

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

Anonymous Referee #2

Received and published: 20 March 2009

Review of manuscript "Impact of convective transport and lightning NO_x production over North America: dependence on cumulus parameterizations" [ACPD 2008-0629] by C. Zhao, Y. Wang, Y. Choi, and T. Zeng.

Overview and Recommendation:

The information within this paper is original and somewhat useful to the regional and global photochemical modeling communities within the ACP readership. However, there are some clarifications and details (see major comments below) that should be

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addressed before publication.

Major comments:

Page 2292, paragraph, lines 11-26. a) A major premise in this article is that the convective parameterization, and not the differences within the WRF and MM5 formulations, is responsible for chemistry differences in the mid and upper troposphere. A clean comparison of convective parameterizations requires the same computational core. Your first sentence of this paragraph states how sensitive convective parameterizations are to the underlying meteorology. The last sentence tells the reader to ignore that fact, and trust your judgment, that convective parameterizations are the responsible for any differences.

b) The Grell et al. (2002) reference is the convective parameterization currently used within the WRF model. The Grell convection scheme used within MM5 (Choi et al., 2008a) is from a much older reference (1993). There are major differences between the two. Was the MM5 convection scheme updated to the newer reference? My suspicion is that the 1993 Grell scheme was used in this study, which makes this whole comparison rather useless to the community.

c) The reasoning behind using MM5/Grell versus WRF/Grell is the absence of a shallow convection scheme within WRF/Grell. There is no information or references pointing to the importance of shallow convection to regional scale meteorology or transport. I would submit that changes in the dynamic core (MM5 versus WRF) are much more important to convective transport than inclusion or absence of a shallow convection scheme.

Page 2293, lines 18-20 Please state the variables that are saved every 5 minutes, and why those particular variables are given higher temporal resolution. If someone were to redo your methodology they should know more details. One important issue that is missing from the paper is the distinction and relative importance between resolved transport and parameterized convection. The temporal resolution of meteoro-

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logical fields has an impact on this partitioning. Some justification needs to be given for your methodology, especially since there is a publicly available on-line version of WRF (WRF/Chem) that circumvents these issues associated with off-line models.

Page 2293, line 24 to page 2294, line3. The SMOKE emissions processor comes with 3 photochemistry options; SAPRC-99, CBM-IV or CBM-V. Which option did you choose and how did it interface with REAM chemistry? Why didnt you also use ethane and propane emissions from this inventory, since you state that GEOS-CHEM default emissions are inconsistent with INTEX-NA?

Page 2294, lines 19-24 There are some major differences between the treatment of lightning in this paper, and previous work that is not mentioned. The Cooper et al. (2006) paper uses an IC/CG ratio of 4.2, and 457 mole/flash of NO_x. How were the 250 moles NO/flash determined? This needs some clarification, and reference to previous lightning and NO_x source estimates.

Page 2295, lines 5-17 VOC measurements are from canister samples. Is there interpolation to put them on the 1-minute merged dataset?

Page 2297, line 24 to page 2298, line 25 , C₂H₆/C₃H₈ discussion a) State specifically which flights in the Singh et al. (2006) reference are used in this data. Also, Figure 3 should have some indication of the number of samples that went into the 1km bin determination of the observations.

b) Are the model results shown in Figure 3 for the times and locations of the aircraft sampling, or are these medians from the entire 2 month run over a pre-defined region?

c) Using C₂H₆/C₃H₈ ratios, rather than absolute C₃H₈ or C₂H₆ concentration comparisons with observations can hide model deficiencies and introduce compensating errors. One critical quantity is the background C₂H₆ concentration supplied upwind from the GEOS-Chem model. Another critical quantity is the emission ratio of C₂H₆/C₃H₈ over the U.S. in your simulations. Maybe background C₂H₆ is high by 50%, the models

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would match observations without convection, and WRF/REAM has too much convective transport. Though it is reassuring to see that both models are somewhat consistent when convection is turned off, comparisons of absolute values of C3H8 with the observations would provide needed credibility to your case, and give skeptics like myself some confidence that the ratios are meaningful, and are not just being used in order to mask some underlying inconsistencies.

