Atmos. Chem. Phys. Discuss., 7, S9903–S9911, 2008
www.atmos-chem-phys-discuss.net/7/S9903/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on “Inversion analysis of carbon monoxide emissions using data from the TES and MOPITT satellite instruments” *by* D. B. A. Jones et al.

D. B. A. Jones et al.

Received and published: 27 August 2008

Response to Reviewer 1

We thank the reviewer for the thoughtful and detailed comments on the manuscript.

Response to General Comments 1 and 2

The primary concern of the reviewer is that we have not substantiated the large increases in emissions in sub-equatorial Africa and Indonesia/Australia through comparison with independent data. We have, therefore, added a comparison of the model simulation of CO (with the a priori and with the a posteriori emissions) with surface observations from the NOAA GMD observation sites. Figure 4 in the manuscript now shows that the model does correctly capture the seasonality of the global distribution of CO at the surface. In the southern tropics, in fall, the a priori simulation underestimates the observed CO (consistent with the comparison between the model and the satellite data), but the a posteriori emissions significantly reduce this bias. The inversion, however, has an adverse impact on CO in the mid-altitude of the southern hemisphere, which was previously identified by Arellano et al. (2006) in their inversion of MOPITT data using the GEOS-Chem model.

Response to General Comment 3

We agree with the reviewer that this discussion was speculative. We are currently preparing a manuscript examining specifically the influence of model errors and spatial resolution in the inversion analysis. This work was presented at AGU in Dec. 2007 as Jiang, Z, D. B. A. Jones et al., Quantifying the impact of aggregation errors and model transport biases on top-down estimates of carbon monoxide emissions using satellites observations, Eos Trans. AGU, 88(52), Fall Meet. Suppl., Abstract A23C-1484, 2007. We have, therefore, removed discussion of aggregation errors in this manuscript.

Response to specific comments

1) We have added the reference.

2) We agree, however, there was no significant change in the CO retrievals between V001 and V002 so we continued using V001.

3) We do not use retrievals in 2005 because our primary objective is to explore the use of the simultaneous TES observations of O3 and CO for reconciling the discrepancy between the modeled and observed O3 distribution in fall 2004. As we explain in our response to the second reviewer we have modified the manuscript to make this emphasis more clear. In particular, we have changed the title of the manuscript and restructured the introduction. The title is now: *The zonal structure of tropical O3 and CO as observed by the Tropospheric Emission Spectrometer in November 2004 I. Inverse*

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modeling of CO emissions.

Furthermore, by April 2005, the DOFs in the TES retrievals of CO, averaged between 30S-30N, had dropped to less than 1.0. It was not until the optical bench was warmed up in December 2005 that the DOFs increase to about 1.5. Therefore, the CO data from spring through early winter 2005 offer only limited information.

4) Heald et al. (JGR, 2004) compared the use of a uniform diagonal error covariance matrix with a complete error covariance matrix obtained using the NMC method. The construction of the NMC covariance, as described in Heald et al and Jones et al. (JGR, 2004), uses differences between pairs of 24-hour and 48-hour forecasts of CO to construct the covariance structure for the errors in the model simulation of CO. As described in Heald et al., this covariance structure was then scaled based on the variances obtained from the differences between MOPITT and the GEOS-Chem simulation of CO. We have text on page 8 explaining this.

5) Duncan et al. [JGR, 2007] recently showed that these emissions provide a good description of the distribution of CO for the period 1988-1997. The more recent EDGAR emission inventory for CO has serious flaws, as summarized in Duncan et al. (2007). The a priori represents a starting point in the inversion from which we optimize the emissions based on the satellite data.

Furthermore, Bian et al (JGR, 2007) presented a comparison of the surface GMD observations with CO simulations obtained with the GFED1, GFED2, and Duncan et al. (2003) inventories and found that these inventories all produced 8220;modeled CO concentrations [that] are within the observed range of variability at most stations including Ascension Island, which is strongly influenced by fire emissions.8221; We, therefore, believe that these emissions provide an acceptable a priori for the inversion analysis.

