**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.

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This is a very interesting attempt to make use of TES retrieved CO_2 for constraining CO_2 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 CO_2 source from VOC oxidation.

We thank the referee for these positive comments.

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.

Justification and modifications to our interpretation of the results are given below as well.

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 CO_2 could still be far away from the actual TES measurements).

The referee makes some valid points here. With regard to the comment about Figure 2, it is important to keep in mind that only two months were shown from our annual inversion, so one should not attempt to draw conclusions from this figure as originally presented. To remedy this, we have expanded and updated the figure with 4 averages, each of 3-months in duration (essentially seasons). This gives a more complete picture of the TES and model values. We opted not to show monthly averages which would result in very many panels and showing an annual average would obscure the details. (The difference plots were moved to Figure 4 to facilitate comparison with the Jacobians.) We would like to emphasize that GEOS-Chem (and transport models in general) give smoother CO_2 distributions than observations around 5 km altitude, hence the differences that the referee notices are both positive and negative, reflecting to some degree noise in the TES retrievals, which is part of the reason that the flux

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adjustments are not extremely large.

Looking at Figure 3, the differences between panels 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.

As an example of the uncertainty reduction, the results for Central America and the Caribbean in Figure 5 may be helpful to highlight. Here we see that uncertainty reduction from the precise but sparse surface measurements and from the TES CO_2 observations is approximately equal for this region, but combining these data sets, gives more information, a further reduction in uncertainty, and a slight shift to a stronger source.

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.

The referee is correct that flask data have been used in the inversion that our prior is based on, but that was a 10-year climatology (1991-2000), whereas the flask data that we use in the inversion come from a single year (2006) that was not part of the prior. This means that no specific measurement was used twice, but of course the spatial coverage of the measurement sites in each case is very similar. The referee is also

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correct that the Gruber et al. (2009) estimates represent a multi-year mean ocean flux and we have used those uncertainties to represent the uncertainty for a single year. It is not possible to quantitatively derive the uncertainties for a single year from the Gruber et al. results, but surely these would be larger than the multi-year values. Our justification for using the multi-year values is the need for tight constraints on the ocean flux to reduce the bias toward a weaker ocean sink that persistently occurs in global inversions due to the use of predominantly background values. A discussed in Section 3.4, this problem is not unique to our analysis. The Gruber et al. values were therefore chosen as a reasonable set of a priori uncertainties to minimize this effect and target our effort at constraining terrestrial biospheric fluxes, which have larger uncertainties. In future work, alternate approaches to dealing with this issue will be considered.

SPECIFIC COMMENTS

Page 4272, line 11: diurnal sampling bias of SCIAMACHY and GOSAT Since the diurnal cycle amplitude of total column CO_2 is expected to be below 1 ppm (see e.g. Olsen Randerson, 2004) this sampling bias is not expected to be of relevance for SCIAMACHY and GOSAT.

The diurnal cycle amplitude for XCO_2 in Olsen Randerson (2004) is estimated to be 1 ppm over the forests near Park Falls, WI and other dense forest areas, although it is not clear if the impact of the diurnal variation in boundary layer height was adequately modeled since this is particularly challenging for models to reproduce. In situ surface data suggest that there would be a weaker CO_2 diurnal cycle where vegetation is sparse (Higuchi et al. 2003, Regional source/sink impact on the diurnal, seasonal and inter-annual variations in the atmospheric CO_2 at a boreal forest site in Canada, Tellus 55B, 115-125), yet other work suggests that there would be a stronger diurnal cycle over some agricultural areas (corn in particular) during the growing season (Corbin et al. 2010, Assessing the impact of crops on regional CO_2 fluxes and atmospheric concentrations, Tellus 62B, 521-532) and perhaps other vegetation types. Although the precision targets for GOSAT and SCIAMACHY are larger than 1 ppm, we do not think

that a bias of 1 ppm due to the diurnal cycle is irrelevant.

