Atmos. Chem. Phys. Discuss., 9, S1999–S2002, 2009
www.atmos-chem-phys-discuss.net/9/S1999/2009/

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.

Received and published: 7 May 2009

Summary:

On behalf of the authors, I would like to thank the anonymous reviewers and editor for their constructive and helpful comments and suggestions on our paper. We first provide the response to the main issues, and follow with more detailed point-to-point responses.

Common Comments:

(1) We agree that we cannot quantitatively separate the effects of the dynamic core and cumulus parameterization, although qualitative analysis would indicate that the latter is

most significant. We therefore will deemphasize cumulus parameterization difference and suggest that both dynamic core and cumulus parameterization are important. In the paper, we also clearly indicate that the Grell scheme in WRF is not the same as the one we used in MM5. We believe there are good reasons to show the results from MM5-REAM. By comparing to WRF-REAM, we demonstrate how convective mass flow and cloud top height affect trace gas distributions in the upper troposphere. More importantly, we can discuss the constraints (or lack of) by existing observations. The transition from MM5 to WRF is voluntary and gradual. Documenting the problems in MM5 is useful for researchers who choose to continue to use MM5 and also for providing the basis for understanding better the papers published previously using MM5 (with the Grell scheme).

(2) We did not show the comparisons for C2H6, C3H8, NOx, and HNO3 in the paper because the results are similar to Hudman et al. [2007]. Now we included the comparisons in the Appendix of the paper.

(3) We updated the old references to the newer ones.

Reviewer 1:

(1) 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.

(2) 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

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constraints needed for the simulated lightning NOx profiles.

Reviewer 2:

(1) We use SPARC-99 option in SMOKE. We partitioned the lumped tracers from SMOKE to the species using the reported species ratios in the emission inventory. We only found >C4 alkanes in GEOS-CHEM are inconsistent with INTEX-A measurements. The ratio of the emissions of ethane and propane from GEOS-CHEM give a more reasonable comparison with INTEX-A measurements than that from SMOKE. The ethane/propane emission ratio was based on the study by Wang et al. [1998].

(2) We set a NOx production rate of 250 moles NO per flash in this study because we found that model simulations with this rate are more consistent with in situ and satellite observations (through trial and error). And also, the rate is consistent with that suggested by Schumann and Huntrieser [2007]. It is now clarified in the paper.

(3) We now use the unmerged hydrocarbon measurements (original canister analysis) for ethane and propane comparison. The results are the same.

Editor (Owen Cooper):

(1) Now we use NOx instead of NO2 in all the comparison with INTEX-NA, and we show the vertical profiles of NOx in the Appendix.

(2) The simulated O3 impact in this model is similar to Hudman et al. [2007]. So we will limit our comparison to previous studies on the lightning impact on the upper tropospheric NOx in the paper.

(3) Over Florida, MM5 simulated cloud top height is more consistent with observations than WRF. That may indicate that the entrainment and detrainment are overestimated in WRF with KF-eta over that region. We mentioned it in the paper now.

(4) All the references related to the INTEX-A measurements used in this study are cited and acknowledgement is added in the paper.

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Reference:

Hudman, et al.: Surface and lightning sources of nitrogen oxides over the United States:magnitudes, chemical evolution, and outflow, J. Geophys. Res., 112, D12S05, doi:10.1029/2006JD007912, 2007.

Wang, Y., Jacob, D. J., and Logan, J. A.: Global simulation of tropospheric O3-NOx-hydrocarbon chemistry: 1. Formulation, J. Geophys. Res., 103, 10713-10725, 1998.

Schumann, U. and H. Huntrieser: The global lightning-induced nitrogen oxides source, Atmos. Chem. Phys., 7, 3823-3907, 2007.

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