

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## Comment on acp-2021-1051

Anonymous Referee #1

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Referee comment on "Quantifying NO<sub>x</sub> emissions in Egypt using TROPOMI observations" by Anthony Rey-Pommier et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-1051-RC1>, 2022

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In this interesting paper the authors use the continuity or mass closure equation to derive emissions from the TROPOMI NO<sub>2</sub> column observations. The authors discuss all the relevant ingredients of the calculation and provide estimates of the uncertainty. I am in favour of publishing, but with substantial revisions in response to a large number of questions provided below.

Why Egypt? I understand there are many cloud-free days over the desert. Does the method require entire regions to be cloud-free? Or could it be applied to France just as well?

Section 2.1: "We use TROPOMI NO<sub>2</sub> retrievals from November 2018 to November 2020". Please provide details. Which version (versions) is used?

Section 2.1: "TROPOMI sounding are gridded for this study at a spatial resolution of 0.1 × 0.1". The authors mention that the resolution of TROPOMI is 3.5 × 5.5 km. So the choice of the grid is a bit disappointing (11×11 km). Why choose this resolution and not a higher one? Please provide details of how the gridding is done. Is this conserving NO<sub>2</sub>?

Section 2.3: "Therefore, the CAMS OH concentrations are used". The resolution of CAMS is not very high, 0.4 degree. Given non-linearities and dependency on NO<sub>x</sub>, would the use of CAMS OH be a good choice? What are typical uncertainties, in particular those linked to the downscaling from 0.4 degree to 0.1 degree?

Section 2.4: "It is therefore necessary to remove the natural part of the atmospheric signal " We do not expect a lot of lightning and soil emissions over the desert. How large a signal is expected, why is removal needed, and how is this done?

Section 2.4: "We conduct this removal by subtracting the mean emissions over desert and rural areas from the mean emissions over urban and industrial areas. " Should "emissions" be "NO<sub>2</sub> tropospheric column concentrations" here? Later in the paper there is a background emission term introduced. Why are background corrections not applied to the concentrations?

Section 2.5: The CAMS emissions also seem to rely on EDGAR and will use similar approaches/assumptions and input datasets. Please comment on how independent or dependent these two datasets are.

Section 3.1, line 184: "Slant column densities are used as vertical densities" This does not make any sense to me, and should be a large and unnecessary source of uncertainty. The simplest approach to the air-mass factor would be a geometric path length of the incoming and outgoing light which depends on the viewing angles and is  $> 2.0$ . So, neglecting the air-mass factor can easily lead to 50% errors. Why is this better than using the air-mass factors from the retrieval?? Furthermore, the slant column will include (be dominated by) the stratosphere. Why not use the tropospheric column? As mentioned, the sink is modelled as concentration divided by lifetime. But this concentration should be the column in the lower troposphere only, otherwise it does not make sense?!

Equation 3: What is the  $\omega_{\text{NO}_2}$  in this formula. Is it the slant column from TROPOMI?

Section 3.2. The discussion focusses entirely on electricity consumption, motivating that 13:30 is representative for the daily mean. However, I would expect that traffic (industry) is also a major source of NO<sub>x</sub>, and this has a distinct diurnal (seasonal) pattern. So the discussion seems to be over-simplified.

Line 258: The city of Riyadh has been extensively discussed by Beirle et al., 2019. A reference to this paper in section 3.3 should be added.

Line 263:  $\sqrt{w^2} = w$ . The notation is a bit unclear.

Equation 7: I still have a conceptual difficulty with a "rural emission". Over the desert the estimated emission should be close to  $=0$  and negligible compared to urban emissions, otherwise the methodology is flawed.

line 324: "limit the high inter-day variability due to changing wind patterns or differences between week days and week-ends". What is the real reason averaging over a month is needed? Winds change, but if the method is correct the emissions should be equal

(assuming stationary sources).

I 359: "Level B is therefore the one that leads to the best match between the lifetime calculated with Equation (2) and the lifetime calculated from line densities." What does this really prove? Does it really mean Level B is better? Due to the coarse resolution we may expect CAMS is biased in OH since it does not resolve the plumes.

Figure 6: Before showing this, I would suggest the authors apply the method to Riyadh and compare with Beirle et al. (2019) to test the consistency of the results.

Table 1: I would suggest to replace "khab/km<sup>2</sup>" by "10<sup>3</sup>/km<sup>2</sup>"

I 420: "It is also observed that TROPOMI NO<sub>2</sub> column densities above this zone are relatively homogeneous" As demonstrated in several papers, there is a clear shipping signal in the TROPOMI data over oceans and seas, and I would expect TROPOMI to be rather inhomogeneous here?!

Figure 8: The unit is "kt" which I assume is 10<sup>6</sup> kg. But what is the time unit? Per hour, per day, per year? I'm a bit surprised by the big scatter for the weekly (daily) values averaged over the entire country?

Section 4.5, Covid-19. There is a nice review paper, <https://doi.org/10.1525/elementa.2021.00176>, which could be added here.

I 488: "no significant changes in OH concentrations ". Does the CAMS system describe the change in emissions and concentrations observed resulting from the lockdown? If not, how would this impact the results (given the non-linearity of the chemistry)?

I521: "TROPOMI-inferred emissions show an annual variability" I was wondering how much we can believe the seasonality in OH as modelled by CAMS? This seems to directly link to the seasonality of the sink term and, as a consequence, the emission estimate. Please discuss.

I551: "S-5P validation activities" Please add a reference

I 558: "For [OH]," The authors showed that OH is strongly height dependent, so it seems

that the choice of the vertical level is a major uncertainty. Has this been accounted for?

Data availability: TROPOMI data is missing here.