

Interactive comment on “Evaluation of modelling NO₂ concentrations driven by satellite-derived and bottom-up emission inventories using in-situ measurements over China” by Fei Liu et al.

Anonymous Referee #1

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This paper compares the modelling surface NO₂ concentrations from a regional CTM CHIMERE driven by the satellite-derived DECSO and the bottom-up MIX emission inventories, against in-situ measurements over China. The detailed evaluation is well demonstrated, and the model performance seems sound. Nonetheless, this work lacks of impressive new findings or insights. Substantial improvements are needed before publication.

Major comments

1) Pg4, Ln26: The authors used the MIX 2010 data, and declared that it was suitable for the NOx simulations spanning over the year of 2015, as DECSO NOx emissions are

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highly similar in year 2010 and 2015. This reasoning is not rigorous, if this work focuses on the comparisons of different emission inventories. Would the NOx emissions in bottom-up inventories also be highly similar in year 2010 and 2015? Furthermore, how about the changes in anthropogenic emissions of SO₂, CO, VOCs and other reactive species between 2010 and 2015 both in DECSO and MIX? Both the differences in emissions for NOx (reactive species) and other species (e.g., CO, VOCs) between DECSO and MIX should impact the concentrations of NO₂ through interfering with gas phase oxidation, particles through aerosol-radiation interactions and heterogeneous oxidation of NOx).

2) I am not sure whether this work focuses primarily on the comparisons of different emission inventories? If yes, the strengths and limitations of satellite-derived DECSO and the bottom-up MIX emission inventories have not been well discussed. Which emission inventory is better and recommended? Would the evaluation results change if a different model or chemical mechanism is used?

3) This paper made considerable efforts in classification of a total of 1413 air quality monitoring sites (e.g., urban sites, suburban sites, and sites share a grid cell). This information might not be that important, and should be presented briefly (or put into supporting information). A 0.25° resolution is a bit coarse in terms of mesoscale air quality simulations. If more sites appear in one cell, the errors would counteract. Would the evaluation results change if a different resolution is used?

4) Table 2 in Pg19: DECSO and MIX underestimated and overestimated the observed NOx, respectively. Then the corrected DECSO, scaled from the original DECSO using the ratio of MIX NOx to DECSO NOx over the simulation domain, simulated better results. What is the point in corrected DECSO (quite arbitrary) and its evaluation statistics? Note that in Figure 2, NOx emissions from MIX and DECSO show large discrepancies in spatial distribution especially in North China Plain.

5) Description regarding the model configuration (especially the gas phase mecha-

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nism) should be added.

Minor comments

1) Pg4, Ln3: the speciation of NOx needs further discussion. The recommended ratios are from Generation of European Emission Data for Episodes (GENEMIS), then their applicability over China? How would these ratios impact the NOx simulations?

2) Pg5, Ln16: the reviewer also uses the data from air quality monitoring sites of the MEP network. How to treat the monitoring sites with abundant and even overwhelming missing data?

3) Table 2, add the unit for RMSE. Please add NME.

4) Figure 10, please add “LST” or “UTC”.

5) Pg11, Ln3: the data source for the daily profile of NOx emissions.

6) Pg11, Ln6, and also in the Abstract: discussions regarding the boundary layer mixing need more verification (figures or tables).

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