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Atmospheric Chemistry and Physics Discussions

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7, S9912–S9920, 2008

Interactive Comment

# *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.**

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**Reviewer 2**

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

*Response to General Comments*

1) The reviewer is concerned that the data do not constrain all the regional sources being estimated. As we acknowledged in the manuscript, the data do not constrain all the sources. In particular, we explicitly state that there is not sufficient information to constrain the source estimates for North America and Europe. However, our interest is on the tropical sources that are responsible for the discrepancy between the model and

the observations in the southern tropics. As we show in the analysis, estimates of the emissions from these tropical sources are well constrained by the data. We agree with the reviewer that the northern midlatitude regions would be better constrained in spring, when the signal from emissions in these regions is stronger. Indeed, this is what was shown in Jones et al. (JGR, 2003): two weeks of TES data in March provided sufficient information to constrain estimates of CO emissions from North America, Europe, and Asia. However, our focus here is on November 2004. We could have aggregated these northern midlatitude sources with the rest of the world, since they are not the focus of the analysis, but we chose to solve for them to illustrate sensitivity of the inversion to the information content of the data. More data, as suggested by the reviewer, may help, but fundamentally it is a signal-to-noise issue. This is an important point and so we have added text on page 13 in the manuscript explicitly contrasting this work with the springtime study of Jones et al. (2003).

2) We agree with the reviewer that increases in CO emissions do not mean an increase in NOx. Smoldering fires, for example, would not lead to significant increases in NOx emissions. However, the ultimate objective of this work is to reconcile the modeled CO and O3 abundances with the TES data and the significant underestimate of O3 by the model implies an underestimate in the emissions of the O3 precursors in the model. As NOx is a key precursor for O3, we chose to scale the NOx emissions. Changes in CO alone would have only a small impact on O3. We have changed the title and restructured the introduction to make it clearer that the focus of this work and the companion study Bowman et al. is on both CO and O3. The title is now: *The zonal structure of tropical O3 and CO as observed by the Tropospheric Emission Spectrometer in November 2004 I. Inverse modeling of CO emission.*

3) Reporting the source estimates as annual totals does indeed depend on the seasonality of the emissions. However, we do not believe that the seasonality is incorrect. Bian et al. (JGR, 2007) showed that the Duncan et al. (2003), the Arellano et al. (2006), GFEDv1, and GDFEDv2 inventories all provided a simulation of CO that is within the



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range of variability of the surface observations at the GMD surface sites. We explain this on page 10. However, most importantly, our comparison of the a priori simulation with the surface data does not suggest a significant error in the seasonality of the a priori emissions.

4) The reviewer expressed concern that our discussion of aggregation error and transport bias is speculative. We agree and have minimized discussion of these errors in the revised manuscript. Instead, these issues are being addresses in a manuscript in preparation by 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.

*Response to Specific Comments*

1) We are confused by this comment. We have shown that within the context of the inversion analysis presented here the information from the two satellites is consistent. Can we extract more information from the data with a different inversion framework? Yes, that is possible. Indeed, that is the motivation behind the Jiang et al. (2007) study that we are conducting. However, that fact does not make the statement that we examine "the consistency of the information provided by these two instruments" misleading.

2) As we explained above, we agree that our discussion of the sensitivity of our results to aggregation error was speculative and have removed discussion of this. We have also removed the text about the issue of sampling. The reviewer's concerns about objectives (1) and (4) were raised in general comments (1) and (2). As we explained in our responses above, we believe that we have achieved these objectives.

3) *p. 4 Section 2.1. Were the MOPITT CO retrievals used in the inversion treated independently? How many profiles were used in the analyses? Were there any preprocessing (i.e. removal of bias) done? Were MOPITT CO columns used in the analyses*



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*as well?*

4) *p. 4 Section 2.2 Were the TES CO retrievals used in the inversion treated independently? How will this affect the posterior error estimates? How many profiles were used in the analyses? How does this number compare to MOPITT?*

The MOPITT and TES retrievals were treated independently, as we explained on page 17634, lines 27-29 of the original manuscript. We do not use the columns. As we explained, we use profiles of CO from TES and MOPITT and account for the vertical correlation in the profile, while neglecting the horizontal correlations.

Since TES operates on a one-day-on, one-day-off observational cycle, we sampled the MOPITT observations every other day. This produced a total of 10613 MOPITT profiles and 3011 TES profiles for the inversion. We have added text to the first paragraph of Section 3 (on page 7) to explain this.

