**Atmospheric
Chemistry
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Discussions

Interactive comment on "Constraining terrestrial ecosystem CO2 fluxes by integrating models of biogeochemistry and atmospheric transport and data of surface carbon fluxes and atmospheric CO2 concentrations" *by* Q. Zhu et al.

Anonymous Referee #1

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The study by Zhu et al. attempts to combine bottom-up and top-down inverse modelling to estimate carbon dioxide surface-atmosphere fluxes. First, Zhu et al. constrain a process model of the terrestrial biosphere with eddy-covariance flux measurements at site level. Then, they extrapolate to CO2 surface fluxes on the global scale using the previously optimized process parameters. And finally, they feed the gridded fluxes as prior constraints into an atmospheric inversion driven by in-situ and satellite remote sensing measurements of atmospheric CO2 concentrations. The study finds that using the prior constraints from the optimized process model improves the match between

modelled a posteriori concentrations and independent validation data in comparison to prior fluxes which are not optimized by the process model.

The paper is of interest to the atmospheric sciences community focusing on surface-atmosphere exchange of CO2 and the related modelling approaches. The employed methods and the respective discussion appear mostly robust though some refinements are required before publication in ACP is recommended.

1. Estimating a state vector through sequentially applying (Bayesian) statistical methods is a promising approach to exploit the information content of observations with different constraint characteristics. The paper, here, combines direct flux measurements with atmospheric concentration measurements. However, it is not a 'clean' case since state vector of the first step are process parameters (from which surface fluxes are calculated), while the state vector of the second step are surface fluxes.

Would it be worthwhile to shortly discuss the theoretical, statistical background of sequential estimates?

One of the major advantages of sequential estimates is that the second step can identify its constraint matrix with the a posteriori covariance matrix derived from the first step. The paper, however, does not use the full covariance matrix but only the variances.

Please comment on how your approach is actually different from just using a better a priori state vector for the top-down approach.

2. The state vector of the top-down approach only includes terrestrial ecosystem fluxes (p. 22597, l.18; Figure 1). I would expect that atmospheric concentration measurements also exhibit some (albeit limited) sensitivity to ocean fluxes. Ocean fluxes are imposed. How sensitive are the estimated biosphere fluxes to ocean fluxes being potentially different from the imposed values?

3. What is the assumed observation error for the atmospheric CO2 measurements?

Does it include a representation error?

4. The validation of the a posteriori concentration fields and the respective discussion should be refined. So far, it is mostly limited to comparing monthly averages at 6 surface sites plus the zonally averaged CONTRAIL data. How are the inland sites selected? Are they seasonally affected by small scale meteorological variability or are they really representative of continental regions? Showing time series of model-measurement comparisons and the assumed measurement errors might help.

- Figure 5 and 6: It would be good to add bars for the fluxes calculated from the a priori parameters.

- Table 4: I would prefer seeing a bar chart instead of a table with the different fluxes including the a priori fluxes.

- Figure 7: The axes scales are too coarse for some of the subpanels; units missing. Actually, I wonder whether the y and x axis are accidentally swapped. If I am not mistaken, there should be 2 model values (GC-TEM, GC-CASA) for each observation value. The figure looks like the other way around.

- section title 3.2.3: CO -> CO_2

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