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Interactive Comment

Interactive comment on "Comparing the CarbonTracker and TM5-4DVar data assimilation systems for CO_2 surface flux inversions" *by* A. Babenhauserheide et al.

Anonymous Referee #2

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This paper presents an inter-comparison of two data assimilation (DA) systems, CarbonTracker (CT) and TM5-4DVar, for CO2 flux estimation. The authors recognize that it is necessary to harmonize the inputs to both systems in order to interpret differences in the resultant flux estimates and assess the relative strengths and weaknesses of the two DA approaches. Sensitivity tests related to the assimilation window length for CT and the observation coverage are carried out to further evaluate the response of the two DA approaches to these parameters. The manuscript is limited in its scope, however. The final conclusions do not add any new knowledge about: (a) the performance of ensemble or variational systems for carbon flux estimation at high resolutions, (b)

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why the carbon community should (or should not) prefer a particular system, especially with the recent availability of remote-sensing data, and (c) the global/continental carbon budget, associated uncertainty reduction from the two DA approaches, and more importantly, which approach actually provides more accurate estimates? These are the types of questions, for example, that the community is interested in. At a minimum the authors should address/consider the following comments before the manuscript is acceptable for publication in ACP.

MAJOR COMMENTS

1. Specification of background error covariance (B) matrix – By the authors' own admission (Pg. 8896, Line 24), it is not possible to get an exact match of the flux uncertainties. This statement is unclear - it is imperative to clarify that this maybe because the authors chose to implement "out-of-the-box" versions of the two approaches. From a data assimilation standpoint, one can and should specify the same initial B matrix (i.e., same spatial and temporal correlation length, same uncertainties) for both the ensemble and the variational system. Once the structure of the B matrix is prescribed, I agree that it may not be a trivial task to revise it to specific lat/long grids (for TM5-4DVar application) or aggregate to broad-scale ecoregion/vegetation types and then generate the ensemble members (for CarbonTracker application). However, the background error covariance plays a critical role in filtering and spatially spreading the information from the observations. Discrepancies between the structure and setup of this matrix impacts interpretation of the differences between the flux estimates. For example, a potential reason for the South American flux anomaly (Section 5.1.1) may be due to the misspecification of prior flux uncertainties in case of TM5-4DVar, which results in a high weight being given to the set of observations from ABP. The authors acknowledge this in an indirect way by highlighting the outlier-detection framework of CT; but again the fundamental basis for that outlier-detection criterion is related to the spread in the ensemble, and thereby the background error covariance matrix.

To resolve this issue, the authors should consider either of the following – (a) (ideal

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scenario) attempt to specify the same initial background error covariance for both systems and do a sensitivity analysis (for one year) to evaluate the impact on the flux estimates, or (b) (practical scenario) if it is realistically not possible to modify the initial B, then clearly state that as a drawback of the way the systems were implemented and make an argument as to how differences in B may manifest in the differences seen in the flux estimation results. In its current form, the study completely overlooks the role that the B matrix plays even though it is one of the important inputs that should have been harmonized for such an inter-comparison study.

2. Motivation of the study and novelty – This study compares carbon fluxes estimated from two different DA systems (CarbonTracker and TM5-4DVar) at aggregated scales. The authors need to make a better case for motivating why such a study is necessary and what new information it provides in terms of improving our understanding of the applicability of DA systems for carbon flux estimation purposes. By reporting the comparison of the posterior flux estimates (but not the associated uncertainties) at aggregated spatial and temporal scales, it is difficult to judge the performance of the two systems at finer spatiotemporal scales, for e.g., biomes/ecoregions, Transcom-scales. Expectedly at aggregated scales both the DA approaches provide similar estimates, and it is not clear what new knowledge, if any, the carbon science community stands to gain from this study.

The authors claim that their primary goal is to evaluate the impact of the inverse method on the "accuracy of the estimated fluxes" (Pg. 8887, Line 17-18). Without any comparison of posterior uncertainties, however, it is difficult to back this statement. Given that the authors have used the in situ network, there may be value in comparing these flux estimates to existing studies from the literature. Have the authors compared their global/continental estimates to other studies over the same time period? Alternatively, the authors can report results at the Transcom3 regions, which may highlight additional regional differences between the estimates from the two DA systems. These are a few possible additional analyses/tests that will add value to the manuscript and make it

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scientifically relevant to the carbon science community.

MINOR COMMENTS

1. Pg. 8884, Lines 13-14 – In the abstract the authors claim that one of the sensitivity tests will include impact of operational parameters "such as temporal and spatial correlation lengths". However no sensitivity tests are presented in the manuscript to justify this statement. Revise.

2. Pg. 8885, Line 16- Differences in assumptions about error covariances (both model-data mismatch and prior) contribute significantly to the differences in flux estimates from different studies. This point needs to be acknowledged here.

3. Pg. 8887, Lines 15-16 – This statement and the associated references need to be revised. For e.g., TM5-4DVar nor CarbonTracker as used in this study took part in the Transcom experiments (Gurney et al. [2004] had TM3 though). Similarly, TM5-4DVar as used in this study wasn't part of the suite of atmospheric inversions used in the Schulze et al. [2009] study. Kindly check the use of references here, and throughout the manuscript.

4. Pg. 8893, Lines 14-15 – For all purposes, the appropriate reference here should be Fisher and Courtier [1995] for showing the feasibility of eigenvector based approximation methods (see http://old.ecmwf.int/publications/library/ecpublications/_pdf/tm/001-300/tm220.pdf). For implementation purposes, the appropriate references are Meirink et al. [2008] and Chevallier et al. [2005].

5. Pg. 8903, Lines 24-25 – It is not clear what the authors mean by –" ..approaches uncertainties from above, . . .". Clarify.

6. Pg. 8906, Line 1-2 – This statement is unnecessary for this portion. Delete.

7. Pg. 8908, Lines 19 – The word 'adjustment' is misspelled.

8. Pg. 8908, Lines 22-23- This statement is unclear. Do the authors mean to say that

potential flux adjustment for ecoregions can be constrained well by a single site? And hence CT does better than TM5-4DVar? Overall the discussion in this paragraph was difficult to follow.

9. Pg. 8909, Line 7 – Replace “observations” by “observational network”.

10. Figures 7, 9, 11 – For the benefit of the reader, it would be better to stick to a single flux unit (such as PgC/region/year) throughout the manuscript. Note that this will require edits throughout the text as well.

11. Figures 7 and 9 –The CT simulations are represented in yellow lines with bars on both ends. Do these bars represent the posterior uncertainty? How are these posterior uncertainties calculated for the CT simulations? If these bars do not represent the uncertainty estimates, then I would suggest using a different symbol/marker to avoid confusion with the other figures.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 8883, 2015.