As we explained on page 17634 of the original manuscript, the choice of the covariance matrix impacts the poorly constrained a posteriori estimates. Heald et al. (2004) found that neglecting the horizontal correlations or assuming isotropic correlations produced differences in their source estimates of 10-50% compared to their "best case" for which the correlation structure was determined using the NMC method. They obtained the smallest differences for the large, well-constrained source regions and the largest difference for poorly constrained regions such as Japan and Korea. In our analysis we find that the tropical source estimates are insensitive, whereas the North American estimate is most sensitive to the error specification.

5) The issue is not with the different versions of the data. There was decreasing signal strength in the TES 1A1 filter throughout 2005, which strongly impacted the CO retrievals. After the warm-up of the optical bench in Dec. 2005, the signal strength increased significantly. This is an issue that is independent of the retrieval version.

6) The reviewer is correct. The differences in sampling patterns and vertical sensitivity



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will result in differences in constraints, as explained in the manuscript. We could have conducted an analysis using only pseudo-data. However, our objective was not merely to assess this sensitivity. Our main focus was to understand the factors contributing to the discrepancy between TES observations of CO and O3 and the model simulation in the tropics in November 2004. Assessing the consistency was a secondary objective of the work. As we explained above in our response to the general comments, we have modified the text and title of the manuscript to make this clear.

7) As we stated in the manuscript, for consistency with the description of the satellite retrievals we chose not to define the observation vector as y and the state vector (the CO emissions) as x. We believe that it would be extremely confusing to use x to represent both the TES retrievals and the inversion state vector. To make this distinction more clear, we have changed the notation for the inversion state vector to u.

8) K is changing for each iteration and we scale the tagged CO tracers, which is derived from taking the derivative of A[ln(H(u))] with respect to u (as described in equation (13) of Jones et al. (2003)). The tagged tracers represent . We do not conduct extra forward model calculations.

9) The error is specified as 20% of the observations. Jones et al. (2003) showed that the transport error, calculated using the NMC approach, was large in the vicinity of the source regions and small in the remote troposphere. Estimates of the observational error covariance by Heald et al. (2004) based on the difference statistics between GEOS-Chem and MOPITT, showed that the largest errors are similarly near and downwind of the major source regions.

We agree with the reviewer, the insensitivity of the Heald et al. (2004) results was because the estimates were well constrained by observations in the vicinity of the source regions. This is also the case here. Both satellites are providing observation over and near the source regions. Certainly, as one moves away from the source region, the signal weakens and thus the estimate becomes more sensitive to the error specification (i.e. the noise level). This is why the satellite observations provide more constraints on the source estimates that the GMD station data, which are located in the remote troposphere. Similarly, Heald et al. (2004) showed that aircraft observations downwind of Asia provided fewer constraints than MOPITT data over and downwind of Asia.

We experimented with different estimates of the observation error, such as 30%, 50% and 100% and it did not have an impact on the tropical source estimates. These are robust. The North American and European estimates were sensitive to the error specification because they are not well constrained. As we explained above, the issue with North America, for example, is that the signal strength is weak during boreal summer and fall. We believe that the reviewer misunderstand our point. TES and MOPITT provide consistent and robust constraints on the tropical sources in boreal fall. We now explictely state in the summary, on page 19, that the inversion is less sensitive to the midlatitude sources.

10) As explained on page 8, following Palmer et al. (2003) we assumed a uniform uncertainty of 50% and neglected any a priori correlation between the different source regions.

11) We have added Figure 3, showing the seasonality of the emissions in the model.

12) We noted on page 17636, line 2, that the OH fields are from Evans and Jacob (2005). The global mean OH abundance of $11.3 \times 10^5\ cm^{-3}$ reported in the manuscript was for November. To facilitate comparison with other estimates in the literature, we now report on page 10 that the global annual mean OH abundance is $10.8 \times 10^5\ cm^{-3}$ which, as reported in Evans and Jacob (2005), compares well with the estimates of $11.6 \times 10^5\ cm^{-3}$ from Spivakovsky et al. (2000) and $10.7 \times 10^5\ cm^{-3}$ from Krol et al. (1998).

13) The reviewer is correct. We cannot assume this for sub-equatorial Africa since fuel combustion could provide a large contribution to these emissions, as we noted on page 17637 of the original manuscript. We have therefore removed this text.



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14) We have removed this text in response to reviewer 1.

