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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Improving NO_2 and ozone simulations through global NO_x emission inversions" *by* Zhen Qu et al.

Anonymous Referee #2

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This manuscript has presented top-down estimates of global NOx emissions using two OMI satellite NO2 products over 2005-2016 and using the GEOS-Chem adjoint inversion method. Considerable differences are found between the two top-down emission estimates. Implementing the top-down NOx emissions to the GEOS-Chem atmospheric chemistry model shows some improvements on the model simulation of tropospheric ozone. The study also points out that model improvements largely depend on the top-down emissions, the ozone metrics used, and model versions.

The manuscript is in general well organized and meets the scope of ACP. One main concern is that the manuscript has been presented as a model evaluation paper that comparing several model simulations with different NOx emissions with surface

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Discussion paper

and sonde ozone measurements. It lacks some analyses in depth to understand the driving factors of the differences. The key findings of this study are also not clear. Do we have a better understanding of the NOx emission trends as constrained by the satellite measurements, or how NOx emission changes affect tropospheric ozone? I think the concern and the following specific comments should be addressed before considering publish.

Specific comments:

1) Page 1, Line 24-25 in the Abstract:
The statement "using NOx emission datasets that have the best performance . . ." is not clear. As ozone simulation is affected by many other factors, the NOx emissions that have the best performance on ozone simulation may not be the correct one. Some results in this study also showed that satellite constrained NOx emissions did not necessarily improve ozone simulation (e.g., China daytime surface ozone in Figure 5)

2) Page 3, Section 2.1:
What was the spin-up time for the model simulations? Were you using the same initial conditions? Please clarify.

3) Page 6, Line 179:
Should here "the average of GEOS-Chem simulated NO2 column density" be OMI observed NO2 column density over 2x2.5 grid cell? Here you are generating pseudo measurements in the statement. The ratio should be calculated by OMI observations to avoid the OMI vs. model biases.

4) Page 8, Line 240-245:
The large differences in seasonal variations of DOMINO and NASA posterior NOx emissions seem interesting. Here you explained that the DOMINO posterior may

better constrain soil emissions. Do you have any evidence or support for that?

5) Page 8, Line 250-256:
Here you showed that prior simulated surface ozone concentrations had double maxima in April and August, and the posterior results partly corrected the biases. What cause the double maxima in the prior simulation? And how NOx emission changes correct the August maximum? Please clarify.

6) Page 9, Line 269-271:
As indicated in Figure 6, interannual changes in the two posterior NOx emissions in Australia over 2005-2016 are not that consistent. The DOMINO results show large reduction over 2006-2010 and then increase afterwards. Do you have any explanation why the two satellite products show different interannual variation and trends over some regions?

7) Page 10, Line 319:
"Ozone measurements in 2014 decreased compared to the 2006 level in China, the US, South America and Mexico". I do not see from Figure 9 that in China ozone concentration in 2014 was lower than 2006.

8) Page 10, Line 314-316: How did you separate the ozone trends caused by NOx emissions vs. meteorology? A description in the main text is needed. Also, you may calculate the meteorology (non-NOx) effects using either GC-adj or GCv12 results? Which one did you use in Figure 9, and how they differed?

9) Page 11, Line 338: It is surprising that the model versions (GCadj and GCv12) simulate very different ozone vertical profiles. GCv12, which is a more updated version, has much large biases in the upper troposphere, in particular with the updated NOx emissions. Can you explain why in GCv12 changes in surface NOx emissions

would lead to large ozone changes in the upper troposphere?

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