects of the models identical (i.e. advection, other physical parameterizations, resolution) as it says in the previous sentence? If so, I think it would be clearer to say something like, "Because all other aspects of the models are identical, when compared to the convective transport and lightning NOx features measured during INTEX-NA, the model difference can be attributed solely to the difference of the convective parameterization scheme." If all other aspects are not the same, then a few sentences should be added discussing any other differences and why these are unlikely to produce differences as large as those produced by the different convection schemes.

(A): We changed the paper. Please see the response common comments.

(Q): p. 2293, Lines 11-13 - See previous comment. If all other aspects of the WRF and MM5 simulations are identical, change to, "Large changes are apparent ... when WRF fields are used in place of MM5 due to the difference in convection schemes between the two models."

(A): We changed the paper. Please see the response common comments.

(Q): p. 2294, Paragraph 2 - Because this study focuses on the differences in two convection schemes, it would be helpful to have a schematic showing how these schemes differ. For example, Figure 1 from Bian et al. (Tellus, 2006) gives a visual explanation of this type of difference for two different schemes. If adding a figure is not reasonable, I think it would help to add a few more sentences about the schemes and, in particular, the differences between them.

ACPD

9, S2017–S2022, 2009

Interactive Comment

(A): Now we add more details describing the KF-eta scheme in WRF and Grell schemes in MM5.

(Q): p. 2294, last paragraph - I think several aspects of the lightning NOx calculation need to be clarified. The first sentence states that, "The lightning NOx production rate is parameterized as a function of convective mass fluxes and convective available potential energy..." Is this the flash rate, which is often parameterized using CAPE and/or mass flux? Or is it the amount of NO produced per flash? It is unclear unless you read the Choi et al. (2005) article and could easily be clarified. The second sentence states that, "The cloud-to-ground lightning flash rates are parameterized on the basis of the National Lightning Detection Network (NLDN) observations..." Are these flash rates parameterized, meaning they are predicted using the NLDN dataset in some way? Or are the observations of CG flash rate used directly as input to the model? The use of the NLDN data needs to be clarified and any applicable equations should be given if they are part of a parameterization. I am also confused about the, "lightning NOx production rate." In the first sentence it states that this is parameterized, but in the last sentence, it states that this is set to 250 moles NO flash. How can both be true? I think the main problem here is that is unclear exactly what aspects of the lightning NOx production are set (either by assuming a single value in both simulations or using data as input) and what are parameterized (and dependent on the convective parameterization used). How are flash locations determined? This all needs to be stated much more clearly.

(A): Now we change the paragraph "The lightning NOx production rate is parameterized as a function ...250 moles NO/flash" to "The cloud-to-ground lightning flash rate is parameterized as a function of convective mass fluxes and CAPE on the basis of the observed cloud-to-ground lightning flashes by the National Lightning Detection Network (NLDN) in summer 2004 as described by Choi et al. [2005]. The parameterization ensures the dynamic consistency between simulated lightning NOx production and simualted convection events. The IC/CG flash ratio is calculated following Wang et al. [1998]. It is assumed that IC and CG flashes have the same energy [Ott et al., 2003; Choi et al., 2005]. Lightning NOx is distributed vertically following the mid-latitude profile by Pickering et al. [1998]. We set a NOx production rate of 250 moles NO per flash in this study through trial and error analysis such that model simulations are consistent with in situ and satellite observations. This production rate happens to agree with the value suggested by Schumann and Huntrieser [2007]."

(Q): p. 2297, Lines 7-9 - The discussion of Figure 1b states that, "While not that significant in pressure coordinates, the altitude difference is quite large..." It would be useful to show either a second y axis for altitude or, preferably, redo Figure 1b using a log scale for pressure so these differences are more readily apparent to the reader.

(A): Now we changed Figure 1b using a log scale for pressure.

(Q): p. 2297, Lines 24-25 - Would it be possible to show the comparison with INTEX-NA C3H8 observations?

(A): The comparison is now added in the Appendix.

(Q): p. 2299, Line 6 - Why is HNO3 produced by lightning not scavenged? Wouldn't this affect the comparison with the INTEX-NA observations? More details of the wet scavenging processes should be given, although if the comparison with the observations is not to be shown, I think the brief HNO3 discussion could be taken out entirely.

(A): We did not show the comparisons for HNO3 because the difference for them between the two models is within the variation of the measurements. We now include the comparison in the Appendix, so they are available to readers who are interested in the results. Lightning HNO3 is not scavenged because it takes time to oxidize lightning NOx to HNO3. In our model (as in most models), we assume that the conversion to HNO3 takes place in the outflow of convection, away from the convective scavenging region.

(Q): p. 2300, last paragraph - It seems that most of the difference in NOx between the

ACPD

9, S2017–S2022, 2009

Interactive Comment

two models comes from differences in IC flash rate which is estimated based on cloud top height. It would be useful to show maps or time series of IC/CG ratio for the two runs. Though no observations are available for comparison, these could be discussed in the context of Boccippio et al. (MWR, 2001) Figure 2 which gives an IC/CG ratio climatology. It seems that the excessive cloud-top heights produced by the MM5-REAM simulation have two effects - one is that they potentially result in an overestimation of the IC/CG ratio producing too much NOx and the other is that they place NOx too high in the cloud. It would be nice to compare simulations using identical IC/CG ratio to separate these two effects, but that may be beyond the scope of this study. I think at least showing the IC/CG ratios produced by the model as described above would be very valuable.

(A): The IC/CG flash ratio is one parameter that determines the total amount of lightning NOx production. The reviewer is correct that IC/CG flash ratio is higher in MM5-REAM than WRF-REAM and we described the ratios in the revision. Both the NOx production rate per flash and the ratio of the lightning NOx from IC and CG flashes are "tunable"; parameters for the lightning NOx source in model simulations. What we try to show in this work is that the more critical factor in 3-D model simulations is the vertical distribution of lightning NOx and its dependence on cloud top height. As we show in Figures 5 and 7, in situ observations during INTEX-A at 8-12 km do not provide critical constraints needed for the simulated lightning NOx profiles.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2289, 2009.

ACPD

9, S2017–S2022, 2009

Interactive Comment