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Interactive Comment

Interactive comment on "Constraints on Asian ozone using Aura TES, OMI and Terra MOPITT" *by* Z. Jiang et al.

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We thank the reviewer for the thoughtful and detailed comments. Below we respond to the individual comments. In addition to the revisions discussed below, all figures (Figure 4, 5, 6, 7) and tables (Table 2), associated with sensitivity calculation, are recreated with the updated NOx and CO emission inventories. Its influence on the analysis is small.

Reviewer #1 General comments (1) As I mention below in one of the specific comments, the reader expects that the improved emission estimates that result from the information provided by the satellites (which are central to this study) would subsequently be used for the modelling in the sensitivity/attribution analysis performed later.

This is also what the reader is left to believe in the abstract and in the conclusions ("...we use satellite measurements...to quantify O3 precursor emissions for 2006 and their impact on free tropospheric O3..."; also first sentence of the Summary). However, on Page 19525, Lines 12-15, it is mentioned that this is not the case. This seems somewhat contradictory, given that the satellite information is apparently used to improve the emissions estimates. Could the authors perhaps include some discussion/evidence (in addition to the sentence on Lines 14-15 on that page) on how the main conclusions of the sensitivity analysis might have changed had the top-down emissions been used?

Response: Thanks for catching this issue. We have repeated our calculation by driving the model with the updated NOx and CO emission inventories. All figures, tables and discussions, associated with the sensitivity calculation, are replaced with the new results. The influence of these updates on the analysis is small.

(2) Since the sensitivity/attribution analysis has been done for all seasons, it would have made more sense to evaluate the model for seasons outside of the summer as well. If that is too difficult at this stage, could you at least include some discussion on known model biases (e.g. based on previous studies) for non-summer seasons?

Response: Thanks for this suggestion! A new figure (Figure 4) was added to evaluate the model simulation. The bias in the CO simulation is significantly reduced. The improvement on the O3 simulation is not significant suggesting that free-tropospheric ozone in the geos-chem model is not significantly sensitive changes in NOx and CO emissions.

Specific Comments: (1) Page 19516, Line 20: If not referring to statistical significance, please use the word "sizeable" or something equivalent.

Response: Thanks! It has been changed.

(2) Page 19517, Line 2: Please change "there is" to "there was".

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Response: Thanks! It has been changed.

(3) Page 19517, Line 17: Why evaluate only then, if the model is going to be used for studying the whole year (see general comment above)?

Response: Thanks for your suggestion! All figures and tables have been updated.

(4) Page 19518, Lines 15-16: Why only for CO?

Response: We use DOFS for CO as a metric for sensitivity of the data as we empirically find that the sensitivity of CO is the limiting factor in these comparisons, that is, if DOFS of CO is > 0.8 then the DOFS of O3 is > 0.8. The description has been changed.

(5) Section 2.1 (general): Worth mentioning here any known biases for TES ozone and CO in this version. That said, which version of the data is being used?

Response: Thanks! More description has been added.

(6) Page 19519, Lines 6-7: Is this the same a priori as for TES? Please mention.

Response: Thanks! It has been changed.

(7) Section 2.3 (general): Any known biases for this product? Please outline.

Response: Thanks! It has been changed.

(8) Page 19521, Lines 4-5: Please explain why 2006 was selected? Perhaps due to data availability/quality?

Response: The major reason is that the data density of TES measurement is higher in 2006-2010. We focused on 2006 just because it is the first year of this five-year period. Explanation has been added.

(9) Page 19523, Lines 5-6: Please rephrase to "We will also study the adjacent domain. . ."

Response: Thanks! It has been changed.

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(10) Page 19523, Line 8: Please rephrase to ". . ., using the GEOS-Chem model driven with a priori emission inventories."

Response: Thanks! It has been changed.

(11) Page 19523, Lines 9-10: Is this 7ppb adjustment made for the comparison shown in Fig. 2? I presume no, but this needs to be clarified.

