

***Interactive comment on* “Evaluation of nitrogen oxides sources and sinks and ozone production in Colombia and surrounding areas” by Johannes G. M. Barten et al.**

Anonymous Referee #1

Received and published: 3 December 2019

Summary of review:

This paper is well written from a technical point of view and the science is solid, but I recommend this current manuscript be split into two (or maybe even three) papers. It appears that the authors are combining several complementary papers into a single manuscript. One that is evaluating the sources and sinks of NO₂ using WRF-Chem and OMI and its effects on ozone, one that is trying to resolve a discrepancy in PBL height, and perhaps even another on the role of lightning NO_x in the Amazonia.

In my opinion, the authors have two options here: 1) either significantly shorten Section 4 & Discussion and add a stronger focus on the SCM model or 2) to exclude Section 5

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entirely. I would prefer option 1, but I will leave that decision up to the authors. I also think option 1 better fits the scope of ACP.

I recommend publication, but only after the scope of the paper is narrowed.

Major comments:

Ln 260: I am uneasy with attributing the high model bias in the Amazonia to lightning NO_x, primarily because the boundary of your domain is fairly close to this region and that climatological winds are generally from the east. I do not think it is correct to automatically assume lightning NO_x is the reason for the discrepancy. I believe that boundary conditions could be playing a role here. Additionally, you may only be using <10 days of OMI data in the comparison (Figure 2), which is particularly an issue here since the NO₂ measurements are near the lower limit of the OMI's capability. In general, a long discussion on the lightning NO_x is not warranted because it is a small sample size near the instrument's detection limit. Please also revise the later parts of the manuscript when lightning NO_x is discussed.

Ln 295-298: After looking at Figure 6, I am confused how the authors are implying that there is good agreement between the modeled NO_x/O₃/CO and surface monitors at any hour. Perhaps I am misinterpreting something, but if not, these sentences should be modified.

Ln 354 How is the boundary layer constrained in the single column model? This seems to be key information, but it is left out. In general, Section 5 is lacking specifics. As emphasized above, I think this could be either a great follow-up paper or Section 4 should be shortened and this could be a larger focus of the paper.

Minor comments:

Ln 101: What initial conditions are used?

Ln 142: The words "on the large-scale" are probably unnecessary.

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Ln ~190: Silvern et al., 2018 should at least be mentioned at this point in the manuscript. It suggests that the NO₂/NO partitioning may not be good in the upper troposphere. The paper shows that NO₂ in upper atmosphere is often too low in global models. This is important when calculating the AMF and could affect it significantly when NO₂ is generally low such as the Amazonia region.

Silvern, R. F., Jacob, D. J., Travis, K. R., Sherwen, T., Evans, M. J., Cohen, R. C., Laughner, J. L., Hall, S. R., Ullmann, K., Crouse, J. D., Wennberg, P. O., Peischl, J., and Pollack, I. B.: Observed NO/NO₂ Ratios in the Upper Troposphere Imply Errors in NO-NO₂-O₃ Cycling Kinetics or an Unaccounted NO_x Reservoir, *Geophys. Res. Lett.*, 45, 4466–4474, <https://doi.org/10.1029/2018gl077728>, 2018.â€

Figure 2: Borders are hard to see. Perhaps change them to white? Also for clarity, perhaps change the values to % of the month instead of number of measurements.

Figure 4b: The units are unclear. Please clarify

Ln 250 & Ln 257: Insert the word "model" before "overestimation"

Ln 273: Is Figure 5d necessary? It does not seem to add any helpful information.

Ln 296: Should clarify to "morning rush hour"

Ln 359: Discussion section should be more concise.

Ln 376 - 390: I'm not sure how many overarching conclusions about lightning NO_x can be made from this study. I suggest this paragraph be removed.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, <https://doi.org/10.5194/acp-2019-781>, 2019.

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