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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Influence of CO_2 observations on the optimized CO_2 flux in an ensemble Kalman filter" *by* J. Kim et al.

Anonymous Referee #1

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This study evaluates the influence of CO2 observations on the analysis of CO2 surface fluxes. The influence matrix concept, which is routinely used within the NWP community, has been employed to assess the benefit of different surface observation sites within the CarbonTracker framework. The novelty of this study is in its application to carbon science since the specific tools/methods discussed here are well established.

General Comment: It seems that a few choices and assumptions (and accordingly the final results) are very much tied to the CarbonTracker setup that the authors have used. Hence, the conclusions may not be reflective of the performance of a generic ensemble Kalman Filter in which the lag window size, localization, inflation parameters etc. can be tuned. In fact there is no discussion of inflation in Section 2.2. The following comments are intended to provide the authors with a few starting points that can make

the study more appealing to the general carbon data assimilation, and not just the CarbonTracker, community.

Specific Comments:

1) By the authors' own admission, a lag window of 5 weeks may not be sufficient to optimize the surface CO2 flux in Asia (Section 3.3.3). This raises two main questions: a) Why didn't the authors use a lag window of more than 5 weeks? Bruhwiler et al. [2005] (Figure 1 in their paper) showed that for some of the remote sites, the lag window might need to be in the order of months. 5 weeks is suboptimal in that respect, and may very well be the reason why the SH (and a few of the MBL) sites seem to provide little to no information (Figure 8). Can the authors show some sensitivity tests when the lag window is increased beyond 5 weeks? Or is this not feasible given the CarbonTracker setup? If the latter assumption is true, then this drawback needs to be clarified early in Section 1. b) The authors repeatedly claim that the cumulative impact over five weeks would be greater than the average self-sensitivity of 4.8%, which is calculated over the most recent assimilation cycle (i.e., one week). But no quantitative value is provided for this 'cumulative impact'. In general, an ensemble Kalman filter is designed to propagate the covariances in time, and hence the cumulative impact can be calculated over the entire analysis period and not just the most recent assimilation cycle. Again if this is an artifact of the Carbon Tracker setup, then this needs to be clearly stated. Or else the authors need to provide magnitudes for the cumulative impact of the observations.

2) Figure 4a – it is particularly curious that the self-sensitivity of the MBL sites are the same as the self-sensitivity of the Difficult sites. In Section 3.2.1, the authors argue that the spread of the analysis CO2 concentrations is small at the MBL sites. But they have to be an order of magnitude lower to compensate for the fact that the model-data mismatch values at the MBL sites are 10 times lower than the model-data mismatch values at the Difficult sites (based on Table 2). Can the authors show a time-series of how the spread in the analysis CO2 concentrations compare between these two sets of sites? Are the spread in the analysis CO2 concentrations that different during the

NH winter months? Or is it because that the assimilation system is unable to use the information from the MBL sites, given the constraints on the lag window size?

3) Does the calculation of the influence matrix take into account systematic errors in the observations? Or are the authors implicitly assuming that the observations do not have systematic errors? If so, why is this a valid assumption?

4) Section 2.3 – This section mirrors Section 2 in Liu et al. [2009] very closely. But it skips an important assumption, i.e., Equations 16 and 17 assume that observation errors are not correlated. This needs to be added in the text.

5) Section 3.1 is called 'validation' but it is unclear what is being 'validated' in this section. Liu et al. [2009] had a similar section titled 'validation' but in that study different data-denial experiments were proposed. Have the authors considered data-denial experiments to better demonstrate the applicability/utility of this influence matrix approach for the carbon flux estimation problem? The authors should show some sensitivity experiments using the data-denial approach, especially to bring out the value of MBL vs Difficult sites.

6) Section 3.1 – Why do the authors claim that the self-sensitivity in EnKF should have a value less than one? Can the authors justify this statement? Further, Lines 15-18 need to be rephrased as it currently gives the impression that when the analysis error covariance in 4DVAR is calculated using the inverse of the Hessian matrix of the cost function, then this being an approximate method will result in self-sensitivity values greater than one.

7) My biggest disappointment is that the quality of the optimized CO2 fluxes has not been assessed. Some robust ways of evaluating the posterior CO2 fluxes (i.e., comparison to biosphere model output, comparison of posterior CO2 concentrations to independent datasets like aircraft observations etc.) would have been beneficial for the reader. Only the uncertainty reductions are presented in Figure 7. Additionally, the color bar should be different for JJA and DJF to bring out the uncertainty reductions for

DJF. The same recommendation applies for Figure 12.

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