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Biogeosciences Discussions

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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The study presents an approach to constrain a DGVM with multiple observational streams of carbon stocks, gross and net fluxes. The authors rely on a latin hypercube stratified sampling to perturb model parameters and create several 1,000-member ensemble simulations of the terrestrial carbon cycle for the historical period. Results focus on the estimation of land-use and land-cover change emissions.

This study is quite innovative in the context of the global terrestrial carbon cycle as model parameters are constrained globally. I have found several similarities between the method described here and the Generalised Likelihood Uncertainty Estimation

method used in hydrological sciences (Beven and Binley 1992).

There are several issues with the current manuscript which need to be addressed.

First, my main criticism targets the description of the sampling method. It is very unclear how the prior probability distribution in Figure 1 and the new best-guess values in Table 1 have been obtained, and how the posterior distribution of the parameters is calculated. Is it based on the selection criterion used to exclude the less skilled model parameters (p7 l5-8)? If Figure 1 and Table 1 present results from the current manuscript they should be described in the corresponding section.

Second, I struggle to understand what experiments were actually undertaken. From section 2.6, it seems that three simulations are performed for each parameter set. These three simulations differ in the representation of LULCC: none (reference), gross or net transitions. Then, the results section reports the three different model configurations $M_{net,net}$, $M_{gross,net}$, $M_{gross,gross}$ while these are first described as three alternative skill weighted median.

Third, I am unclear about the skill-weighted mean method. Simulations with either net or gross land-use configuration are likely to yield different results so it is hard for me to justify $M_{gross,net}$. I understand that the $M_{gross,net}$ skill-weighted mean provides the best results compared to benchmarks (Table 3) but it could be an artefact, couldn't it? Also, some parameter sets are likely to perform better in some regions and worse in other. Therefore, would a spatially-explicit weighting scheme (Schwalm et al., 2015; Exbrayat et al. 2018) be more suited to constrain the ensemble?

Hereafter are some more specific comments

p4 l6: CRU TS3.23 covers 1901-2014, so how are simulations performed for 1800-2014 (or is it 1800-2016 like in the abstract?) please clarify throughout the manuscript

p4 l21: please define what model metrics

p5 l6: how have these distributions been chosen?

p7 l1: please write MSE^{i}_{rel}

p9 l28: 'LULUC'? please correct here and in several other places

p10 l13-23: please include some information about the uncertainty displayed in the Figures here and throughout the text

p11 l1: please quantify 'slight'

p14 l11: see previous comment on the study period

p16 l3: an informative figure would a covariance matrix of the parameter sets' scores for each criterion

p16 l12: according to Figure 8b and d, the model captures the seasonality but not the interannual variability. This is worth reporting (and explaining).

Fig 1: $M_{net,net}$ is not defined

Fig 3: please explain the sign convention as it seems at odd with figure 4 (E_{LUC} in particular)

Fig 7: this figure is very complicated. Why is it important to look at the whole ensemble, and the constrained one? Constraining the ensemble uncertainty is not a major point in the rest of the manuscript and uncertainties are not reported in most of the text.

Fig 8: please move the legend

References

Beven, K. J. and Binley, A.: The future of distributed models: model calibration and uncertainty prediction, Hydrol. Process., 6, 279–298, 1992.

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