

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 #1

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General comments

The authors present an inversion of carbon monoxide emissions using satellite observations from TES and MOPITT. They show that these instruments yield similar emission estimates, including large enhancements over sub-equatorial Africa and Indonesia/Australia compared to the prior. They also demonstrate that the inversion might be affected by various types of systematic errors. The authors do a nice job in evaluating the consistency between CO observations from two different satellite instruments. However, I have a number of concerns regarding their analysis.

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1. The large increases in emissions over certain regions are not substantiated by independent data (comparisons with previously performed inversions for completely different periods cannot really be considered as such). I see two directions in which such substantiation could be achieved:
 - (a) Comparison with bottom-up non-climatological emission inventories.
 - (b) Comparison of the prior and posterior simulations with independent observations, e.g. ground-based FTIR observations, which should be available from Wollongong and Lauder sites.
2. The initial condition may have a large impact on the retrieved emissions, especially since the authors analyse only a short time period. How can it be excluded that strong emission enhancements are just a result of an underestimation of the initial CO field? In any case, the authors should compare their initial CO distribution with surface measurements, to show that they correctly capture the global distribution at the surface.
3. The authors discuss extensively the possibility of aggregation errors due to the very coarse resolution on which emissions are analysed. Indeed, with only 8 parameters in the control vector the risk of aggregation errors is large. I strongly encourage the authors to, instead of only presenting long discussions, assess the presence of aggregation errors by using smaller regions.

If the authors address these issues in a satisfying way, the paper may be acceptable for publication.

Specific comments

P17629, L10-12: The issue is addressed, for example, in Müller and Stavrakou (2005). This should be mentioned here.

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P17631, L21-24: I don't see how an improvement in the calibration algorithms can be a reason NOT to use V002.

P17632, L3-4: The low DFS may be a reason not to use the retrievals in fall 2005, but why not use retrievals in earlier 2005? A longer analysis period would strengthen the paper.

P17634, L14-17: To which specification of model error did Heald et al. (2004) compare the uniform model error?

P17635, L18: 1985 is very old. Is it reasonable to assume that emissions of 2004 can be described by a single scaling factor of the 1985 distributions?

P17636, L12-16: This way of presenting emission totals is highly misleading. Suppose that the prior emissions in November are low compared to the rest of the year, and that they are doubled in the inversion. Although this would correspond to a small emission increment, it would appear to be a large increment because the whole seasonal cycle is multiplied by the same factor. If the authors want to present per-year emissions, they should simply convert by applying a factor $366/N_{\text{days}}$, where N_{days} is the number of days in the assimilation window.

P17636, L15-16: The inversion covers two weeks in November. Although the observations in this period contain information on emissions in October, these are not optimized. Thus, the inversion does not 'provide a constraint on source estimates for October'.

P17636, L16: 'reflecting CO'. What does this phrase mean?

P17636, L24: The actual global mean OH abundance in the model is irrelevant to the fact that biases in OH lead to biases in emission estimates.

P17637, L2-20: The comparison of a two-week period in November 2004 with a 1-year period in 2000-2001 is quite meaningless.

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P17638, L11-14: I don't see why a higher-resolution inverse model than employed in this study should be expected to give better agreement between source estimates from TES and MOPITT. It may well be that there are small-scale biases between the instruments, which would result in differences between the source estimates in a high-resolution inversion, whereas they may have been averaged out in the present set-up.

P17638, L15-16: 5 to 28 November is not a whole month.

P17638, L17-19: Over Northern Africa the source estimates for the 'short' and 'long' MOPITT inversion differ by almost 50%. This cannot be called 'similar'.

P17638, L20: As pointed out above, the inversion does not constrain October emissions.

P17638, L20-22: Jones et al. (2003) provided statements on TES observations. These cannot be directly transferred to MOPITT.

P17638, L25: Maybe change 'local' to 'regional' since the transport scales meant here are quite large.

P17639, L2-7: Suggest referring to Gloudemans et al. (2006), who investigated long-range transport in the southern hemisphere based on model simulations and SCIA-MACHY observations.

P17639, L12-19: What causes the particularly small contribution of North-American emissions to CO concentrations in the middle troposphere? Is this feature restricted to North America or does it also hold for, for example, Europe? And if so, why are emission estimates for Europe not affected?

P17639, L19-23: Explain that these numbers relate to the posterior error covariance calculated from the inversion (as I assume). Maybe also give the formula for this calculation.

P17640, L3-6: If TES is not sensitive to North-American CO emissions, why are emis-

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sions adjusted in the inversion? Because of the prior constraint, one would expect unconstrained parameters in the cost function to be unaffected.

