

Interactive comment on “CO emission and export from Asia: an analysis combining complementary satellite measurements (MOPITT, SCIAMACHY and ACE-FTS) with global modeling” by S. Turquety et al.

Anonymous Referee #1

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Review of the paper “CO emission and export from Asia: an analysis combining complementary satellite measurements (MOPITT, SCIAMACHY and ACE-FTS) with global modeling” by Turquety et al.

General comments

The main objective of the paper is to document the CO sources over and pollution export from Asia through the simultaneous use of CO data set from different satellites. In the mean time, the capability of a global chemical transport model to represent those sources and export is also evaluated. I find the paper to be very interesting

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because there is a clear attempt to intercompare CO observations from 3 satellites and also to combine their different level of information to explore the Asian source and continental export of pollution. The resulting outcomes however, especially those in terms of the characterization and/of quantification of the Asian sources and export are not entirely new and original, and I would like to encourage the Authors to focus more on findings that are really different in comparison to other existing studies.

Specific comments

Section 1.

Please provide additional details about the work of Jiang et al., 2007. How does that differ (or is similar to) the present work?

Section 2.

I have several comments about this section. In fact, I am not sure I clearly understand the main purposes of this section. If the overall idea is to justify the scaling of the EDGAR inventory, then the Authors should say so.

Some more comments:

What do the Authors mean by “Different convection schemes” (page 1714, line 6-7). I assume this has not been done in the frame of this paper so please provide references. Page 1713, line 23: Please define Asia.

Page 1716, line 3-5. Please quantify what you mean by “The agreement between the model and the surface data is very good”. What does “very good” mean?

Page 1716, line 9. Same comment than previously. The Authors say “the transport events are well captured”. What does that mean? How could they say that from a comparison to the ESRL data?

Page 1716, line 23-27. The argument that the MOZAIC data are representative of highly polluted regions that are not well resolved by the global models because of their low model resolution does not hold for the middle troposphere, in my opinion. In fact during their ascend/descend, the planes get further and further away from the

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highly polluted conditions that they can find in the boundary layer. Also, what are the implications of this underestimate for the comparison with satellites (see for example sensitivity of MOPITT to that region of the troposphere)?

Section 4

Page 1722, lines 25-27. Please include references for these processes.

Page 1722, line 2. What do you mean by “vertical winds”? Are those associated to convection, orographic forcing? How would vertical winds over the western Pacific induce a transport of the pollution from the boundary layer to the free troposphere? Does that mean that the pollution has been already transported over the oceanic region? These questions are also connected (to some extent) to my remark mentioned above about the real contribution of this study to the understanding/quantification of the Asian pollution export.

Page 1723, line 4. Was the LMDz-INCA model included in the study of Shindell et al.?

Page 1724, line 15-25. I wonder whether the Authors try to tell too much from these various satellite observations. Given the inherent uncertainties of satellite observations as well as the apparent systematic bias between SCIAMACHY and MOPITT, I wonder whether the BLR quantity that they defined can be interpreted in any quantitative manner. In addition, they have to apply a correcting value that is taken over “west-central Asia”. What is west-central Asia? Why is that region chosen? In general, why should that value be correct if the emissions over Asia are incorrect?

Page 1725, lines 3-6. Do the Authors imply that emissions may be underestimated by 37% over some regions of China? How this number compare with more recent estimates of Asian emissions? If they believe that their method is somewhat quantitative (which they do, from what I can read in the paper), then they should compare their results to those from recent emission inventories.

Page 1725, lines 7-8. Do they imply that all sources are too low in their model, including biomass burning and anthropogenic sources? Or is there an artifact in their methods which results that all source regions become apparent when plotting the BLR? Do they find similar (or different) results for other regions of the world?

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Page 1725, line 9-10. I may be mistaken but could not find any discussion about that on Section 4.1.

Section 4.3.

This section is a bit wordy (also includes some repetitions from previous sections to some extent) and (in my opinion) does not provide many new insights in terms of the trans-pacific transport of CO. I recommend that the Authors re-write this section in a more concise way, only focusing on new insights.

Conclusion

As mentioned before, the conclusion needs to better highlight the new findings of their work with respect to the pollution export/transport.

Page 1731, line 15-17. The Authors say that “a more thorough analysis of the possible trends in the MOPITT CO...”. Why don’t they recommend the use of the BLR (rather than the entire MOPITT column) to derive trend in emissions (availability of data)?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 1709, 2008.

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