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Geoscientific Model Development Discussions

Interactive comment on "Bayesian Inference and Predictive Performance of Soil Respiration Models in the Presence of Model Discrepancy" *by* Ahmed S. Elshall et al.

Anonymous Referee #2

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The manuscript submitted by Elshall et al. is an interesting study dealing with the complexity of soil C model parameterization. In recent decades, the complexity of those model as well as the different tools to parameterize has increased substantially leading to potential misuses of powerful but complex mathematical approaches. The goal of Elshall et al is therefore to evaluate the impact on process-based model predictions of neglecting a couple of assumptions of the Bayesian framework as it is often done by soil modelers to avoid complexity.

The present study might not be super novel for the entire modeling communities in geoscience as mentioned by the other referee. Nevertheless, it underlines a flaw of

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several carbon soil modeling studies and might be considered as novel in this context. It is a pity that the author may not freely communicate their models and scripts it would have definitely increased the impact of the paper.

Even though the objectives of the paper are important and deserve to be published, in my opinion, the manuscript in its present form is sometimes too hard to read and needs some simplifications. A first recommendation might be to have a table summarizing all the acronyms and try to reduce them when not necessary. Secondly, a workflow scheme might also be useful to understand the logic of the authors, which is not always super clear. Finally, I missed some definition to be sure I fully understood the text. In particular, it is not crystal clear to me what the author means by 'data model'. From my understanding, a data model is based on data but the observed data are presented quite fare from the data model. Another point is that I still do not fully understood how the authors link their data model with their process-based model. I understood that the data models are used for posterior parameter estimation but sometimes the text makes me doubt.

I don't understand why the author fixed the upper limit of the physical range of CUE to 0.6 (the mean over terrestrial systems) whereas in the paper they cited several observations are above 0.6

Some typo: l121 'and' not necessary L176 please correct the parenthesis L611: despite instead of desp8ite

I, therefore, think that this manuscript deserves publication after a deep rewriting to clarify the methods used.

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