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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Intercomparison of NO_x emission inventories over East Asia" *by* Jieying Ding et al.

Anonymous Referee #1

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General comments This paper presents a comparison of nine NOx emission inventories for East Asia including five bottom-up and four top-down inventories constrained by satellite observations, shows large uncertainties associated with their spatial and temporal variabilities, and suggests a few important issues for future development of an emission inventory in this region. The paper is generally well written but can be improved significantly after a careful editing.

I believe that this kind of intercomparison study for emission inventories in East Asia has not extensively been conducted yet. In this regard, this manuscript includes interesting and important results, which are worthy of publication. However, I have a major concern in the analysis used in this paper, which prevents me from recommending acceptance with ACP at the present form, and I will elaborate it below.

As indicated in the manuscript, some inventories include both anthropogenic and natural sources, but some do not have natural sources such as soil, lightning, and biomass burning, which have large seasonal and inter-annual variabilities. Without carefully segregating natural source contributions to the inventories, a simple comparison of total values among the inventories may cause a serious misunderstanding in their quality. I believe that all bottom-up inventories have sectoral emissions. The top-down emissions with CTMs may have a difficulty to separate individual sources but at least could separate anthropogenic versus natural sources because they have different seasonal variation. Therefore, I suggest that authors should compare inventories including anthropogenic sources alone and then go on to do the similar analysis for natural sources separately if they can.

Specific comments 1) Page 1, line 30, - I have a hard time to agree with this sentence because it does not appear that they show good agreement in total values.

2) Page 2, line 1, - There must be some typos here.

3) Section 2 – I believe that each inventory typically has a base year from which it projects values for other years based on some proxy data. If there is available information on this, please state it in the manuscript.

4) Page 6, line 15 – CHIMERE has a top layer at 500 hPa, which is too low to account for lightning NOx emission. So in the inverse modeling with CHIMERE, how would lightning NOx contribution to the observations be taken into account? I would assume that a climatological partial column would not change with time.

5) Page 6, line 28 – Obviously, there is a difference in the pixel sizes of satellite observations, which also differ from the model resolution. A detailed information on this would be necessary in the manuscript. How would this difference cause a discrepancy in the DECSO data with each satellite observation?

6) Page 7, line 35 – revision is required for clarity.

7) Page 8, line 10 – Could you explain the reason for the discrepancy here?

8) Page 9, line 30 – How would the difference in the local overpass time and pixel size make a difference in the seasonality of inferred emissions? It is not clear to me at all.

9) Page 10, line 29 – “Biases” would not be appropriate because we don’t know the true. I would suggest to use “differences” instead.

10) Page 10, line 32 – How about lightning?

11) Page 11, line 7 – It appears to me the same as in Figure 7.

12) Page 14, line 21 – It makes me wonder if two simulated NO2 concentrations also show a similar or greater magnitude of differences as shown in the top-down emissions.

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