Replies to Comments on Manuscript bg-2017-41

We would like to express our sincere gratitude to Dr. Trevor Keenan, Prof. Jasper Vrugt, and the other anonymous reviewer for their insightful and constructive comments and suggestions. All comments have been addressed below and considered in the revised manuscript as highlighted in red.

Associate Editor's Evaluation

Comment:

Thank you for your detailed response, which was greatly appreciated by the two referees. Both referees feel the manuscript has very much improved, and suggest publication following minor revisions. Please keep their comments in mind when preparing your manuscript for final submission, particularly Jasper Vrugt's comments regarding implications for homoscedastic NEE errors, and reviewer 2's suggestion to strengthen the abstract by better emphasizing the residual analysis.

Response:

We appreciate the associated editor for the positive assessment of the manuscript. The comments from both reviewers have been thoroughly addressed in this response file and considered in the revised manuscript as highlighted in red.

<u>Referee #1's Evaluation</u>

General Comment:

Thanks for your revised manuscript. I appreciate your extensive replies to my earlier comments. This certainly makes reviewing easier. I do see several things that can be further explored/investigated, but like to focus here on two remaining things.

Response:

We appreciate Dr. Vrugt for his positive assessment of the manuscript. The two specific comments have been addressed below and considered in the revised manuscript as highlighted in red.

Specific Comments:

Comment 1:

I think you can improve further a bit the discussion of the results. The generalized likelihood function obviously gives a different posterior - yet, the maximum likelihood value (you use uniform priors) of most parameters do not seem to be that much affected by the residual error assumptions. This maybe because you assumed the residuals to be Gaussian distributed - although you now model the residuals with an autoregressive operator - it maybe that the assumption of normality causes only few parameters to differ from their values derived from a standard Gaussian likelihood function without serial correlation of residuals. I think you can enhance the paper a little bit on this topic - that is - discuss a bit more the parameters that are similar and those that differ; those parameters that do not change much with a different likelihood should, in theory, be reasonably well determined. Those that change a lot affect in large part the residual properties (autocorrelation). This may have consequences for regionalization - relating parameter values to observable system properties.

Response:

We thank Dr. Vrugt for his thoughtful insights and constructive suggestions. The related discussion has been added in the revised manuscript as highlighted in red.

Comment 2:

Your inference with the generalized likelihood points out that the measurement errors of NEE could actually be homoscedastic (exhibit a constant variance). This maybe a very important finding - as this contradicts common assumption. Does this finding not deserve more exposure and analysis. Again, this finding may be due to epistemic error (or forcing data errors), but still it contradicts what is typically assumed, right? I think you should alert the reader here a bit more - certainly more work is needed on this topic. Certainly, it would help to enlarge the prior distribution of sigma_0 and sigma_1. Maybe this is part of the crux as well.

Response:

We appreciate Dr. Vrugt for his thoughtful insights and constructive suggestions. The following sentences have been added in the revised manuscript to emphasize the important finding: "*This finding contradicts what we usually assumed that the data errors are heteroscedastic. The reason could be caused by the epistemic error or forcing data errors. Or, an extended prior distribution of σ_0 and σ_1 may give different results. More work is needed to find out the underlying reasons.*"

Referee #2's Evaluation

General Comments:

The authors have done a very thorough job in addressing the reviewers' comments on the previous version of this manuscript. It is now much more informative for the Biogeosciences audience, with much of the overly technical material removed and replaced with more general description and explanation, with a greater concentration on the model and relating parameters to ecological processes.

The residual analysis, and the consequent selection of an alternative (correlated) error model to use in the likelihood function is a very interesting addition – and in many ways more important than the comparison of the different MCMC approaches. The final paper would benefit from some minor revisions that highlight this. It would be good to see this mentioned in the abstract, and in the introduction – particularly in reference to Trudinger et al (2007), which highlights the importance of likelihood function choice. We also need to see an assessment the impact of the new likelihood function on the ppdfs. How many parameters now meet the "constrained" criteria? What impact does this have on predictive ability in terms of the CRPS metric, and predictive coverage? Given the distinct broadening of many of the ppdfs, the suspicion is that this is greatly reduced?

Response:

We appreciate the reviewer for the positive assessment of the manuscript. In the abstract of the revised manuscript, we emphasize the importance of likelihood function choice by adding the following sentences: "*In addition, this effort justifies the assumptions of the error model used in Bayesian calibration according to the residual analysis. The result indicates that a heteroscedastic, correlated, Gaussian error model is appropriate for the problem, and the consequent constructed likelihood function can alleviate the underestimation of parameter uncertainty that usually caused by using uncorrelated error models.*" Besides, we added the following sentences in the introduction: "*In addition, while the importance of likelihood function choice on Bayesian calibration has been well realized (Trudinger et al., 2007), the reasonable usage of an appropriate likelihood function has been barely explored in land surface modeling.*"

In the revised manuscript, we discussed the impact of the new likelihood function on the PPDFs. For example, we added the following sentence: "*In addition, Figure 13 indicates that parameter uncertainty is larger in the correlated likelihood than the uncorrelated one for most parameters, and fewer parameters are constrained in the correlated likelihood than the uncorrelated case.*" Although we did not analyze the impact on predictive ability, it is expected that the predictive uncertainty would be broadened and more observations would be enclosed in the predictive coverage.

Specific Comments:

Comment 1:

Line 181: “and” -> “there is” (?) unclear

Response:

For clarification, the related lines have been revised as: “*The proposal distribution employed in the AM algorithm is a multivariate Gaussian distribution with means at the current iteration* $\boldsymbol{x}_t$ *and having a covariance matrix* $\boldsymbol{C}_t$ *that is updated along the chain evolution.*”

Comment 2:

Line 195: I believe Hararuk et al. (2014) used a test run of 50,000 simulations of a matrix approximation of CLM, not the full model – a somewhat different computational task

Response:

We thank the reviewer for the insight. The related sentence has been revised as: “*For example, Hararuk et al. (2014) inferred* $\boldsymbol{C}_0$ *from a test run of 50,000 simulations of a matrix approximation of the community land model in estimating the PPDFs of soil carbon related parameters.*”

Comment 3:

Line 426: “uncorrelated”

Response:

Corrected.