P17640, L21: What were the main differences in model configurations, and can these differences explain the contrasting source estimates obtained?

P17644: Two effects are important here, and these are not clearly separated. First, changes in CO emissions lead to changes in OH. Second, changes in CO emissions are likely to be associated with similar changes in emissions of other trace gases (e.g., NO_x), which also lead to changes in OH. The first effect is a direct chemical feedback. The second effect is an indirect (via emissions of other trace gases) chemical feedback. This should be better explained.

P17644, L1-5: This sentence is unclear. Did Müller and Stavrou show that jointly optimizing CO and NO_x emissions was better than optimizing only CO, or that using GOME NO_x and surface CO observations was better than using only surface CO observations?

P17646, L18: The fact that source regions are chosen independent of the spatio-temporal resolution of the observations is not a problem in itself. As long as proper prior error assumptions are made and the resolution of the state vector is high enough, there should be no problem. (For example: a grid-based approach can be fine, although the grid definition is certainly independent of the observations).

P17646, L29: These points have now been made several times. Text could be shortened here substantially.

P17653, Table 2: The a priori model simulation has a negative bias with respect to both TES and MOPITT for all regions. This overall bias will affect the emissions considerably. Have the authors checked the consistency of the prior model simulation with surface observations? (See also general comments). Have the authors considered correcting for a possibly existing general/global bias in the model or in the observa-

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tions?

P17653, Table 2: MOPITT has overall a considerably larger (in absolute magnitude) bias than TES compared to the prior simulation. Yet, the emission estimates from TES and MOPITT are relatively similar. How can this result be explained?

P17653, Table 2: The region 'Central Pacific Ocean' includes South America and a part of the Atlantic. Either change the definition or the name of this region.

P17655, Fig. 2: The total CO columns retrieved from TES and MOPITT differ considerably, where MOPITT is generally higher. I assume this is related to different averaging kernels and a priori profiles. It would help if the authors discuss this shortly.

P17659, Fig. 5: Is there a specific reason why the CO distribution is shown here in a different way than in Fig. 4 (instantaneous vs. 2-week average; 5 km vs. 8 km altitude; as a percentage of total CO vs. as CO concentration in ppb)? The point to be made (that North-American emissions contribute relatively little to CO concentrations in the middle troposphere) would become much clearer if one could compare directly with the regions shown in Fig. 4.

Technical comments

P17626, L20: reduce → remove/eliminate

P17626, L26: spatial → spatio

P17630, L5: Add 'a' between 'is' and 'gas'

P17630, L21: Write ratio with a slash instead of horizontal division line.

P17630, L20-23: Sentence contains twice 'and'. Suggest to split into two sentences, replacing first 'and' by 'It'.

P17631, Eq. (2): Not sure whether this formula should be repeated, since it is the same as Eq. (1). If authors want to repeat, add superscript 'TES' to ϵ ; similarly, add

superscript 'MOP' to ϵ in Eq. (1).

P17632, L5: Consider starting a new section here, since this paragraph relates to both TES and MOPITT.

P17632, L14: (for TES compared to MOPITT) \rightarrow (TES minus MOPITT).

P17633, L6: add 'error' between 'priori' and 'covariance'.

P17637, L3: are \rightarrow is.

P17638, L17: week \rightarrow weeks.

P17638, L18: remove 'data'.

P17638, L21: add 'observations' after 'TES'.

P17640, L9: observation \rightarrow observations.

P17641, L2: show \rightarrow shows.

P17641, L2: remove 'Asian'.

P17641, L11: fire-counts \rightarrow fire counts.

P17643, L26: Fig. 5 \rightarrow Fig. 4.

P17645, L3: remove 'the'

P17646, L21: vectors \rightarrow vector

P17653, Table 2: Be consistent in describing the latitude bounds of the regions. For example: change 0-5S to 5-0S; change 25-0N to 0-25N.

P17657, caption Fig. 3: proiri \rightarrow priori

P17658, caption Fig. 4: Units are in ppb \rightarrow Units are ppb. Same for Fig. 8.

References

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Gloudemans, A. M. S., M. C. Krol, J. F. Meirink, A. T. J. de Laat, G. R. van der Werf, H. Schrijver, M. M. P. van den Broek, and I. Aben , Evidence for long-range transport of carbon monoxide in the Southern Hemisphere from SCIAMACHY observations, *Geophys. Res. Lett.*, 33, L16807, doi:10.1029/2006GL026804, 2006.

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