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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.

Anonymous Referee #2

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General Comments:

The manuscript provides inverse analyses of regional CO sources using observations of CO abundance from two satellite instruments, i.e. TES and MOPITT. The study provides added insights on the consistency of CO observations and the information they provide on CO sources, which are poorly constrained, despite several inverse studies in recent years. However, I have several concerns with regards to methodology and the conclusions drawn from their analyses. In particular, I am not convinced that the data used (Nov 4-15) in their time-independent inversion are sufficient enough to constrain all the regional sources being estimated. The conclusions on the magnitude of annual regional biomass burning emissions of CO are heavily linked to their assumption on the seasonality in Duncan et al. 2003 and the data used in the inversion. Observations during July, August, September, October, December and January should be considered in order to reasonably estimate the annual emissions of biomass burning in this region (or a sensitivity analysis in this regard). Secondly, the conclusions drawn on the magnitude of Asian and North American sources are not well-justified, given that most of the CO signal from these sources will not be in November but in Northern Hemisphere Spring. The contrast in the estimates for North American CO emissions may be largely due to lack of data to constrain the source in the first place. Thirdly, their conclusions on the sensitivity of source estimates to model biases, aggregation errors and spatio-temporal resolution of the inversion are somewhat speculative, given the lack of sensitivity analyses conducted on these issues. Also, I do not agree on how the authors conducted their analysis on the feedback of OH on CO; in particular, the uniform scaling of NOx emissions from posterior CO emissions. Increases in CO emissions do not necessarily mean increases in NOx. A proper way to test the sensitivity of the source estimates to the assumption of OH is a non-linear inversion approach. In view of these concerns (see below for other comments), I believe the manuscript needs to be revised.

Specific Comments:

Abstract: |This is the first quantitative analysis of the consistency of the information provided by these two instruments on surface emissions of CO in an inverse modeling context|. The term information provided by these two instruments| is misleading since it is possible that these observations provide more information if the inverse analysis was done differently.

Introduction: I dont seem to get the impression that the following key objectives, which were cited by the authors, were well-supported in the paper by some analysis in reasonable detail. 1) Quantify CO emissions 2) Impact of sampling on source estimates 3) Sensitivity of estimates to systematic errors (aggregation) 4) Sensitivity of estimates

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to prescription of OH

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 as well?

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?

p. 6 para 1. |We use version V001…| Am I to understand that DOFS of V001 is about 1-2 and V002 is about < 1?

p. 6 para 2. |The focus of the work presented here is to assess if this agreement between the two datasets imply consistency in the constraints that they provide on surface emissions of CO when the data are incorporated in an inverse model|. Shouldnt they be consistent if the CO concentrations are consistent? I think they may not consistent because of their differences in sampling patterns and in part, on differences in DOFS. But can this possible inconsistency be elucidated more clearly (and cleanly) using pseudo-data analysis?

p. 7 The notation is a little bit confusing. The vectors x and y are in contrast to previous inverse studies (Heald et al, Arellano et al, etc).

p. 7. Is Ki changing for each iteration? Am I to understand that it is only scaled uniformly using yi and no extra forward model calculation was done?

p. 8. Para 1. | were insensitive to the specification of the error covariance structure…We assume a uniform observation error of 20%|. Is this 20% of the observation (either TES or MOPITT)? If it is, this means that there is a small error to lower CO concentration and high error to high CO. This may not be true at all, especially for model errors, e.g. transport of biomass burning sources. I believe the insensitivity reported by Heald et al, 2004 was because the source estimates were well-constrained

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from the data used, which was limited near the source regions. I would think that the sensitivity will increase with increasing domain of the data (more towards the North-east Pacific) as transport becomes important. I would also think that this insensitivity may not be true for TES if the number of data is not sufficient to constrain some of the regional sources (e.g. North America and EU). Did you try to do the inversion with other error covariance structure and did this change your estimates? In other words, how robust is your conclusion on consistency between TES and MOPITT?

p. 8. Para 1. How was Sa constructed?

p. 9 It might be a good idea to show the seasonality of Duncan et al. 2003 since this is a major assumption in the inverse analysis.

p. 9. Para 2. How does your OH compare with previous work?

p. 9. Para 3. |however, we assume that the increases in emissions suggested for the tropical regions are associated mainly with greater emissions of CO from biomass burning|. Is this true for Africa given the uncertainty in emissions from biofuel use?

p. 10 Para 0 | but it should be noted that the prescribed OH fields have a global mean OH….would adversely impact…| How adverse would this impact be?

p. 10 Para 1. It is likely that Duncan et al is low but based on Petron et al 2004, this doesnt seem to be low.

p. 10. Para 2. |with the exception of North America…| How about North Africa?

p. 10. Para 2. |these differences represent the potential influence on the source estimates of the different spatio-temporal sampling of the TES and MOPITT measurements …| I agree but it might be possible that they will still be consistent if sufficient data are used (e.g. Spring observations). In this view, the conclusion is somewhat speculative.

p. 11 Para 0. Im not convinced that 2 weeks data is enough especially if the seasonality is off. August, September and October emissions will still affect modeled CO in November.

p. 11 Para 1. |The inversion analysis can independently quantify emissions form the 3 continental regions…|. This is a bit qualitative. Can this be supported by an analysis of the posterior covariance?

p. 12 Para 0. How about differences in sampling? This tells me that the TES data is not able to constrain NAmerica. I would think that more data will provide better constraints.

p. 12 Para 2. While aggregation error is important, I dont think the discrepancy illustrates clearly the importance of properly selecting the state vector. How about using Spring observations first?

p. 12 Para 3. I dont think it is clear that the Asian emissions are well-constrained in this analysis. In this regard, the comparison of the magnitudes with other estimates may not be conclusive.

p. 13 Para 1. Is this indicative of a MOPITT or TES bias as well?

p. 14 Para 1. |By aggregating all of the Asian emissions ….| Is this largely due to aggregation or transport?

p.15 Para 0. Kindly correct me if Im wrong. As I understand, the NOx and CO emissions may not necessarily be directly proportional. High emissions of CO occur during smoldering phase of combustion while high NOx emissions occur during the flaming phase. Should this analysis be done using non-linear approaches, e.g. Petron et al. 2004?

p. 16 Para 1. |to assess the constraints that these data provides on estimates of surface emissions of CO|. I think this is misleading, given that there were not enough analyses carried out in this study.

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