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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 #2

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A new lightning NOx parameterization was developed, and the model simulations were applied to compare to in situ NO2 observations during DC3 and SEAC4RS. The implications for OMI NO2 VCD retrievals were also analyzed. While I think the paper covers interesting areas of research, the results from this paper are in line with previous studies. It would be a stronger paper if more significant improvements in either modeling or analysis results were made.

Some of the discussion is rather odd. A few of the issues were raised in my initial review but no modifications were made to the paper. These issues are not complex but do require more in-depth thinking than what was given in the manuscript. I think that the uncertainties in lightning measurements and modeling, satellite measurements and retrievals, and in situ measurements should be clearly acknowledged.

(1) Lightning modeling uncertainty

I pointed out in the initial review that the abstract statement in line 11-12 on page 1 should be deleted. Comparing NO2 VCDs between two parameterizations is not meaningful because the amount of lightning NOx (LNOx) is a function of specified IC/CG ratio, NOx yield per flash, and the vertical distribution of LNOx. Values can be easily modified to produce similar LNOx values between the two parameterizations. For the same reason, line 12-13 on page 11 in the conclusion section should be deleted. The NOx production rate per (IC or CG) flash cannot be determined with available observations.

The abstract statement in line 5-6 on page 1 is based on model comparison with SEAC4RS data, when the lightning activity in the Southeast is relatively low. Unless it is a science objective, aircraft in missions like SEAC4RS usually flies in sunny days and steers away from thunderstorms. The 50% reduction is also only specific to the CAPE-PR parameterization in this paper and it offers little value to other lightning parameterizations.

It is not surprising that the CTH based parameterization doesn't work well. It's a poor choice in WRF-Chem, but I presume that it's a good reason to compare it to a new scheme. It would be better that other parameters, say those used by Allen et al. (2002) and more recently Luo et al. (2017), were used.

A third issue raised in the initial review is line 17-19 on page 4. Which convection scheme was used for the results presented later in the paper? Are there differences? I did not find the results comparing the two convective schemes. Zhao et al. (2009) compared MM5 Grell and WRF KF schemes and found large differences. My understanding is that the Grell scheme in WRF does not have the large bias in MM5. It should be discussed.

(2) Lighting measurement uncertainty

Section 2.2 described the ENTLN lightning data and its use in this paper. Figure 1 in the paper by Luo et al. (2017) compared the lightning distributions observed by ENTLN to NLDN. For the data they used, ENTLN had more IC and CG lightning flash rates than NLDN. The lower IC flash rates in NLDN are likely due to a lower detection efficiency. However, the CG flash rates in NLDN are also lower. As implied by Luo et al. (2017), the NLDN CG flash rate data have been the "gold standard" in previous LNOx studies over the US. The distributions of NLDN flash rates were also different from ENTLN data in that work. The uncertainties of ENTLN data should be acknowledged. It is another reason the two statements on lightning parametrization in the abstract (discussed above) are not robust science results and should be taken out. Some explanation is due for the reasons of not using NLDN data.

The comparison in Figure S1 is inadequate since it is only for one day only. LIS observations are mostly for IC flash rates and there are good reasons to believe that a CG flash produces more NOx than an IC flash (see the relevant discussion in Luo et al. (2017)).

(3) OMI retrieval uncertainties

The comparisons of Figures 4 and S2 can only be used as a qualitative not quantitative measure of lightning NOx in the model. For clean regions of the SE, the difference is on the order of 1x1015 molec cm-2. Considering that the OMI uncertainty is larger than 1.5x1015 molec cm-2, it is difficult to say which sensitivity simulation is quantitatively better. There are additional uncertainties in OMI retrievals including surface albedo, cloud, and background noise. With the uncertainties in mind, the difference among the lightning sensitivity simulations in Figure S3 is therefore insignificant. Appropriate discussion on the uncertainties of OMI retrievals and the implications for this paper should be included.

There is no description on how OMI data under high cloud-fraction conditions are treated. Those data cannot be used in the comparisons of Figures 4 and S2.

(4) Uncertainty of the upper tropospheric NO2/NOx ratio

In sections 4, the uncertainty of the upper tropospheric NO2/NOx ratio was discussed. This issue doesn't affect model comparison with in situ observations when NO measurements are available. Comparisons with in situ NO, O3, and JNO2 should be included with the discussion of Figure 2. To evaluate model lightning NOx simulations, using NO measurements will get around the uncertainty of NO2/NOx ratio. Therefore, the last statement of the conclusion section (line 13-14 on page 11) is inappropriate and should be removed.

This uncertainty affects the retrieval of OMI data only if the unknown interferences by other nitrates are insignificant. Furthermore, the uncertainties of OMI data may mask out the effects. More detailed discussion should be included.

Other comments:

(1) P. 4, Line 26-27, Eq. (2), Luo et al. (2017) used a formula of , what is the reason for not including the other terms? What are the reasons for not using UMF or CPR?

(2) P. 8, Figure 3, Is the large urban VCD change mostly due to Orlando? Figure 1 shows very little lightning activity in the other SE region.

(3) P. 9, Line 6-7, the 19% value should be compared to Zhao et al. (2009).

(4) Figures are hard to read in general.

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