

## ***Interactive comment on “Impact of convective transport and lightning NO<sub>x</sub> production over North America: dependence on cumulus parameterizations” by C. Zhao et al.***

### **Anonymous Referee #1**

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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 convective 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

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scale models. The discussion of lightning NO<sub>x</sub> production and its dependence on convective parameterization will also be of interest to readers of ACP and the use of the C<sub>2</sub>H<sub>6</sub>/C<sub>3</sub>H<sub>8</sub> 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 NO<sub>x</sub>. Those are outlined below.

p. 2290, Lines 20-21 - "The model divergence on lightning NO<sub>x</sub>..." - 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.

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 NO<sub>x</sub>.

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

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 NO<sub>x</sub> 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.

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

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fields are used in place of MM5 due to the difference in convection schemes between the two models."

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.

p. 2294, last paragraph - I think several aspects of the lightning NO<sub>x</sub> calculation need to be clarified. The first sentence states that, "The lightning NO<sub>x</sub> 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 NO<sub>x</sub> 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 NO<sub>x</sub> 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.

p. 2297, Lines 7-9 - The discussion of Figure 1b states that, "While not that significant

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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.

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

p. 2299, Line 6 - Why is HNO<sub>3</sub> 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 HNO<sub>3</sub> discussion could be taken out entirely.

p. 2300, last paragraph - It seems that most of the difference in NO<sub>x</sub> between the 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 NO<sub>x</sub> and the other is that they place NO<sub>x</sub> 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.

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

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