Page 2299, lines 3-19, HNO₃ discussion Without supporting figures this discussion is hard to follow. Moreover, HNO₃ will depend on the assumed NO_x production rates from lightning. Since these are uncertain within a factor of 2, and the rain-out treatment is so crude and model-dependent, it seems this paragraph has little to offer in terms of useful information.

Page 2299, lines 18 to 26, page 2300 a) I am familiar with another SCHIAMACHY comparison over the eastern U.S. between June and August of 2004 (Kim et al., GRL, VOL. 33, L22812, doi:10.1029/2006GL027749, 2006). In that study the authors used a more restrictive criteria (cloud fraction < 15%) and found much higher occurrences of SCHIAMACHY data available for use (a minimum of 4 and a maximum of 10 samples) for any given location over the 3 month period. This number of 2 for a 2 month period seems too low, particularly since the criteria is looser.

b)The comparisons in figure 4 raises some obvious questions: what would Figure 4 look like with 457 mole NO/flash produced instead of 250? How accurate were cloud tops predicted by the 2 models on the 2 days with cloud free observations? Both models use the same vertical profile for NO_x injection, but wouldnt discrepancies be even larger if IC flashes were apportioned vertically according to cloud top?

Page 2301, lines 2 and 3, reference to Figure 5: a)The values of lightning NO_x near the surface should probably be mentioned. These appear significant, and should affect rural surface level O₃ values. Is this due to the Pickering (1998) profile assumed? Is MM5 NO_x at the surface so much higher because of the NO_x emission parameteriza-

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tion, or is it because convection is bringing lightning NO_x down to the surface more efficiently?

b) Only a suggestion: If surface O₃ is affected significantly, this would also have implications for previous lightning NO_x assessments on surface O₃ (Kaynak et. al., Atmos. Chem. Phys., 8, 5151-5159, 2008). It may be useful to at least qualitative state if your results (at the surface) are consistent with that reference.

Page 2301, line 25 to top of page 2302: a)The presentation within figure 7 used to make the relationship between increased NO_x and increased O₃ obscures or ignores a lot of useful information. While the first 3 panels (left to right) are appropriate and useful, the last panel on the right (differences) is hard to digest, and is only used to say that 20 ppbv of additional O₃ is possible. It seems more useful to show NO_y and O₃ differences over the entire domain (averaged over the 2 months) or vertical distributions of O₃ changes averaged over the same area as in Figure 5.

Page 2302, lines 19-20: Please be more specific about the area used to derive these percentages. Is this over the model domain, eastern U.S. or what?

Page 2302, lines 20-28, page 2303 lines 1-7: A simple figure would help the reader digest this information more easily. It is somewhat ambiguous, line 21 says up to 70% lightning NO_x contribution in the 8-12km region. But page 2303 lines 3-4 say 40%.

Pages 2303-2304, Conclusions: You cannot say that 2 convective schemes were compared. Output from two different models, using two different convective schemes were compared.

Minor comments:

Page 2292, line 5, GOES-CHEM model should be GEOS-CHEM

Page 2292 line 11, Grammar, sensitive with should be sensitive to

Page 2292 line 16, Grell et al. (2002) should be Grell and Dévényi (2002)

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Page 2294 line 1, spell out VISTAS acronym.

Page 2294, lines 8-12, and Figure 1b Do the fluxes for the KF-eta model include the detrainment/entrainment fluxes shown in the plot? This should be stated.

Page 2297, lines 28, also Page 2298, line 9: no reference to Wang and Zeng (2004) in reference list.

Page 2316, Figure 6. What are the holes in the 300-400 hPa cloudtop interval? Some explanation is needed.

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

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