6) This way of presenting the data depends on the assumption that the seasonal cycle of the emissions in the model is correct, which we explicitly acknowledged in the paper.

However, we have now added Figure 3 showing the seasonal cycle of the emissions (in response to reviewer 2), since this is such an important assumption in the work. We note that comparison between the a priori simulation and the surface data, shown in Figure 4, does not suggest that the seasonal cycle is incorrect in the model..

Scaling the estimates as suggested by the reviewer would result in an overestimate of the annual emissions since the tropical sources have a strong seasonal cycle and we are conducting the inversion during the end of the season with maximum emissions.

7) The reviewer would be correct if we were conducting a 4-dimensional variational analysis in which we optimize the initial state vector to best reproduce the observations over the 2-week assimilation period. However, the Jacobian matrix (influence functions) is calculated using a separate tracer for the emissions from each region in the state vector. In early November, the distribution of the tracers for each region will reflect the influence of emissions from the previous months (also noted by reviewer 2). The extent of the impact of emissions from previous months on the tracer distribution in early November will depend on the lifetime of CO. Petron et al (2002) found that in a given month, the dominant contribution was from emissions from the previous three months. We have added text on page 8 citing the results of Petron et al. (2002).

8) We are referring to the fact that the information from the emissions decay in the atmosphere based on the lifetime of CO, as discussed above. We have reworded this to make the text clear.

9) The reviewer is correct, it is irrelevant in the context of the biases in the estimates of the regional sources. We have removed the text.

10) It is meaningful to the extent that the seasonality of the emissions is correctly represented in the model. Our comparison of the model with GMD data (Figure 4) shows that, despite the fact that there was a mild El Nino in 2004, the Duncan et al. (2003) inventory provides a reliable simulation of the seasonality of CO in the tropics.

11) The reviewer is correct, the coarse resolution inversion may average out biases in the source estimates, however, we note that a higher resolution model would offer more degrees of freedom to better fit the data. (Of course, with more degrees of freedom one could obtain a better fit with unphysical changes in the emissions.) Nevertheless, we have removed discussion of the influence of the aggregation errors here.

12) We have corrected this.

13) We have modified the wording. Indeed, on page 17641, line 7, of the original manuscript we explained why we would expect these differences in the North African emissions.

14) We respectively disagree with the reviewer. As we explained above, over inversion framework does not constrain only the emissions from the beginning of the data window. However, this point is not relevant to our discussion here and thus we have changed the text.

15) In Jones et al. (2003) we established that the TES sampling and measurement precision provides sufficient information to constrain estimates of the continental sources of CO with two weeks of data. The consistency of the TES and MOPITT results here, with two weeks of data, does suggest that we can transfer these results to MOPITT. We note that in the absence of this work one would expect that the results would be transferable since the TES and MOPITT retrievals have similar DOFS and MOPITT provides greater sampling of the CO distribution.

16) We have made the suggested change.

17) We have added the reference.

18) North America and European emissions provide a small contribution to the total CO abundance in the free troposphere in fall as a result of the short lifetime of CO in the summer. In fall through winter, as the CO lifetime increases, the emissions of CO from North America, Europe, and Asia accumulate in the troposphere. As we noted

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in the manuscript, the North American and European estimates obtained with the TES data were correlated, suggesting that there is not sufficient information of independently quantify these sources with TES. MOPITT provides more information and we find that the European and North American estimates are not correlated. However, with MOPITT the North American estimate is correlated with the background. It is not clear why MOPITT seems to provide more information on the European emissions. The transport patterns for the two regions are different. North American emissions are lifted into the free troposphere mainly by frontal lifting along the east coast of the Continent and over the Great Lakes region, and capturing these synoptic signals could be more challenging for both instruments.

19) We have added text explaining this on page 12.

20) As we explained on page 17640, lines 12-13, of the original manuscript, this estimate should indeed remain at the a priori. The fact that it does not indicates the presence of systematic errors in the inversion analysis.

