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Biogeosciences Discussions

Interactive comment on "A Bayesian Approach to Evaluation of Soil Biogeochemical Models" *by* Hua W. Xie et al.

Anonymous Referee #2

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This study fits two soil decomposition models (one linear and one nonlinear) to response ratios of CO2 fluxes to warming collected from field experiments. They estimate full parameter distributions using MCMC in a Bayesian framework. This is a great way to fit and evaluate models, as well as to place uncertainty bounds on subsequent predictions.

I wanted this paper to consider more the implications of its findings. For example I would have expected the introduction to focus more on why temperature response is especially important since that is the dataset that the authors focus on here. I was also interested to compare the performance of the two models. The performance metrics were presented but their results are never discussed. Why was it that CON generally performed better at most SOC densities? What are the estimated parameter ranges

compared to other models/literature values? For example the activation energy for SOM decomposition seems to be higher than the activation energy for other processes like uptake which may have some interesting implications. This is partially an interpretation of the scope of Biogeosciences vs. say Geoscientific Model Development, but I expected a little more interpretation of the processes underlying the performance metrics.

L62-66: Citation for this discussion of R2?

Also there are other metrics for evaluating Bayesian models that are not discussed here – you don't need an exhaustive review but ROC/AUC and BIC seem common.

L125: 0.9995 and 0.001 seems like extreme adaptation and step sizes to me, causing the model to take many small steps. If this is a supported strategy, can you provide a citation or justify further?

L191: As written, implies that AWB performs better because it has a higher RR in subsequent years after the first year, but the data show that the first year has the highest RR, so CON seems to correspond more closely to this. In the discussion, you can bring up the potential realism of oscillations given the Harvard Forest long term warming experiment.

L325-333: This discussion of R2 and other cost metrics seems repetitive to the introduction.

It seems like the performance metrics would be yet better with a lower SOC density (<50 mg SOC/g soil), if it were possible to achieve them without the AWB instability. I think you could fix the instability by changing your decomposition/uptake kinetics. Right now in the uptake equation DOC is in the denominator but its initial concentration is much smaller than MIC. So you can either flip to Reverse M-M for uptake or use ECA where both quantities (SOC and ENZ or MIC and DOC) are in the denominator <- this may be harder to fit because it will be more constrained, but it is also harder to break.