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# Comment on gmd-2021-375

Anonymous Referee #1

Referee comment on "Improving the joint estimation of $CO_2$ and surface carbon fluxes using a constrained ensemble Kalman filter in COLA (v1.0)" by Zhiqiang Liu et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-375-RC1, 2021

**Improving the joint estimation of CO2 and surface carbon fluxes using a Constrained Ensemble Kalman Filter in COLA (v1.0)**
*Zhiqiang Liu et al.*

The authors investigate the benefit of constraining the CO2 mass when simultaneously estimating the CO2 (state variables) and the surface carbon fluxes (parameters) with the LETKF in an idealized setup. The science is valid and benefits the corresponding research field. However, major improvements are needed in terms of language and the discussion of the results. I listed the major issues of this paper below.

Language:
Unfortunately the English is very poor, which makes it is sometimes difficult to decipher what the authors mean. Some examples:
- line 66: "The system replaces the GCM …". What is the GCM replaced with? Is it with GEOS-Chem? This is not clear from the sentence.
- line line 207: "To get the prior ensemble …." What does it mean: "at 1 October 2014 within 30 days"?
- line 264: "However, the SC amplitude and the phase are reinvestigated…" I don't understand the word reinvestigated in this context.

Many articles ("the" and "a/an") are either missing or are there where they shouldn't be.

In my opinion there are too many abbreviations introduced, which makes it hard to read the paper. Perhaps including a table with all abbreviations would help.

In the abstract, line 23, the authors state that they introduce a Constrained Ensemble Kalman Filter. However, if I am not mistaken, this was introduced by Pan and Wood

(2006).

Provided information:
It was necessary to read Liu et al 2019 to understand the current manuscript. I feel that the authors did not optimize the selection of information given. For example, the authors chose to write down the equations for the LETKF, but did not explain what an "observation window is". In my opinion the authors have 2 choices:
1) Let the readers explicitly know that this is a follow up paper of Liu et al 2019, so that they know they should read Liu et al 2019 first. In this case, a lot of the paper up to section 2.3 should be shortened (and also section 3).
2) Make sure that this paper is understandable without Liu et al 2019. Specific examples of information that needs to be added are:
- explaining the "running in place" principle. This can be very short as is for instance done in the abstract of Liu et al 2019.
- The authors are estimating the SCF, but the results show fields of FTA. Do they calculate FTA from SCF? Clarify! Also, what is E_t in equation 12) (from Liu et al 2019 I deduce it is the time average)?
- Line 66: "The system replaces …" Elaborate on the reasons why the GCM model is replaced. The GEOS-Chem model has not been introduced, so the reader does not know that it does not include an estimation of transport uncertainties related to the meteorological field. Again, this is well explained in the abstract/introduction of Liu et al.2019. Also, the authors should shortly discuss the implications of assuming perfect meteorological fields. Is it a reasonable assumption, i.e. are the errors small in comparison to $CO_2$ and the SCF in reality? Or do the authors expect it should not impact the estimation of SCF a lot? Or is this assumption made to isolate the effect of LETKF_C on the SCF?
- The introduction of COLA is very confusing. Did I understand correctly that COLA is the name of the system which uses the GEOS-Chem model as the ATM and LETKF_C (without or without CEnKF) as the DA algorithm? Please clarify in the manuscript.
- How is the inter-annual variation calculated?
- Many figure captions miss information (for example I assume the shaded region in Figure 4a is spread, but it is not stated)
- The RMSER is introduced and is even discussed as if the RMSER is presented, but none of the Figures actually show the RMSER. They seem to show the difference compared to the truth.

Science:
I am concerned that the authors chose to constrain the $CO_2$ mass on the ensemble mean only. They state in line 140: "We further simplified the method by constraining only the ensemble mean state, which significantly reduced the computational cost without influencing the performance". However, the authors provide no evidence that the performance is not influenced. By constraining only the mean, each ensemble member is free to violate the mass conservation. However, as Pan and Wood 2006 point out, adding the mass conservation constraint essentially adds physical information to the DA process. By not enforcing the mass constraint on each member, a lot of this physical information is lost. Also, as the authors point out in line 75, "the impact of mass gain or loss could last for a long time". I would like to add that the impact is not necessarily linear in time, nor symmetric. As a result, a $CO_2$ forecast could be very different when constraining each member as opposed to only the mean. I think that constraining the mean is still helpful because it will likely (though not guaranteed!) reduce the imbalance in each member anyway, but it remains an "ad hoc" solution forced by computational restrictions. I therefore think that the authors should do one of the following:

- Do an experiment with all members constrained. It would really be great if this could be done, but I am not aware of the computational limitations the authors have. If it is not possible, perhaps a set of experiments EXP-L, EXP-LC and EXP_LC2 (where EXP_LC2 corresponds to constraining each member) is feasible with a smaller ensemble size?
- Elaborate on this point. Explain that it is not possible to constrain each member and try to justify the choice of only constraining the mean and discuss the possible drawbacks. In Figure 11, show also the ensemble imbalance spread for EXP-LC, not only EXP-L.

A large portion of the results is dedicated to evaluating EXP-L with respect to the prior. However, this is what was covered in Liu et al 2019. The current paper should focus on the comparison between EXP-L and EXP-LC with maybe the prior as a helpful benchmark. I therefore think that the abstract sentence on line 28 "At the seasonal scale …" should be deleted. Also because it is not clear to the reader that the "improved system" is referring to EXP-L, not EXP-LC. This confusion is also very much present in the conclusion, where I also think the improvement of EXP-L over the prior should not be highlighted as a result. I therefore also suggest that the authors merge section 4.1 and 4.2 and basically make all plots with prior, EXP-L and EXP-LC. If the reason the authors did not include EXP-LC in Figures 4-7 is that the difference between EXP-L and EXP-LC is not visible, this should be explicitly stated. Perhaps the authors meant to communicate this on line 302, but this was not clear to me. A likely reason for the lack of difference is that the SCF are updated using the covariances between CO2 and SCF, which are probably not as strongly impacted by the mass constraint as the ensemble mean. One would see a much greater effect on a CO2 forecast.

The RMSER is introduced but not presented. I feel the RMSER (both as a function of time, averaged over space (Figure 4b), and as function of space averaged over time(Figure 7)) would be an efficient and effective way to present results. I would be interested in both the RMSER of EXP-L with respect to the truth, and the RMSER of EXP-LC with respect to EXP-L.

More results could be shown. For example, the inflation scheme for C02 is different than in Liu et al 2019. It would be nice to show the spread and RMSE (or spread skill ratio) of both C02 and the SCF throughout the experiment period, including the spinup. Also, more could be said and shown about in effect the mass constraints has on the SCF increments and covariances between CO2 and the SCF. I would also be curious to look at the background RMSER (EXP-L with respect to EXP-LC) of the background CO2, not only the increments.

Plots:
Figure 7 and especially Figure 10 are too hard to read. Either make the plots bigger or show the RMSER with a colorbar that is centered aroud zero, so that it is easy to spot the improvement regions.