21) Both studies used the GEOS-Chem model, but Heald et al. was regional, whereas Arellano et al. was global. As we stated on page 14, this could contribute to some of the differences.

22) The reviewer is correct. This could have been better explained. We have modified the text on page 17.

23) Muller and Stavrakou (2005) showed that using GOME NOx and surface CO observations was better than using only surface CO observations. We have stated this more clearly on page 18 in the manuscript.

24) The reviewer is correct. The key point is to conduct the inversion at a resolution "high enough." We have reworded the text.

25) We have shortened the text.

26) The model is indeed biased low relative to both TES and MOPITT. However, com-

parison of the model with surface data from the GMD network does not indicate a global bias in the model.

27) As we explain below (in response to comment 29), we expect differences between the MOPITT and TES retrievals since different a priori profiles and constraint matrices are used in the retrievals from the two instruments. By transforming the model (using equations 4 and 5) with the MOPITT and TES a priori profiles and averaging kernels the bias associated with the a priori profiles should be removed when the transformed modeled field is compared with the satellite data (as was done for Table 2). However, as shown by Luo et al. (2007), the vertical sensitivities of the instruments, as reflected in the averaging kernels, is different. Therefore, the bias between the model and the observations will be manifested differently in the column average between the two instruments because of the different vertical smoothing in the retrievals. For example, Luo et al. found that substituting the MOPITT a priori profile in the TES retrievals reduced the bias between TES and MOPITT (TES minus MOPITT) from -11% to -5.4%. However, smoothing the transformed TES profiles with the MOPITT averaging kernels further reduced the bias to -4.4%. The source estimates are similar with the two datasets because these differences due to the vertical smoothing in the retrievals are accounted for in the inversion since the averaging kernels are incorporated in the Jacobian matrix. We have added text on page 7 explaining this.

28) There were some typos introduced in the online version of the manuscript. The Central Pacific region is defined over 10S-10N, 180W-80W. Similarly, the Global region should have been 60S-60N (not 60S-0N) and the North Pacific should have been 25-60N, 175W-120W (not 25-0N,175W-120W).

29) The MOPITT column abundances are different than those from TES due to the different a priori profiles and constraint matrices used in the retrievals. In particular, MOPITT uses a uniform a priori, whereas TES uses a regionally a priori based on the MOZART model. As we mentioned in the manuscript, Luo et al. found that after accounting for the different a priori profile and averaging kernels, the bias between the

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two datasets was less than 5% in the column We have added text to the discussion of Figure 2 (on page 6) to better inform the reader of this.

30) We chose to plot the instantaneous, relative abundance of CO in Figure 7 (previously Figure 5) to show clearly the relative contribution of North American emissions to the total CO abundance and because it is the instantaneous rather than the time-averaged spatial distribution that the instrument samples. The instantaneous distribution shows the synoptic structure that the satellite must sample to provide significant information on the contribution of North American CO emission to the total atmospheric CO. Plotting the absolute CO abundance due to North American emissions in Figure 7 and comparing them with Figure 6 (previously Figure 4) would not be helpful in assessing the importance of North American emissions compared to emissions from the tropical regions as there is little interhemispheric transport of CO. In addition, the CO abundance in the northern hemisphere is much greater than in the southern hemisphere, therefore one cannot compare the absolute CO abundance due to emissions from the tropical regions with that from North America and easily assess the relative importance of these regions toward the total CO abundance.

Response to Technical Comments

1) The text has been removed.

2) The text has been removed.

3) Corrected.

4) Changed.

5) Changed.

6) Changed.

7) The section is already entitled "The TES and MOPITT Instruments". We therefore do not see why a new section is needed here.

8) Changed.

9) Done.

10) Changed.

11) Changed

12) Changed.

13) Done.

14) Changed.

15) Changed.

16) Changed.

17) Changed.

18) Changed.

19) Done.

20) The text has been removed.

21) Done.

22) Done.

23) Done.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 17625, 2007.

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