Geosci. Model Dev. Discuss.,
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Geoscientific Model Development Discussions

Interactive comment on "Estimating Surface Carbon Fluxes Based on a Local Ensemble Transform Kalman Filter with a Short Assimilation Window and a Long Observation Window" *by* Yun Liu et al.

Anonymous Referee #2

Received and published: 23 August 2018

[General comments]

It was really fun to read this informative manuscript which well describes its goal and methodologies. Authors introduce interesting methodology to use different length of observation window (OW) from that of assimilation window (AW) for estimating surface carbon fluxes (SCF) which does not have enough observations to be well constrained. However, it would be great to improve the manuscript responding to the following points. 1) This study does not assimilate other available observation datasets of atmospheric CO2 such as GV+, GOSAT, etc. Authors need to explore a possible

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sensitivity of AW/OW lengths to the observation density. Since the current experiments includes column mixed OCO data only, you may need much longer OW. If you include more observations like GV+ (direct information, not like column-mixed information) and GOSAT, it may results in quite different RMSEs from AW/OW length experiments (Table 1). One can guess that you may need much shorter length of OW in the case with more observations including direct in-situ CO2 concentration data. Also, this study incorporates very low resolution of the numerical model. Increasing model resolution increases the number of unknowns while you can use much dense remote sensing data (with proper thinning/superobing). In that case, the ratio of GV+ data contents to column mixed remote sensing data contents would drop, and then there would be another possible sensitivity of AW/OW lengths. 2) In addition, the horizontal localization scale sets too small (150km) although the horizontal resolution of the model is very coarse. If it is not just typo, the exceptional setting of horizontal localization scale will cause high frequency errors of SCF estimates with 6-hr AW. Therefore, authors should check whether the conclusion is still valid with reasonable setting of the horizontal localization scale ($\sim$1000-2000km). This reviewer doubts that greater horizontal localization scale may give good enough SCF results even with 6-h AW. 3) Experimental setting includes slowly varying parameters, SCF that have only seasonal variation without diurnal cycle. Authors need to explain whether this long OW will be good for estimating SCF that fluctuates from day to night every day.

[Specific comments]

1. p.7, line 5: “at every land grid point” means authors only correct SFC over the land? not ocean? Please clarify it.

2. p. 7: Since this study set the horizontal resolution of the model very low, the observation data of OCO-2 were aggregated. Please give more detailed explanation about how to aggregate the observations.

3. p. 9, lines 19-20: A regular 4D-LETKF has 1.5 times longer forecast than the

assimilation time window. e.g. if you have 6-h cycles of 4D-LETKF, you need 9-h forecast. Please correct this sentence.

4. p.10, lines 15-16: This reviewer cannot fully agree with the statement about the sensitivity of enKF DA to the ensemble size. It would be great to give any reference to support this statement, or to modify it carefully.

5. p.10, line 20: When authors use more than 400km horizontal resolution, a horizontal localization radius should be about 1500 km as a standard deviation of Gaussian localization function. This reviewer hopes that 150km is just typo. Otherwise, please seriously answer the major comment 2 above.

6. p.12, line 20: Does the experiments include a diurnal cycle? If not, please correct the sentence from “mainly on” to “only on”, or appropriately.

7. p.13, line 5: “deviations of estimates from the “truth” incases” cannot be clearly found from the figure.

8. Figure 6: Please give more detailed information that you show as a result. How did you define summer and winter (which months are they)? In addition, agreement of your estimates with true state looks amazing. But, it would be great if you additionally show how far your prior states of SCF were at the very initial time.

9. p.18, lines 25-27: This statement needs to be modified carefully. The new assimilation method can be useful for the parameter estimation with EnKF when the observations are too limited to constrain the parameters well and the parameters have slow and smooth variation in time and space, respectively. For example, if your parameters have very rapid temporal variation, long OW may not work well as the SCF case in this manuscript. In that sense, the statement should be revised.

[Technical corrections]

1. Figures does not have subtitle of (a), (b), etc, although authors explain the subfigures in that way. It would be good to explicitly mark them.

2. p.11, line 7: “However” does not seem appropriate in the context.

3. p.12, line 6: “The details of experimental settings” and the results of RMSEs “are shown in Table 1.”

4. Figure 7: Color bars need to be rescaled to analyze figures better. For example, authors need to include large concentration better for the first four figures. Current color bar does not show what is going on over 409 ppmv (authors should add the unit "ppmv" in the figure too), which seems important to analyze positive/negative error patterns over the northern part of Russia. For the last two figures, authors may rescale the color bar to see more details of difference.

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