15) It is not clear what the reviewer is referring to here. We assume that it has to do with our estimate for Indonesia and Australia. Petron et al. (2004) reported an a posteriori estimate for emissions from "Oceania" (Indonesia and southeast Asia) of 89.6 Tg/yr, which is similar to our a priori estimate of 69 Tg/yr for Indonesia and Australia and much less than our a posteriori estimate of 155 Tg/yr (based on TES data). The Petron et al. (2004) estimate is also significantly lower than that from Arellano et al. (2006). It is difficult to compare our results directly with Petron et al. since our regional definitions are quite different (we include emissions from Australia, but omit those from southeast Asia for this region). Furthermore, our comparison of the model with the satellite and GMD data show that the a priori does provide an underestimate of CO abundances in the tropics.

16) The difference between the North African estimates is 10%, which is consistent with our statement that "the source estimates inferred from the two datasets are about 20% or less, with the exception of North America."

17) The statement is valid for fall. In spring the CO lifetime is longer and therefore the signal strength for the midlatitude sources is stronger. The conclusion is not speculative. In Jones et al. (2003) we showed that in spring, two weeks of TES data can constraint estimates of North American emissions, in contrast to the results obtained in this study in November. We have added text on page 13 contrasting these results with those of Jones et al. (2003).

18) The reviewer is not convinced that two weeks of data provide sufficient information to constrain the surface emissions, yet the inversion successfully reduced the bias between the model and the satellite data in the tropics and between the model and the surface data in the tropics, where there is strong seasonality in the emissions.

We believe that there are two separate issues raised in the reviewer's comment. The first is whether two weeks of data provides sufficient spatio-temporal coverage to quan-



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tify the sources? We believe that, together with the Jones et al. (2003) study, we have shown that this is indeed the case, if the source strengths are sufficiently strong. The second issue is whether we can report the source estimates obtained in the inversion as an annual estimate? That, as the reviewer noted above, depends critically on capturing the seasonality of the emissions. As we responded above, comparison of the modeled CO with GMD data does not indicate a error in the seasonality of the emissions in the model.

19) This was determined from examination of the a posteriori covariance, as was the fact that the North American and European estimates were correlated. We have added to text on page 12 to make this explicitly clear.

20) As we explain in the manuscript, neither instrument provides sufficient information to reliably constrain the North American estimates in fall. More data may be helpful, but the signal from these sources is weak in summer and fall.

21) Using data in spring would help, but that is exactly the point that we are making. In spring, it is possible to independently solve for North American and European emissions, but in fall there is insufficient information in these datasets to constrain the estimates of these emissions. This is what we mean by properly selecting the state vector given the information content of the observations. We have nevertheless reworded this in response to reviewer 1.

22) We agree that the Asian estimates are not well constrained. As noted by the reviewer above, the Asian signal is weak in the fall. We have added text on pages 15 and 19 explaining this.

23) We have not tried to determine whether the bias is in the observations or in the model. We suspect that it is likely a model bias, which impact the northern hemisphere mid-latitude sources because of the reduced sensitivity to these sources in fall. Isolating and characterizing such a bias, however, would be challenging and is not within the scope of our analysis. We believe that the impact of the bias is strong in our analysis



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because we do not optimize the initial state (as suggested by reviewer 1). The impact of such a bias would be less severe in a time dependent inversion analysis such as Arellano et al. (2006), for example, which also used the GEOS-Chem model, because the initial state in November depends on the previously optimized months.

24) We cannot determine that based on the analysis presented here. We have therefore removed the discussion in the revised manuscript.

25) The reviewer is correct, whether the fires are smoldering or flaming will influence the NOx emissions. Please see our response to the general comments of the reviewer.

26) We respectfully disagree with the reviewer. We believe that we have determined the constraints on the CO source estimates offered by the data. We have shown that in the context of the inversion framework employed here, the two datasets provide similar estimates of the CO sources in the tropics but have difficulty constraining the emission estimates from the mid-latitude Northern hemispheric sources; we showed that the a posteriori emissions provide an improved simulation of the distribution of CO in the tropics. We appreciate the reviewer's interest in seeing a more detailed sensitivity analysis of the impact of the different sources of errors in the inversion on the source estimates. However, that is not the focus of the analysis. As we stated in our response to the general comments above, which we believed we did not articulate clearly in the original manuscript, the primary objective of this work is, as a companion paper to Bowman et al., to provide an estimate of the combustion-related surface emissions for CO (and other ozone precursors by proxy) to reconcile the discrepancies between the modeled and observed abundances of CO and O3 in the southern tropics in November 2004.

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



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