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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of Siberian observations on the optimization of surface CO_2 flux" *by* J. Kim et al.

Anonymous Referee #2

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This study evaluates the influence of additional CO2 observations (from the JR-STATION towers) on the analysis of Eurasian and global CO2 surface fluxes. The novelty of this study is in using these additional tower observations, which have not been used within an inverse modeling study before. The results demonstrate that these observations do have a certain amount of impact, namely it adjusts the flux patterns and magnitudes between Eurasian Boreal (local) and other NH land regions (non-local). This is expected based on the way an inverse modeling system works, especially the resultant interplay between the observation density/network and the prior weighed by their respective covariances. This is not a novel finding in itself. What is of greater interest, are the adjustments that are made to the surface fluxes and whether they are correct or not (especially the reduced sink in Eurasian Boreal region). No independent evaluation, either of the posterior CO2 concentrations or the posterior fluxes with any kind of independent data has been provided, however. The authors have compared their flux estimates to a suite of previous studies. But these studies cover different temporal extent (i.e., span across a wide variety of years), and second all of the estimates fall within the reported uncertainty bounds. There is no rationale behind the authors claim that the flux estimates from the JR experiment are more comparable to the previous studies than the CNTL experiment (Page 14, Lines 4-5). It is also highly misleading that in Section 3.2 the authors show results comparing the posterior CO2 concentrations to the observations that are being assimilated in the first place. Finally in Section 3.3, the uncertainty reduction should be calculated individually for the CNTL and the JR experiments relative to the prior uncertainties that were specified. It is again misleading to compare two posterior uncertainties (without knowing which one provides a baseline) and call this calculation as an "uncertainty reduction".

The following points provide a checklist on critical sensitivity tests/issues that should be addressed to first validate the results presented in this study, and thereby make it relevant and appealing to the carbon science community.

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1) Evaluation of posterior CO2 concentrations with independent data – This is the most important step that is missing from this study. This should be done either by comparison with independent data or via data denial experiments. In the latter case, specific set of in situ observations that are common to both CNTL and JR experiments may be held in reserve (i.e., those data should not be assimilated into the CT system). The posterior CO2 concentrations from the two experiments should be compared to this independent data both qualitatively and quantitatively.

2) Uncertainty estimates associated with the analyzed flux estimates – On Page 9, Lines 13-14, the authors claim that the "...uncertainty is calculated as one-sigma standard deviation of the fluxes estimated, using Gaussian errors". It is unclear why the authors choose this approach when they are using an ensemble Kalman filter based

system, where they should be able to directly recover the posterior uncertainty over the entire time period. Why is such an ad-hoc approach used to calculate the uncertainty? What is the basis for this approach?

3) Reduced uptake estimated in EB between 2002-2009 – Possibly the real significant finding from the additional JR-STATION tower observations are that the overall magnitude of the uptake reduced in Eurasian boreal region during NH summer. This is a reasonable conclusion for the summer of 2003 (anomalous drought for this year) but the authors claim a consistent reduction averaged out over the entire 2002-2009 period. The authors do not address any underlying mechanism for this difference in uptake from the two experiments. Is this simply an artifact of the inverse modeling setup, interplay between data density, error covariances, etc.? Or are there changes in vegetative activity that took place during this period in the Eurasian Boreal region and the JR-STATION tower observations were able to observe those local changes. The authors need to provide some form of mechanistic understanding for their inverse modeling results.

4) Prior flux estimates and associated uncertainty used throughout the study – For Figures 4,5 and 6 the authors should add the prior flux estimates (say green or gray bars/lines) to the figures. For the uncertainty reduction reported in Section 3.3, the authors should use the prior uncertainties as a baseline and compare the posterior uncertainties from their two experiments.

5) Section 3.3 self-sensitivity calculation – It is slightly counter-intuitive that the single JR-STATION tower that is located at $\sim$60N, 130E provides the same influence on the analyses as all the other set of JR-STATION towers that are clumped together between 60-90E. As per previous studies (Cardinali et al. 2004, Liu et al. 2009, Kim et al. 2014), typically there is a negative correlation between the self-sensitivities and the spatial density of the observations. Can the authors comment on why that one single tower observation does not provide higher influence than a cluster of towers together?

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Discussion paper

6) Minor comments- (a) Kindly check the spelling of 'Eurasian Boreal' in Figure 5A. (b) The color scale for Figure 7 should be modified – either a linear increase or use something analogous to a log scale. Currently it jumps from a scale of 34-36 to 70-75. (c) For Section 3.4 and Table 5, the authors should choose a set of studies spanning the same spatial domain, temporal extent, space-time resolution at which fluxes are estimated and then compare to their estimates from the CNTL and the JR experiments. This would help out bring out the main message in this section.

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