Review of the manuscript: “Estimating the variability of NOx emissions from Wuhan with TROPOMI NO2 data during 2018 to 2023”

General comments

The manuscript employs the superposition column model (previously published in literature) in combination with TROPOMI tropospheric NO2 column data to estimate city-scale NOx emissions and lifetimes and their variabilities. The paper is an extension of a previous work from the same (almost) authors covering a longer period, which allows for the study of the seasonal, weekly and interannual variability. Overall, the manuscript is well written, but in my opinion, there are some parts of the methodology that requires clarification. I suggest publications if the following issues are properly addressed:

Specific comments

Methodology (Sect 2.3): The explicit definition of lifetime and of the final emission E appears to be missing.

L164 “The terms Ei, k, and α are fitted” what about the background coefficient b?

L164- Concerning the OH concentrations, if I understand properly, you use that information to constrain the fitted k coefficient. Is this needed to obtain a “good fit”? Is this worth running a full CTM? What would happen if you let the fit run free (or set a reasonable fixed range), so that you would be not dependent on CTM outputs? What is the variability of the monthly OH? I suppose that if it changes a lot, it makes sense to have a dynamic initial guess, but could you discuss more your choices in this regard? I ask this because, you are making a case for data-driven emission estimation methods, but you still need model data to make your method work. This should be mentioned, I think.

L171 “We restrict the emissions to a gaussian shape” It is not clear how you do that, could you clarify?

L171-172 “a scale factor is applied to the emission term. It is found to be ~0.1 for all the days that lead to the best fit of the NO2 line densities.”
It is not clear where this number comes from: what do you mean with “best fit”? Also, does this mean that you are minimizing the difference between your estimates and the inventory? This sounds strange if you then evaluate your estimates against the same inventory. Can you clarify?

L183 “We also exclude the days with estimated NOx emissions beyond 0.5-1.5 times the ABACAS bottom-up emissions.” Why do you exactly do that? I read your reasoning concerning the uncertainty and the seasonal variability, but I think you could include also “bad” results as well or at least provide some statistics about them. How many of such days are there? What are the possible reasons for disagreements?

L254 “Their much lower summer-to-winter emission ratio may be caused by much lower

estimated summertime NOx emissions or much higher winter emissions or both." This sentence is maybe a bit self-evident. Are there any specific difference to be mentioned here?

L271-273 "In this work, the a priori NOx emissions are used to restrict the computation of NOx emissions. Thereby, we have partly avoided the possible underestimation of NOx emissions." This is again what might be problematic. If you restrict the computation of the emission to the a priori inventory-based information, is it right to verify your estimates against those same emission inventory values? And, in general, if you need a good bottom-up inventory for your method to perform well, what is the added value of the satellite-based estimates? What would happen without that emission term in the cost function?

L387 "the difference is only 4.7% compared to the ABACAS inventory." Again, the satellite-based emissions are limited to remain close to the ABACAS inventory, so a smaller difference is expected.

Conclusions: you could more thoroughly comment on the limitations of the method, such as the dependence on CTM data and on bottom-up emission inventory data.

Technical corrections

Abstract: TROPOMI should be defined

L39 you should probably add a more general (maybe also older) references to this first statement.

L57 It should be noted that the superposition column model presented here is also dependent on CTM (via OH), so it does not solve the issue of running such complex models.

L58 Beirle et al. (2011) actually do not use plume rotation, but they separate the data in 8 classes based on wind direction and then fit the EMG function. Rotation and EMG together were used for example by Lu et al. (2015) among many others. https://acp.copernicus.org/articles/15/10367/2015/

L60 Empirical Modified Gaussian model (EMG) -> this is actually Exponentially-Modified Gaussian model

L62 applied (... -> this is not a complete reference list, add e.g. at the beginning of the references

L91 10-15% there is tilde instead of a dash line here.

L144-145 "rotate the grid map toward the mean wind direction" I would avoid the word rotation here as plume rotation is often used to indicate another method (e.g. Fioletov et

al. 2017). This is actually just a resampling to a grid aligned with the wind direction as you properly described in the caption of Fig. 1.

Fig. 1 panel a: in the title: origional -> original

L114-124
Does it mean that you only directly use GEOS-CHEM data for the initial value of [OH]? Maybe you could clarify this a bit.

L191-192 “There are least valid days in winter (December to February) after spring (March to May) for the cloudy and polluted conditions in winter.” not sure what you mean here, could be “There are least valid days in winter (December to February) due to the cloudy and polluted conditions.”

L240 To verify this, it would be useful to check some traffic data in the city, if publicly available.

L242 Add references here.

L299 “under 2022” you mean as compared to or lower than 2022?

L344 “It has a small influence (less than 1% in Wuhan’s case) on the overall estimation of city NOx emissions, for the days with fast wind make up only less than 10% of the total number of days.” The grammar here is a bit off, please rephrase.

L415 “The Wind fields” the world wind should not start with capital letter.