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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Inverse modeling of CO_2 sources and sinks using satellite observations of CO_2 from TES and surface flask measurements" *by* R. Nassar et al.

Anonymous Referee #2

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This is a very interesting attempt to make use of TES retrieved CO2 for constraining CO2 sources and sinks. Overall the paper reads very well. The methodological section is very well worked out, including some innovative elements, such as the sub-annual variability of fossil sources and the 3D CO2 source from VOC oxidation. In the results section, my impression is that the enthusiasm about the contribution of the TES measurements is not fully supported by the actual results, as will be explained in more detail below. Nevertheless I am of the opinion that this study acceptable for publication, provided that the authors address the issues raised below.

GENERAL COMMENTS

In general, the TES data are found to be fairly consistent with the prior model and independent data. The main benefit is in the tropics, where the surface measurements provide almost no significant constraints. Looking at Figure 2, I am a bit surprised about this outcome, because the measurements show differences with the prior model of the order of ~5 ppm. It should take substantial source adjustments to overcome that difference. The question is why this doesn't happen. The answer is probably that the measurements receive a sufficiently low weight such that only a small correction of the 5 ppm gets corrected in the end. The current manuscript provides no information on the residual errors between the optimized model and TES. Because of this it is difficult to judge what the comparisons in Figure 6 mean for the performance of TES (TES a posteriori CO2 could still be far away from the actual TES measurements).

Looking at Figure 3, the differences between panel b) and d) are clearly limited to the tropical continents. It is known that the posterior uncertainties of flask inversions are large there. Because of this the difference with TES doesn't say much without uncertainty ranges. These are shown in Figure 5, although the difference between the flask only and combined inversion is very difficult to see. This makes it difficult to judge how much is really gained from TES.

I was surprised to see the similarity between all panels in figure 6 other than those for the TES-only inversion (bottom-right). The posterior solutions obtained using flask data remain very close to the prior. A reason for this is that the flask data were actually already used to derive the prior. Therefore the posterior solutions derived using flask data actually count that information twice. This may be one of the reasons why the impact of TES is small. Another reason lies in the use of fairly tight ocean priors. The problem there is that the Gruber estimates represent the multiyear mean ocean flux, i.e. they don't represent the uncertainty of the ocean flux in any specific year, which is larger due to inter-annual flux variability.

SPECIFIC COMMENTS

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Page 4274, line 11: diurnal sampling bias of SCIAMACHY and GOSAT Since the diurnal cycle amplitude of total column CO2 is expected to be below 1 ppm (see e.g. Olsen & Randerson, 2003) this sampling bias is not expected to be of relevance for Sciamachy and GOSAT.

Page 4273, line 25: What justifies the choice of threshold cloud optical thickness of 0.5?

Page 4277, line 20: The fact that a proper integration over the prior covariance matrix leads to unrealistic low results indicates that the underlying assumptions are not realistic. For one reason this can be due to the low prior uncertainties (as discussed already). Else it may not be realistic to assume that the prior uncertainties of the ocean fluxes are uncorrelated (they may be positively correlated).

Page 4280, line 10: Since the uncertainty is larger than the flux estimate itself for the African tropical forest it is not clear why the change in sign is considered a robust result.

Page 4282, line 11: Flux anomalies over Indonesia may well be obscured by cloudiness. The question is how many measurements that are expected to show signals of the biomass burning event that is mentioned survived the cloud filtering procedure.

Page 4285, line 13: Since riverine carbon is likely to be respired in the coastal zone a proper representation as oceanic flux requires that the coastal zone is resolved by the inversion. This is unlikely to be the case. Some confusion between oceanic and terrestrial fluxes in the coastal zone is inevitable.

Appendix A: The derivation presented here seems unnecessarily complicated. If I understand correctly the only assumption that is made is that the contribution of each sub region scales with its area. The sum needs to match the region integral. This yields a single equation with a single unknown.

Figure 2: Does the middle panel represent the prior or the posterior Geos-Chem model?

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Figure 4: It is unclear why negatives show up in the bottom panel. What is shown is dC/dE right? Then how can an increase in emission lead to a reduction in concentration?

Figure 5: The caption should provide a more detailed explanation of the legend. The legend itself is not self-explanatory. Even with help of the main text it is not easy to figure out what represents what.

Figure 6a, bottom left: Looking at the shape of the data cloud it is difficult to imagine that the regression slope is smaller than 1. Is the listed number of 0.872 correct?

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Interactive Comment