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

This value was selected to balance the need to reject measurements with cloud/aerosol contamination yet to avoid rejecting too many measurements since TES sampling is relatively sparse for a nadir sounding satellite instrument. This cloud optical thickness threshold also seemed a reasonable choice based on our TES CO_2 validation efforts, however a lower threshold could perhaps result in less variability in the TES CO_2 data.

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).

We agree that the real fluxes will have spatial correlations but the uncorrelated/diagonal covariance matrix assumption is commonly used because it greatly simplifies the mathematics. Furthermore, as we discussed in Section 3.4, more sophisticated inversions, which do account for the spatial correlation, also produce biased ocean estimates. Quantifying the exact impact of this assumption on our analysis is beyond the scope of this paper.

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.

We are claiming that the lack of a strong sink is a robust result based on the consistency between the different members of our small ensemble, which all show a source. It would of course be more robust if the error ranges on the results for this region were smaller.

Page 4282, line 11: Flux anomalies over Indonesia may well be obscured by cloudi-

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ness. 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.

The referee makes an excellent point here. The biomass burning emissions plume containing elevated CO_2 could also contain much cloud and aerosol such that measurements are either rejected or perhaps have a partially obscured signal, preventing the inversion from properly identifying the event. A detailed analysis of this region and the effect mentioned is beyond the scope of this study, but we now acknowledge this possibility in the text of section 3.1.

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.

We agree that our inversion would not be able to resolve the coastal zone, but it is not clear if the riverine carbon would necessarily be released in the coastal zone or if the riverine water would mix with the ocean and release the carbon farther away and over a longer time frame.

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.

Although the detail provided in the appendix may not be entirely necessary, those equations outline the steps applied to aggregate uncertainties from smaller regions to larger ones and disaggregate uncertainties from large regions to smaller ones. This appeared as an appendix because it was not central to the research findings, but since the approach is already documented in the discussion paper, we believe that to be sufficient and have removed the appendix for the revised manuscript, replacing it by one brief new sentence added to section 2.4.

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

The middle panel in the original figure represents the a priori GEOS-Chem distribution transformed with the TES averaging kernels and TES a priori profiles. We have now modified this figure to show 4 three-month seasonal averages rather than just 2 months of the year.

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?

The Jacobian **K** was defined immediately after Equation 7, as **K** = $\partial x^m(u)/\partial u$, where u is the surface flux and $x^m(u)$ is the modeled atmospheric CO_2 concentration, which is a function of the surface flux. The Jacobians are provided for the a priori fluxes, which are not homogenous for each region. In the specific case of the African Tropical Forest region, the negative node of the Jacobian arises from the fact that this region encompasses area from two separate regions in the Transcom inversion (Baker et al. 2006b, GBC), which is a major component of our a priori. The Baker et al. (2006b) inversion estimated African fluxes as a source north of the equator and a sink south of the equator. Those results, which were used to derive our prior are also depicted in the lower panel of Fig. 2 of Nassar et al. (2010, GMD) after removing the contribution from biomass burning. Since we have defined different inversion regions than Transcom, the instantaneous prior flux for a given gridbox can have a different sign than the annual average for the region. We remove this panel in our revisions and instead use Figure 4 to emphasize the Jacobian and TES-model differences that impact the South American Tropical Forest flux.

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.

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Based on the referee's comment, we have now added a more detailed description to this caption to clarify our legend.

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?

The calculation for the slope was verified and the value of 0.872 is correct. Although the cloud of data points appears to have a slope greater than the 1-to-1 line when the figure is viewed without clear detail for those points, enlarging the panel on one's screen (i.e. 600

Upon investigation of the slope and verifying our calculation, we did however notice that our units for the variance, which were given as ppm, should have been $(ppm)^2$. Rather than use $(ppm)^2$, we instead report the root of the variance or standard deviation, which has the units of ppm since this is has a more understandable meaning.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 4263, 2011.