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Interactive comment on "A Bayesian Ensemble Data Assimilation to Constrain Model Parameters and Land Use Carbon Emissions" *by* Sebastian Lienert and Fortunat Joos

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Lienert and Joos apply a bayesian data assimilation framework to the LPX-Bern model in order to constrain a selection of model parameters using a range of local to global carbon and water cycle observations. In the manuscript, they describe the framework and illustrate the key model performance criteria. This framework allows them to provide a data-constrained simulation of the regional and global terrestrial carbon balance between 1860 and 2016, and in particular to estimate the land-use related carbon emission, including an uncertainty range.

This is a very good study integrating multiple observations in a systematic and repro-

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ducable way to constrain a process-based global carbon cycle model. This system is not only used to produce a newly calibrated LPX-Bern version for future use, but also to provide useful insight into the magnitude (and particularly the uncertainty) of land-use emissions. Overall this is a valid contribution to Biogeosciences.

Unfortunately, I have troubles following the method description. The description of the way, the parameter distributions are updated is remains fairly unclear. I recommend that the authors devote a special section in the Methods section to clarify a couple of points: a) how was the prior distribution of the parameters derived (literature ranges typically only allow to assume uniform distributions); b) how exactly is the ensemble updated after the metrics are calculated. Is it the probability distribution of each parameter, which is updates? This would lead to a new LHS set to be produced, and subsequent new model runs? Figure 1 would suggest that this is the case, but in this case, the new set would be dependent on the metric and metric weighting, which contradicts the statements made on P2 (also, it's computationally probably prohibitive). Or is each LHS sample weighted according to the model performance, and this weight then used to calculate the PDF of a modelled output? If that is the case, I don't understand the iterative nature of the LHS sample updates? Also, in this case, it would be good if the authors would elaborate on the way they've estimates the posteriori distributions. Given that the authors highlight the ability to change the cost-function and weighting as a key strength of their method, it would be also interesting if they would add a discussion point as to how robust they believe the posteriori parameter distributions are against their choice of metric & weighting.

I have a number of further suggestions to improve the clarity of the manuscript:

P1 L17: in the context of a data assimilation paper, the use of assimilated here is confusing. replace by stored?

P2 L3: Add "Amongst others," at the beginning of the sentence

P2 L4: unclear what uncertain prescribed LULCC processes are meant to be, perhaps

give examples, or clarify that it's the representation of these processes that is uncertain

P2 L9 DA "should" be an integral part of model development, but unfortunately it is not always.

P2 L10: Is the Houwelling reference appropriate here? This does mostly relate to inverse atmospheric modelling

P2 L12: Not sure that I understand sequentially correct here. Most DA methods would assimilate different data sources simulateneously. Also, I think cost-function is the more common term for metric in this context

P2 L14 This sentence is a bit out of context in a paragraph on alternative DA methods, because benchmarking does in general not imply DA. It seems more logical to merge this sentence with the Paragraph starting in L25, and move the entire paragraph to L6 after Le Quere et al. 2016.

L2 L18: As noted above, I have troubles following here: LHS simply provides a set of parameter combinations, in which each parameter is sampled given a specified distribution and notably, ensuring that there is no correlation amongst any of the parameters. LHS does not imply any model metric per se. The way the posterior distribution is derived from the prior distribution and the model metrics is unclear. How many iterations would be needed to arrive at a stable solution, what is the stopping criteria, and why is it possible to change the metric during the DA procedure? This would change the posterior distribution, and therefore impede convergence.

P2 L31: I wonder if the flow of the introduction would be more logical if one would first talk about the LULCC processes as in this paragraph, then about the benchmarking in the preceeding paragraph, and only then about data assimilation?

P2 L31: While the (add) "net" land-atmosphere flux can "to some extent" be . . .

P2 L32: add "residual" terrestrial carbon sink?

P4 L21: I think it is worth highlighting that the strength of LHS over other sampling techniques is that the set of parameters in uncorrelated.

P4 L20: The text confuses MC parameter sampling techniques, which are indepedent of any purpose the sampling is made for, from MC Data assimilation techniques, which are not?

P4 L26: the description of alpha_a should correspond to table 1, it is not FAPAR!

P4 L7: Literature range only allow to give uniform distribution. How where the non-uniform distribuition parameters obtained / estimated?

P4 L7: I have trouble following from here on. Maybe this would become clearer, if first all the metrics and data sources were explained, and then the way the distributions are updated is clearer presented.

P8 L10: which winds were used for the transport? I assume that the winds were not interannually varying?

L9 L5: Inversion typically refers to the inverse modelling of atmospheric transport, whereas here - as far as I understand this, you simply take the land flux as the residual of the fossil fuel emission and ocean uptake.

P9 L10: Are these data sources not redundant with the global maps of total and soil C storage described earlier?

P9 L27: I don't understand the reasoning for the duplication of ensembles with gross transitions. Please motivate.

P9 L31: As noted above, I have difficulties following this description.

P10 L5-8: Is material for the introduction, not the results section

P10 L 8-11 can be safely removed.

Section 3: When giving numerical estimates, please add either range or standard devi-

ation, whenever the number is based on the ensemble. I also think that the more logical arrangement of the Results sections would be to first talk about model performance, and then about the attribution of the net land flux to LULCC and residual.

P13 L4: Why would an underestimation of the ELUC not affect your conclusions about ELUC?

P17 L6: is the use of the word significant appropriate here?

P17 L8: why not?

P 18 L 3: Why is this different from the approach described in Section 2?

Conclusion Section: There is no need to repeat details of the methods or approach undertaken

Figure 1: Ensure all lines are visible

Table 1: Check units and definition for E0. This seems more like an activation energy to me (not a temperature sensitivity

What are the units of the k_la:sa? Is this simply a scalar?

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