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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Lightning NO_2 simulation over the Contiguous US and its effects on satellite NO_2 retrievals" *by* Qindan Zhu et al.

Anonymous Referee #1

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The Zhu et al. manuscript presents results of WRF-Chem simulations conducted for the DC3 and SEAC4RS experimental periods with two convective parameterizations, and lightning flash rates were predicted using a different scheme associated with each convective scheme. Lightning NOx (LNOx) was predicted and compared with a version of the BEHR OMI NO2 retrievals and with aircraft data from the two experiments. Results show improved prediction of flash rates with the Grell 3D convection and CAPE-PR lightning scheme compared with Kain-Fritsch convection and cloud-top-height lightning scheme. LNOx production of 500 moles per flash produces the best model comparison with the OMI retrievals, but the model produces an NO2/NOx ratio in the middle and upper troposphere that is smaller than observed by aircraft, suggesting that the production is really less than 500 moles/flash.

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Discussion paper

Here is my major issue with this manuscript: Throughout the manuscript the authors state that the lightning prediction is improved with use of CAPE-PR compared with CTH. However, the lightning schemes are run with different convective parameterizations which are going to produce different convective characteristics (locations, timing, frequency, amounts of precipitation, etc.). Therefore, they would need to run CAPE-PR with Kain Fritsch to truly be able to say that CAPE-PR is better. If making this additional model run is not possible, I would then suggest that throughout the paper the authors refer to CAPE-PR as Grell3D/CAPE-PR and refer to CTH as KF/CTH to reflect the fact that it is a combination of convection and lightning schemes that are producing the difference they see in lightning flash rates.

Specific comments: p. 2, line 11: give examples of near-filed analyses (e.g., Huntrieser et al. (several papers); Pollack et al., 2016). add another sentence: Near-field estimates of LNOx per flash have also been make through use of cloud-resolved models with LNOx production constrained by observed flashes and aircraft data from storm anvils (e.g., DeCaria et al., 2005; Ott et al., 2010; Cummings et al., 2013).

p. 3, line 4: flash count frequency distribution over time,....

p. 3, ine 9: need a reference for CAPE-PR here

p. 3, line 15: Provide the time periods that are being simulated here. The reader needs to know if 2012 emissions are appropriate. Otherwise, the reader doesn't learn the simulating periods until Section 2.3.

p. 4, line 16: the neutral buoyancy level

p. 4, lines 14-22: Won't the difference in flash rate between the two model runs be partially due to different convective parameterizations and partially due to different flash rates schemes? Here is where the authors either need to add another model run (KF with CAPE-PR) or start calling the two runs Grell3D/CAPE-PR and KF/CTH.

p. 4, line 29: was this detection efficiency value applied to teh ENTLN data? Flash

counts should be divided by 0.7.

p. 5, line 12: should the reference by 2019 instead of 2018? 2019 is the one with v3.0B in the title.

p. 5, line 15: what is "NASA tropopause temperature"? Is it from the MERRA-2 product?

p. 6, lines 14-15: But, how much of this improvement might be due to use of KF convection rather than Grell 3D convection? I don't think you can conclude that one lightning scheme is better than the other with these two simulations that use different convection schemes.

p. 8, lines 4-5: Need to point out that this is really only true for the CTH model runs for SEAC4RS. Both model and observations are very small in this layer for DC3.

p. 11, lines 4-5: Here again, this conclusion needs to be modified. See above.

p. 15, lines 1-2: Update Laughner and Cohen reference

Figure S1: Since LIS only observes a swath across this region for a few minutes each day, I assume that the ENTLN data are subsetted in time to match the LIS overpass times. Is this correct? If so, you need to say that in the caption. Or, if the ENTLN data are really for the entire day/night for all days, then the comparison is not valid. The c) panel of the figure does not look to be correct. Is it really ENTLN - LIS rather than LIS - ENTLN? I see some pixels where neither appears to be correct.

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