

Atmos. Chem. Phys. Discuss., referee comment RC2
<https://doi.org/10.5194/acp-2022-161-RC2>, 2022
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Comment on acp-2022-161

Anonymous Referee #2

Referee comment on "Improving NO_x emission estimates in Beijing using network observations and a perturbed emissions ensemble" by Le Yuan et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-161-RC2>, 2022

Review of Yuan et al. 2022 (acp-2022-161)

This paper presents a method for improving emissions estimates using a network of observations in Beijing. Emissions inventories are a major source of uncertainty in air quality modelling and reducing this uncertainty is important for public health policy. This work makes a valuable contribution to improving emissions estimates in Beijing and outlines a method which can be used in other regions. The study shows how data from a network of low-cost sensors yields results consistent to that obtained using the long-term air quality network data, meaning this method could be applied to areas without an existing air quality monitoring network.

I enjoyed reading this work. It is a well written, interesting paper with a detailed discussion of results and comparison to other studies. This work is within scope for ACP and I'd recommend this manuscript be published after addressing the comments below.

Specific Comments

-I found the title a little ambiguous. Consider changing from 'Improving NO_x emissions...' to 'Improving NO_x emissions estimates...'

-Line 115 – what height were the SNAQ sensors deployed at?

-More details should be provided about the 'elicitation of expert knowledge' process. How many people were consulted? How did you select experts? Did you design a questionnaire which was sent to people? If so, could you include a copy of this questionnaire in the

supplementary information?

-Lines 135-136. Does this imply all profiles are the same for all pollutants in the inventory? Or is there a different diurnal, monthly and vertical profile for each pollutant. Please make this clearer.

-Line 139. The authors describe the area that the base emissions cover in the text, but it would be helpful to visualise this with a figure. Could the authors include a map of the base emissions (total or by source sector) to show the overlap with monitoring sites? This could be overlaid in Fig. 1 or included as a new figure in the supplementary information.

-Lines 206-210. I didn't understand why CO was perturbed in the model if it is treated as inert and will not affect NO_x concentrations? Please add some lines to clarify why this was done.

-Section 2.3 – More details should be given about the model set-up. What was the spatial resolution of the model? The text says a 'high resolution' model was used but this is vague. Was the resolution the same as that of the base emissions – 3 km x 3 km?

-I am led to believe that the model is run for the whole year of 2016 but this isn't clearly stated anywhere when describing the model set-up. Please add some text to make this clearer.

-Figs 4 and 5. I recommend that the authors add a 'site type' label next to each group of names. I appreciate that the colour coding is described in the figure caption but a label would make it easier for a reader to interpret the figure.

- There is no discussion about any seasonal variation in the agreement between the model and the base emissions which would be interesting to see in the results and discussion. Was this investigated and if so, could some details be added?

Technical Comments

-Table 1- Footnote says that nighttime fraction is given as a percentage but I think the table gives it as a ratio.

-Line 422 – in-text description of Fig. 7 before describing Fig. 7(f) would improve readability.

-Line 496 – add 'were' to sentence ...NO₂ concentrations **were** in much...

-Line 497 - remove 'were' after observations