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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A Global Synthesis Inversion Analysis of Recent Variability in CO_2 Fluxes Using GOSAT and In Situ Observations" *by* James S. Wang et al.

Anonymous Referee #1

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This paper, entitled "A global synthesis inversion analysis of recent variability in CO2 fluxes using GOSAT and in situ observations" by Wang et al., combines satellite and ground-based observations of CO2 concentrations to infer sources/sinks of carbon over the world.

One of the unique aspects of this work is the adoption of a batch Bayesian synthesis inversion approach that is at higher spatial (perhaps; see comments below) and temporal resolution relative to conventional global inversions that allow full-rank posterior error correlations to be evaluated. However, I have major questions about the results, and I found the conclusion about GOSAT observations to be weak. Therefore, I recommend major revisions prior for consideration for publication in ACP.

MAJOR COMMENTS I. Utility of satellite observations The authors carried out extensive comparisons between inversion results using in-situ data versus those from using the GOSAT satellite data, as well as both data. The results often differed dramatically, and the comparisons against independent aircraft data highlighted potential biases in the GOSAT data. I am left wondering whether it is worth assimilating the GOSAT data at all, given the fact that the inversion-derived spatial and temporal patterns of carbon fluxes could be highly erroneous, with significant biases that are not reflected in the posterior errors (see point II. below). After highlighting several issues with GOSAT in the Discussion and Conclusions section, the final paragraph was anticlimactic and weak. Can the authors provide a broader guidance on how the GOSAT data can be properly used (if at all)? Is there an important take-home message that this study can provide to the reader? A clear, concise message is lacking in this lengthy paper.

II. Uncertainties (prior and posterior errors) Several issues arose as I considered how the prior and posterior errors were dealt with in this paper: 1) More quantitative information regarding the prior "observation" uncertainties at different sites based on the "model-data mismatch" would be helpfulâĂŤe.g., Table or a map of the prior errors at different sites. 2) The handling of transport errors requires more discussion. The authors adopted a single transport model (instead of an ensemble of transport models as some other studies do), PCTM, and assumed that the "model-data mismatch" accounts for the transport error. How did PCTM perform as part of previous TRANSCOM experiments, and other tracer experiments (e.g., SF6)? Its interhemispheric mixing? How about its vertical mixing and rectifier strength? Where does PCTM fit within the well-known "diver-down" figure in Schimel et al. [2015] (www.pnas.org/cgi/doi/10.1073/pnas.1407302112; its Fig. 3)? 3) What is the logic behind neglecting a priori spatial and temporal error covariances? The stated reasoning is "The neglect of a priori spatial error correlations is justified by the size of our flux optimization regions, with dimensions on the order of one thousand to several thousand km, likely greater than the error correlation lengths for our 2° x 2.5° grid-level fluxes. For example, Chevallier et al. (2012) estimated a correlation e-folding length of ∼500 km for a grid size close to ours of 300 km x 300 km based on comparison of a terrestrial ecosystem model with global flux tower data." If the inversion for flux adjustments is carried out at scales of ∼1000 x 1000 km, following the TRANSCOM regions, this is imposing an artificial error covariance lengthscale of ∼1000 km (significantly larger than the ∼300 km lengthscale), thereby resulting in aggregation errors. Also, given this coarse lengthscale, can the authors make the claim that the adopted inversion is really high spatial resolution? If 2° x 2.5° grid is considered high resolution, this is not much different from CarbonTracker's spatial gridding. 4) Are the prior uncertainties of proper magnitude? While the authors enlarged the observation uncertainties by a factor of 2 to lower the normalized Chi-squared value closer to 1, Table 2 shows that the in situ only inversion results in a Chi-squared of 4.0, which suggests that the prior uncertainties may still be underestimated. Should the prior uncertainties be inflated even more? 5) Can we place much faith in the posterior errors for calculated sources/sinks that are derived through the inversion? There are several cases in the paper where different inversions yield different carbon fluxes whose values do not overlap even if allowing for the posterior error bars. Since there is only one true carbon cycle, the posterior errors should allow for the "true fluxes" to be encompassed within the error bars even under different inversion setups. How much trust (if any), therefore, should we place on the posterior errors? I realize that other global inversion studies often face the same situation of underestimating posterior errors, but this would be an opportunity for the authors to comment on the posterior errors.

III. European sink The large European sink of -1.5 PgC/yr is difficult to reconcile with ground-based information. While the large sink could be caused by biases in GOSAT data, as pointed out by the authors, I am puzzled by the fact that the summertime drawdown is so large in the prior fluxes as well, reaching fluxes of -5 PgC/yr and comparable with the summertime drawn in boreal and temperate North America. Is there really enough biomass/vegetation to sustain a summertime drawdown of -5 PgC/yr in

Europe? Also, I suggest the authors consider and cite a recent paper focusing on the European sink [Reuter et al., 2017, BAMS; http://dx.doi.org/10.1175/BAMS-D-15-00310.1].

OTHER COMMENTS Length of text: This is a long paper, with the main text reaching over 40 pages (double-spaced). The Results section as a whole felt unnecessarily long. Can the authors seek to condense?

Spatiotemporal resolution: From the start of the paper the fact that the inversion is at “high resolution” is mentioned numerous times. This is supposedly a strength of the inversion technique adopted in this study. However, it is not until deep in the Methodology section that the reader finds out exactly what resolution is adopted in both space and time. Can the authors specify exactly what the resolution is early on (even in the Abstract)? Also, can the spatial gridding really be considered to be high resolution (see comment above in comparison to CarbonTracker)?

Lines 462-464: It appears that the in-situ only inversion also produces winter-time uptake in Boreal Asia (Fig. 6). This is not just a feature in GOSAT-based inversion. Why?

Figs 8 and 12: The different colored bars are hard to distinguish. I suggest separating out the Global flux numbers as a separate panel and zooming in the figure to a smaller range for the regional results.

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Discussion paper