Response: We didn't remove the 7 ppb bias in the TES O3 measurements. The reason is the World Ozone and Ultra-violet Radiation Data Centre (WOUDC) sites used in the validation (Verstraeten et al., 2013) are mainly located in Europe. The validation also shows larger bias in summer (8 ppb) and smaller bias in winter (5 ppb). It may not be an accurate evaluation for East Asia. Explanation has been added.

(12) Page 19523, Line 11: Worth mentioning here that such negative biases for CO are common in present-day modelling (e.g. see Fig. 2 of Naik et al., 2013).

Response: More citations have been included.

(13) Page 19523, Lines 19-21: Ozone and CO interannual variabilities and trends seem to be well correlated on Fig. 2. That is worth mentioning as well somewhere around here.

Response: We have added more description.

(14) Figure 2 (general comment): The fact that with such CO biases ozone is still captured relatively ok (at least for July-August) implies that there may be biases in other aspects of the simulation that compensate for the CO influences. Please comment.

Response: According to new Figure 4, 10-20% change on CO emission can significantly reduce the bias on CO. The sensitivity of O3 on CO is only about 20% of O3 on NOx (Figure 5, 6), which means 20% change on CO emission can only result in 0.2 ppb change on O3 concentration. Thus, it seems that the influence of CO bias on O3 is very small.

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(15) Table 1: There are no red colors appearing. Also: Are the slopes/correlations calculated using daily mean values? Please mention.

Response: The model values are sampled at TES measurement time and location rather than daily mean values. Description has been added in the Caption.

(16) Page 19524, Lines 13-16: Not clear from Table 1 that there are larger discrepancies over the ocean (outflow region). Looking at the mean values, slopes disagree more with TES in the outflow region but correlations look somewhat better there.

Response: Thanks for your comment! The discrepancies between TES/Model are larger for the outflow region in special months. However, as you indicated, it makes more sense to only compare the mean values because the data is sparse. The statement about the "larger discrepancies over the ocean" has been removed.

(17) Page 19524, Lines 17-19: Suggested rephrasing: ". . .implies that the model captures oxidant-related processes well over East Asia and Northwest Pacific (or Asian outflow region)."

Response: Changed.

(18-1) Page 19525, Line 6: Are the authors referring to the NOx emissions here? Not clear. (18-2) Page 19525, Lines 7-9: The meaning of this sentence is somewhat non-transparent and does not follow from any previous discussion. Please expand and explain further. (18-3) Page 19525, Lines 12-15: This could confuse a reader: the earlier text makes one think that the top-down estimates are made in order to get improved a posteriori emissions. Why would they then be ignored?

Response: We have repeated our calculation by using the updated inventories. This paragraph has been completely rewritten.

(19) Page 19526, Lines 4-9: Are there any ideas on why the contribution from CO comes mainly from further north in China compared to the NOx contribution (see Fig. 4)?

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Response: A very good question! Our speculation is North China Plain is more inclined to be VOC limited and therefore is not sensitive to NOx emission. On the other hand, it is sensitive to CO emission from this region. More discussion has been added.

(20) Page 19528, Lines 2-3: Why are VOC sensitivities not shown on Table 2?

Response: It would be a good idea to have a sufficient estimation for VOCs. Unfortunately, the current model only allows us to calculate the sensitivity on biogenic isoprene. The values, based on biogenic isoprene, will be much smaller than the actual value based on total VOCs. For this reason, we believe it is not suitable to compare it with sensitivity on NOx directly.

(21) Page 19528, Line 9: ROA has not been defined earlier.

Response: Changed.

(22) Page 19529, Lines 18-19: The fact that Chinese NOx emissions have the largest con- tribution needs to be mentioned first. As it reads currently it may mislead the reader to believe that ROA emissions are more important than those from China.

Response: The discussion has been modified.

(23) Page 19529, Lines 19-20: I agree that there will be some consequences for North America, but they have not been demonstrated here, so I suggest removing that part of the sentence or writing something like "with potential implications for background O3 concentrations of North America".

Response: The discussion has been modified.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 19515, 2014.