Atmos. Chem. Phys. Discuss., 14, C6664–C6668, 2014
www.atmos-chem-phys-discuss.net/14/C6664/2014/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

Received and published: 8 September 2014

The manuscript by Jiang et al. makes use of a collection of satellite information in combination with a state-of-the-art chemistry-transport model (and its adjoint version) in order to explore the relationship between different emissions and tropospheric ozone production over China and its outflow region. The topic is certainly suitable for Atmospheric Chemistry and Physics. The work presented is interesting and will be a useful addition to the discussion on anthropogenic versus natural effects on ozone pollution in the area, as well as over the Pacific. Furthermore, it is a nice demonstration of how the synergy of multiple Earth observation datasets and global modelling can be used to answer intriguing scientific questions. Therefore, I recommend publication following some revisions described below.

GENERAL COMMENTS:

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

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?

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?

SPECIFIC COMMENTS:

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

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

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)?

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

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?

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

ACPD
14, C6664–C6668, 2014

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

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

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

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

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.

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).

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.

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.

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

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.

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)."

Page 19525, Line 6: Are the authors referring to the NOx emissions here? Not clear.

ACPD
14, C6664–C6668, 2014

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.

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?

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)?

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

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

Page 19529, Lines 18-19: The fact that Chinese NOx emissions have the largest contribution 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.

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”.

REFERENCES:

Naik, V., Voulgarakis, A., Fiore, A. M., Horowitz, L. W., Lamarque, J.-F., Lin, M., Prather, M. J., Young, P. J., Bergmann, D., Cameron-Smith, P. J., Cionni, I., Collins, W. J., Dalsøren, S. B., Doherty, R., Eyring, V., Faluvegi, G., Folberth, G. A., Josse, B., Lee, Y. H., MacKenzie, I. A., Nagashima, T., van Noije, T. P. C., Plummer, D. A., Righi, M., Rumbold, S. T., Skeie, R., Shindell, D. T., Stevenson, D. S., Strode, S., Sudo, K., Szopa, S., and Zeng, G.: Preindustrial to present-day changes in tropospheric hydroxyl radical and methane lifetime from the Atmospheric Chemistry and Climate Model Intercomparison Project (ACCMIP), Atmos. Chem. Phys., 13, 5277-5298, doi:10.5194/acp-13-5277-2013, 2013.

ACPD
14, C6664–C6668, 2014

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

ACPD
14, C6664–C6668, 2014