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Comment on acp-2021-1051

Anonymous Referee #2

Referee comment on "Quantifying NO_x emissions in Egypt using TROPOMI observations"
by Anthony Rey-Pommier et al., Atmos. Chem. Phys. Discuss.,
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Rey-Pommier and co-workers report on a framework aiming to quantify NO_x emissions in Egypt relying on slant TROPOMI NO₂ columns, wind fields from ECMWF ERA5 reanalyses, and temperature, OH, NO and NO₂ fields from CAMS. This top-down approach is based on the continuity equation in steady state, with the species concentration in an elemental volume being a result of flux transport in and out of the volume, the sources inside the volume from emissions and chemical production (e_{NO_x}), and the sinks inside the volume from chemical loss and deposition (s_{NO_x}). The reaction of NO₂+OH is considered as the only significant sink term in the calculations. The slant columns of NO₂ are used in lieu of the vertical columns in the continuity equation, leading to errors that are not well assessed in the manuscript. The top-down emissions in Egypt are found to be in good agreement with the CAMS-GLOB-ANT_v4.2 bottom-up inventory, except for substantial temporal variations in the TROPOMI-based emissions not found in the inventories. Those variations, in particular a pronounced minimum in the winter 2019/2020, are very implausible. Their proposed explanation in terms of variations in electricity consumption does not stand scrutiny. A more in-depth analysis is required to better understand the possible sources of error.

I cannot recommend publication at this stage due to the following concerns that need to be adequately addressed and the choices made well justified.

Comments:

- l. 142 and following: the "urban" pixels (>100 hab. km⁻²) are not all truly urban. Croplands in Egypt are located almost exclusively within the "urban" cells of Figure 1, whereas the non-urban pixels are mostly (semi-)desertic. Therefore I doubt that the removal of the non-anthropogenic part of the NO_x emissions discussed here makes any sense. Soil NO_x emissions are primarily located within the Nile delta and Nile Valley. Same holds for agricultural residue burning, a substantial source of pollution in Egypt (<https://egyptindependent.com/agricultural-burning-clouding-sky-sickness/>).

- l. 174-184 : Equation (1) does not make much sense if slant column densities (SCD) are used as vertical column densities (VCD). The air mass factor (AMF) is generally different from unity, and furthermore, it varies in time and space. Contrary to the assumptions made here (Section 4.7), the albedo over the region is not uniform since we have areas covered by deserts, by crops, by water and by cities. The vertical profiles of NO_2 can also be expected to vary according to the landscape. Those variations will impact the divergence term in Equation (1). The authors state that the AMF is taken "into account in the final uncertainty estimates". However, Section 4.7 only discusses the VCD uncertainty due to the AMF, ($\sim 30\%$ following Boersma et al., 2004). This does not say anything about the impact of using SCD instead of VCD in Equation (1). I am worried that the real impact of this substitution is unknown.

- l. 193 I don't understand "using a temperature-dependent analytical formula for different pressure ranges". Burkholder et al. provides a general expression of the rate as function of both T and [M]. Please clarify.

- l. 196-197 I don't understand "The value of k_{mean} therefore represents the total loss of NO_2 due to OH and cannot be used to infer HNO_3 and HOONO production". This is not clear. Only the first channel is a true NO_x sink, therefore the other channel should be ignored entirely.

- l. 211-212 "Losses due to deposition and the formation of (...) nitrates are thus considered insignificant in Egypt where the forest cover is totally negligible": this is not correct. Forest cover might indeed be very low, but vegetation (mostly croplands) is present in the so-called "urban cells" of Figure 1 (leaf area index typically between 1 and 2 according to MODIS). Furthermore, TROPOMI HCHO maps show HCHO vertical columns over the Nile Valley and the Delta ($>1 \text{ Pmolec cm}^{-2}$) in summer, which are similar to values found in Southern Europe. This suggests significant NMVOC emissions, of biogenic and/or anthropogenic origin. Organic (peroxy)nitrate formation cannot be assumed to be negligible. There is very likely a significant net export of RONO2 and PAN compounds from the Nile area to the surrounding regions. A comprehensive model might be needed to evaluate its importance. Quite importantly, this export might be seasonally dependent, since organic nitrate formation is strongest in summer.

- l. 213 Regarding the HNO_3 -forming channel of the $\text{NO} + \text{HO}_2$ reaction, note that field studies (e.g. Nault et al. 2015, doi:10.1021/acs.jpca.5b07824) indicated that this path is very minor.

- l. 216-217 Production of PAN might peak in the late afternoon, but it might still be significant earlier in the day.

- l. 221-228 Why is electricity consumption assumed to be the best proxy for NO_x emissions? Traffic and industry follow different patterns. According to current inventories, what are the respective relative contributions of the main sectors (traffic, industry, power

generation) in Egypt? Some discussion is needed.

- I. 258 "We therefore use the nearby city of Riyadh (...) to perform the comparison between the CAMS-induced lifetime and the fit-induced lifetime": despite some similarities, Riyadh and the Nile valley are quite different environments, with much more vegetation and NMVOC emissions in Egypt than around Riyadh, possibly impacting e.g. the wind profile, the OH fields and the NO₂ profile. Therefore, the OH validation for Riyadh might be of limited value for Egypt. I recommend comparing the TROPOMI HCHO columns over Egypt and Riyadh.

- Equation (7): As explained above, the "urban cells" do contain natural emissions. Moreover, the non-urban cells contain anthropogenic emissions. Those are less intense than in the Nile Valley and delta, but the natural emissions follow the same pattern. I recommend to drop this separation.

- I. 389-406: What is the location and capacity of the main power plants in Egypt? Are the industries mentioned in the text (e.g. cement plants) really strong NO_x emitters?

- Section 4.6 The temporal variation of TROPOMI-based emissions seems very unrealistic. The 2019/2020 winter minimum would be explained by reduced electricity consumption due to reduced usage of air conditioning. But then why not in the previous winter? In any case, it should be possible to check whether the biggest power plants are the places where the seasonal behavior is most pronounced. And regions without any significant power plants should not exhibit this phenomenon at all. I doubt very much that air conditioning would increase so much the traffic-related NO_x emissions. I think much more plausible that the temporal variations are due to errors in the methodology, in particular regarding NO_x sinks and the air mass factors. I recommend to check whether the TROPOMI AMF (or an AMF recomputed using CAMS profiles) presents significant temporal variations. This requires a more in-depth analysis than is currently provided.

- Section 4.7 Regarding uncertainties, as noted above, I doubt that NO₂+OH is really the only relevant NO_x sink in the area. Furthermore, the impact of ignoring the AMF in Equation (3) should be assessed. The uncertainty of only 1 m/s for the wind components seems optimistic since the precise altitude at which the wind is interpolated is arbitrary, and the Coburn et al. study concerns the U.S. which is likely better characterized in the CAMS model.

- I. 239 Burkholer -> Burkholder

- I. 378 "towards in the northeast and southeast quadrants": unclear. Do you mean towards the northeast in summer and southeast in winter?

- l. 666 : I could not find Huijnen et al. 2016, please provide URL