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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 #1

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The manuscript of Nassar et al., "Inverse modelling of CO2 sources and sinks using satellite observations of CO2 from TES and surface flask measurements" covers a highly relevant topic, namely to use an established approach applied to new satellite data to obtain information on regional CO2 surface fluxes. The manuscript is well written, important new aspects are covered and the topic is highly appropriate for ACP. I therefore recommend its publication after the major items discussed below have been considered by the authors.

I am not entirely convinced about the conclusions of the study that in fact new robust knowledge about regional CO2 surface fluxes has been obtained from the TES

retrievals as, for example, stated in the Abstract.

There are indications that the TES retrievals suffer from biases. For example Figs. 6a and 6b show that the agreement of the model with the independent observations is significantly degraded if only TES is assimilated compared to the free running model and if only flasks are assimilated. The authors point out that the agreement is best if flasks and TES are both assimilated but as one can see from the scatter plots, the changes are very small so that it is hard to see a difference between the scatter plots where only the flasks are assimiated and the flasks and TES is assimilated (I agree that the metrics are better but only marginally; I would not over-interprete the small changes of the metrics). The changes are however large if only TES is assimilated. This indicates that the TES data have only a marginal influence on the assimilation which is dominated by the flasks. This also indicates that the TES data and the flasks are not consistent. In addition, there is a large difference between the TES retrievals and the model as one can see from Fig. 2. Please comments on this. Can TES biases be excluded ? This is critical as small differences (biases) will result in large flux changes. Please also show (add) more maps: at least the model with flasks assimilated (e.g., added to Fig. 2; see also following comment).

Concerning the regional results shown in, e.g., Fig. 3, it is nor clear for me why the TES data suggest a sink for "S. American Tropical Forest". A-priori this region is a strong source. Assimilating the flasks results in a weaker source. Adding TES turn this into a weak sink, which is highlighted in the abstract as one of the major findings of this study. Only TES retrievals over the ocean are used. According to the Jacobians shown in Fig. 4, "S. American Tropical Forest" CO2 flux information is transported westward over the ocean where the TES observations are obtained. This indicates how TES obtains information for a land region although only ocean observations are made (used). Fig. 4 also indicates that this transport depends on the month and is larger in March compared to December (at least in 2006). Nevertheless, the prevailing wind direction seems to be westward. Looking at Fig. 4, TES observes elevated CO2

over the ocean west of "S. American Tropical Forest" (at least in May 2006 but also in Nov as one can see from the difference plots), whereas the free running model (using a-priori fluxes corresponding to a strong source) does not show such a regional enhancement. Just from looking at Fig. 4, I would conclude that TES suggests that the "S. American Tropical Forest" region is a stronger source compared to the a-priori fluxes used by the model. However, the inversion suggest the opposite. This may be due to the fact that the time period as selected for the various plots are different but this may also indicate a problem with the inversion. Please provide better evidence by showing (comparing) TES retrievals and model results (similar as done in Fig. 2 but for, e.g., annual and seasonal averages) that the TES retrievals in fact point to weaker "S. American Tropical Forest" fluxes (at least qualitatively - to do this quantitatively is the job of the inversion but I would like to see a more direct evidence of this major finding to ensure that the result in not an artifact of the mathematical machinerie of the inversion).

I am also concerned about the large source as suggested by the TES only inversion for the region "North American Boreal Forest" shown in Fig. 3c. There are not TES observations even close to this region. Despite this the response of the inversion is quite strong. Please clarify how this can happen and provide evidence that this is a result to compensate for regional TES biases or model transport errors.

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