

Atmos. Chem. Phys. Discuss., referee comment RC2  
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## Comment on acp-2022-435

Anonymous Referee #2

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Referee comment on "Inferring and evaluating satellite-based constraints on NO<sub>x</sub> emissions estimates in air quality simulations" by James D. East et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-435-RC2>, 2022

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The authors developed an inverse modeling framework to infer NO<sub>x</sub> emissions by assimilating satellite observations. The highlight of the method is to separate the surface and lightning emissions contributions. However, I don't fully understand how the separation has been achieved by reading section 2.5. Additionally, the writing may need improvement. I recommend careful copy editing of the manuscript.

General comments:

- CMAQ model does not include stratosphere simulations. Will this contribute to uncertainties in the inferred emissions? I recommend adding some discussion about this.
- Section 2.1. Fig 2. The OMI/TROPOMI ratio is larger than 2 for large areas. I fully understand that it is time-consuming to integrate the TROPOMI data into the system and thus the authors did not update it. But the tremendous differences between those two datasets as shown in Fig 2 make me worried about the uncertainties associated with the usage of the early version of TROPOMI data. I suggest at least running the system with updated TROPOMI data for a short period, like a month, to have an understanding of the uncertainties.
- Section 2.4. Lamsal et al use OMI NO<sub>2</sub> to calculate delta omega and omega. In this work, the authors propose to use CMAQ simulations for the calculation alternatively. What is the advantage of using CMAQ simulations compared to satellite NO<sub>2</sub> observations?
- Section 2.5. Do the authors treat all changes in NO<sub>2</sub> as lightning emissions changes for non-populated areas? Do anthropogenic emissions only cover populated areas? If so, I would recommend clarifying and pointing this out in the abstract and conclusion.
- Is there any specific reason to use both OMI and TROPOMI? It seems the system works well by using a single instrument. I recommend clarifying the pro of using two instruments.

Specific comments:

- line 20. Inferred from?
- Line 22. Smaller bias?
- Line 24. Improve performance or reduce bias?
- line 25. This sentence is the conclusion sentence of the abstract, but it looks lengthy and unclear to me. I recommend rephrasing this sentence substantially.
- Line 33. It is more common to use "global inventory".
- Line 42. Why is the bottom-up inventory incomplete?
- Line 42-43. Are you indicating that large uncertainties in emissions estimates for developing countries will propagate into that for developed countries? Please try to rephrase the sentence here. The current meaning is unclear to me.
- Line 48. What is the definition of detailed emissions updates?
- Line 57. What is the emissions smearing effect?
- Line 59. Do the authors indicate that averaging a few observations will reduce the biases significantly? I assume the averaging will help to reduce noise, but not systematic bias.
- Line 73. Please try to briefly explain the reason why assimilation allows for minimizing influence from the upper troposphere before claiming it. It is also not clear to me how it will be an extension of the work of Lamsal et al.
- Line 83. I rarely see the term "surface-based observations". I suggest a more common-used term of ground observations.
- Fig 1. Caption. I don't quite get the meaning of "the boundary of the inversion algorithm".
- Line 118. Improvement of the bias?
- Line 122. Results should not be capitalized.
- Line 138. What is representative-day?
- Sect 2.2. What is the spatial resolution of the CMAQ simulation?