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Comment on "Exploiting satellite measurements to reduce uncertainties in UK bottom-up NOx emission estimates"

Anonymous Referee #1

Referee comment on "Exploiting satellite measurements to explore uncertainties in UK bottom-up NO_x emission estimates" by Richard J. Pope et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-583-RC1, 2021

The paper presents satellite measurements of NO2 from OMI and TROPOMI for UK, and estimates NOx lifetimes, emissions, and trends. The goal, as stated in the title, is to reduce uncertainties in bottom-up emission inventories.

While the topic is generally of high interest and would match the scope of ACP, I recommend to reject the paper due to methodological flaws and shortcomings, and as I do not see the claim of the title supported by robust results.

Below I discuss the four result sections in detail.

3.1 NOx sources

Spatial correlation between major NOx emission sources and TCNO2 could be already seen in GOME data in 1996 – this is neither new nor surprising. But as surface emissions and TCNO2 are different quantities (as stated by the author), it is hard to derive any quantitative information from this. For me the conclusion of Fig. 3 would be that there is no obvious discrepancy between the spatial distribution of NOx sources in NAEI and TCNO2, but I don’t see how far this comparison might help to improve the bottom-up inventories.

3.2 Trends

The authors compare trends in NOx emissions and TCNO2 for 2005-2015. However, the row anomaly in OMI started already earlier than 2015 and potentially degrades the OMI timeseries in a nonlinear way. Thus, I do not consider the presented trend estimates from OMI TCNO2 to be robust.

The discussed trends in AURN for urban/background/rural sites might be related to nonlinear chemistry. But they might also be just related to different emission trends for different sectors or regions, which is not resolved in the presented NAEI data (though the information would be available, I assume).

3.3 Top-down emissions

The authors state that they follow a simple approach in order to derive NOx emissions from TCNO2, and apply a formalism modified from similar methods in literature. While I see the potential of the general approach for detecting and quantifying NOx emissions from megacities on global scale, I consider it quite challenging to quantify NOx emissions of smaller cities with an accuracy good enough to be of use for improving bottom-up inventories of such high quality as NAEI.

For the detailed implementation, I see the following shortcomings:

(a) I don't understand the justification for the method used for determining the background. Why should the intersect of the wind-flow LD with the all-flow LD be a "reasonable estimate" for this? As shown in Fig. 1 b, wind-flow and all-flow LDs are complex curves with several minima and maxima. Searching for intersects just yields a number of quite arbitrarily distributed points. Why is B not just included as additional parameter in the fit?

(b) Eq. 1 is a rough simplification; it does not account for the spatial extent of the city, and it completely ignores additional emissions. In particular for cities like Manchester, with several smaller cities in the East, this will affect the lifetime estimate for the westerly wind case, and thus also the estimated emissions are affected.

(c) Seasonal effects (on emissions and lifetimes) are not discussed at all.

(d) Some a-priori choices (like for the "source width") are not provided in the text, and uncertainties for these choices (like selection of pressure level for wind data) are not discussed.

3.4 Comparison to GEOS-Chem

The authors compare TROPOMI TCNO2 for 2019 with a model run based on 2016 emissions. The emissions have been adjusted to 2019 using the linear trend derived from the temporal evolution of NAEI total emissions 2009-2018. However, this approach does not account for sector- or region specific changes. For instance, it could be that the decrease is mostly due to emission reduction in traffic sector (thus mostly affecting London emissions), while rural emissions stay more or less constant. Thus, the spatial patterns shown in Fig. 6 might actually be also caused by a change of emissions in 2019 compared to 2016 which cannot be just described by an overall linear decrease.

Overall, the authors derive emission estimates and trends from satellite observations of NO2 based on rather simple assumptions, and compare those to bottom-up inventories. Despite the methodological shortcomings listed above, the authors find rather good agreement to NAEI bottom up emissions. Basic conclusions, however, are in essence that no disagreement was found.

But even with modified/improved methodology, I doubt that the approach presented in this study, applied to 1 year of TROPOMI data, would allow to actually improve a bottom-up inventory for the UK. For this, I would expect that more refined methods and consideration of longer time periods would be necessary.

Additional comments:

- the maps of UK have a strange aspect ratio; it seems to me that the projection was trying to correct for the latitude grid being shorter than the longitude grid at 53°N, but applied the correction in the wrong direction and stretched the latitude aspect instead of squeezing it.

- figures are hard to read, in particular Fig, 3 a-d.