

Interactive comment on “Quantifying burning efficiency in Megacities using NO₂ / CO ratio from the Tropospheric Monitoring Instrument (TROPOMI)” by Srijana Lama et al.

Anonymous Referee #2

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The paper presents NO_x/CO emission ratios as derived from satellite observations of NO₂ and CO. It demonstrates the high potential of TROPOMI for atmospheric research. The paper is generally well written. However, the details of the method lack some details, and the robustness of the results is hard to evaluate with the given information. Thus, before publication, major revisions are necessary.

The method description has to be extended. In particular, the two approaches have to be illustrated for real data rather than just for a schematic plot. In addition, the definitions of background and upwind areas have to be made consistent for all cities. If this is not possible, the authors have to discuss this in detail, motivate their choices, and

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add a discussion of uncertainties due to the a-priori settings. Error bars only account for statistical day-to-day variations, but ignore the impact of a-priori settings and the CER procedure.

Detailed comments:

- Line 117: Is this bias in NO₂ accounted for in your study? How would the results change if NO₂ would be scaled up accordingly?
- Table 1 settings: I am puzzled by the different definitions for different cities, especially as Line 146 states that the settings are "not critical". So why are they different at all? Do you need to tune the area definitions in order to get the right results??? How do the results look like if a consistent setup is chosen for all cities? Why is the upwind area for Riyadh different for dlat and dlon by a factor of 30?

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- the methods are explained in plain words, but not illustrated for real data. So Fig. S2 should be moved to the main text, and the background/upwind regions etc. should be marked in this plot. In addition, the ERA wind vector should be added. The rotated patterns and the percentiles used for the second approach should be provided in a separate figure.
- both methods compare columns "upwind" and "downwind" of the investigated cities. This approach requires that there *is* transport taking place. Wind speeds for Mexico City are quite low, as can also be seen in Fig. S2. So did you consider a minimum threshold for the wind speed? I expect that it would help to remove inconclusive days.
- Line 157: with Eqs 2&3, daily ratios are calculated. But how is the total ratio (shown in Fig. 3) derived? Is it the mean of all daily ratios? This is by definition different from the second approach, where first CO and NO₂ are averaged and then the ratio of means is calculated. Thus, also for approach 1, the ratio of means should be taken.
- Line 196: What emission database is used by CAMS? EDGAR? MACCity? Or

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something else? How far does this affect the following interpretation and discussion of CAMS OH? How do CAMS spatial patterns of CO and NO₂ compare to TROPOMI? Please provide a Figure in the Supplement. Is TROPOMI CO and/or NO₂ assimilated in CAMS?

- 2.6.1: The bootstrapping approach evaluates the statistical uncertainty for the results with the chosen approach. But on top, there are also other uncertainties, like systematic effects introduced by the definition of radii etc. In particular the uncertainties of the wind direction and wind speed have to be discussed as well.

- Figure 3: Please estimate the uncertainties of NO₂ lifetime and AK correction and provide error bars for the CER results as well. I expect that these uncertainties are far higher (and thus more relevant) than the purely statistical bootstrap uncertainties.

- 3.2ff: Please check the discussion and conclusions (a) for NO₂ probably being biased low and (b) according to the quality of CAMS emissions and the agreement between TROPOMI and CAMS

Minor comments:

- Lines 40-42: The references stating the high uncertainty of Chinese emissions are from a time period where development in China was vastly increasing. Meanwhile, NO_x emissions have been reduced, and the awareness of air pollution has increased in China. I would thus assume that these high uncertainties do not hold any longer.

- Lines 60-61 Please provide refs to SCIAMACHY (Bovensmann) and TROPOMI (Veefkind).

- Line 70: Should be NO_x emission.

- Line 75: Transport disperses NO₂ and CO similarly, but the lifetime of NO₂ is far shorter! See the different plume extents shown in Fig. S2.

- Line 117: Avoid misreading as "the bias is low", e.g. "NO₂ is biased low by about

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30%"...

- Table 1 lat/lon: Please provide consistent number of digits for lat/lon. $.01^\circ$ should be accurate enough.

- Fig. S1: I don't understand why there is a need for separating 4 different wind directions in the formalism; rotation matrix should work the same for all four cases!?

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-1112>, 2019.

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