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Biogeosciences Discussions

Interactive comment on "Long-term bare fallow experiments offer new opportunities for the quantification and the study of stable carbon in soil" *by* P. Barré et al.

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1 General comments

The discussion paper by Barré et al. presents a compilation of SOC data from seven long-term bare fallow experiments. It will certainly attract the attention of the scientific community to this highly valuable data resource. I agree with the authors that these data may offer new insights into SOC dynamics in general, and the nature of stable SOC compounds in particular. The data should be made publicly accessible, preferably as supplementary material to a future paper. The paper is largely well written, concise

and comprehensible.

Barré et al. analyse the data by inversely estimating decomposition rates and pool sizes of simple SOC models of increasing complexity. In particular, the authors use this method to estimate the size of the stable carbon pool. Inverse modelling is a generic and powerful method that has the potential to provide new insights on SOC dynamics. In their study, Barré et al. use a Bayesian approach to estimate the model parameters and quantify their uncertainties using a classical first-order approximation. I have some concerns, however, mainly about their use of this inverse method. These objections will be detailed in the following section.

2 Specific comments

(2.1) The pedotransfer function used to estimate missing bulk densities (p. 4895, l. 26, equation number is missing) uses only one predictor variable (SOC). Moreover, it seems that this equation is rather site specific and only regionally applicable. More universal and reliable estimates can be obtained when accounting for other predictors, such as texture, stone content, soil depth and soil type. I recommend to use the pedotransfer function developed by Martin et al. (Soil Sci. Soc. Am. J., 73:485-493, 2009).

(2.2) Why do the authors use informative priors with "very large" dispersion for the model parameters? How large is "very large"? Wouldn't it be more appropriate to apply noninformative (uniform) priors in this case? Noninformative priors would actually better reflect the prior knowledge about the model parameters and circumvent the need for assuming log-normal and uncorrelated informative priors. From my point of view, both assumptions are questionable anyway. In doing so, only the limits of the feasible parameter space need to be specified. Furthermore, the term that quantifies the mismatch between the model parameter estimates and their priors would disappear from Eq. (1). The approach would then no longer be considered Bayesian, of course.

(2.3) In general, the uncertainty in the SOC measurements could be estimated simultaneously with the model parameters. This would avoid making assumptions about the magnitude of the measurement error, which has been reported by the authors to be problematic. In the case of the Askov, Kursk and Versailles data the first guess was shown to be too overoptimistic (that is, too small). Also, including the measurement uncertainty in the vector of estimated parameters $\vec{x}_b$ would result in more reasonable uncertainty bounds on the estimated parameters and, consequently, model predictions. In this approach, however, the interpretation of this error measure would change slightly, in a sense that it now includes the uncertainty due to measurement error and the uncertainty due to model error.

(2.4) Which gradient-based algorithm was used in this study? The reference is incomplete here. Gradient-based algorithms have the drawback of being locally convergent. That is, the algorithm might converge prematurely because it gets trapped in a local minimum of the cost function. This also means that it is difficult to judge whether indeed the global minimum was located or just a local, non-optimal solution was found. To gain confidence in the best solution found by these gradient-based algorithms, it is convenient to repeatedly start the optimization from various locations in the parameter space. Did the authors follow this procedure? Also, the expression "we converged to a minimum..." appears inappropriate. It is the algorithm rather that converges.

(2.5) For models that are nonlinear in their parameters (which is true for all but the first model used) it is not allowed to average over the parameters. The model output corresponding to the averaged parameters and the average of the model outputs

obtained for each replicate will not be the same in this case. I suggest to include all individual measurements taken at a given site in the measurement vector $\vec{x}$ and to fit one model per site only. This results in effective parameters for a given site, that is, parameters that take into account the variability between the various replicates.

(2.6) p. 4900, ll. 26-27: How did the authors test if a parameter was significantly different from zero? Please provide some explanation here.

(2.7) How does the sum of the estimated pools compare to measured SOC? This comparison would help to judge whether the estimated pool sizes are realistic, and hence, the conclusions drawn from this model exercise are robust. If the discrepancy is substantially larger than the uncertainty in the SOC measurements, however, this would indicate that something is wrong. The model would be able to nicely fit the data, but for the wrong reasons. The estimated model parameters would be rather meaningless then.

(2.8) Wouldn't it be more reasonable to constrain the model by calculating the size of the most stable pool from the sum of the remaining (estimated) pools minus measured SOC? This approach was adopted, for example, by Paul et al. (Soil Sci. Soc. Am. J., 70:1023-1035, 2006). This would also supersede the assumption of a log-normal prior for the inert pool, as described in the last paragraph of the Methods section. Given the relative small amount of data that is typically available for calibration of the model and the limited amount of information about this pool contained in (some of) the data sets, this prior is likely to have a dominating effect on the posterior estimate. This might also explain why the estimate for this pool reported in this study is substantially larger than previous estimates of the "Inert Organic Matter" pool defined in RothC.

(2.9) p. 4905, ll. 18-23: The paper by Lehmann et al. (Nat. Geosci., 1:832-825, 2008) might be worth citing in this context.

(2.10) p. 4907, ll. 14-22: The first part should be moved to the discussion section. It does not contain conclusions. The last sentence would better fit into the acknowledgements. It is also not a conclusion.

(2.11) Tab. 1: The abbreviation "EP" used for potential evaporation should be replaced with "PE" (to make it consistent with the table caption). Moreover, a reference would be useful here since there are several modifications of the Penman equation. What does the uncertainty ranges for initial and final C stock stand for?

(2.12) Fig. 1: Why is the model uncertainty the standard deviation times 5? What confidence level does that stand for? Also, I suggest to show the model uncertainty as uncertainty envelopes instead of bars at the bottom of the figures. In my view, uncertainty envelops are a more intuitive representation of model uncertainty.

3 Technical corrections

(3.1) The second author should get a double star superscript (instead of a single star) to make it consistent with the footnote.

(3.2) p. 4890 ll. 28-29: This sentence is too casual. Bare fallow is indeed the only way – not the easiest – to deplete the more labile carbon compounds. Also, the expression "wait patiently" appears inappropriate.

(3.3) p. 4892 l. 27: The abbreviation "SOC" should be used here instead of "OC", which was not previously defined. See also p. 4895 l. 15. and others.

(3.4) p. 4893 l. 11: The abbreviation "FYM" was not previously defined.

(3.5) p. 4896 l. 16: The title of this subsection could be improved, for example "Inverse modelling of SOC dynamics".

(3.6) pp. 4896-97: The mathematical symbols used for vectors should be revised in this section.

(3.7) p. 4897 l. 15: The symbol χ^2 should be used instead of "chi-2". See also l. 19.

(3.8) p. 4900 ll. 21-23: The explanation (in parentheses) of the AIC is redundant and should be removed.

(3.9) p. 4901, l. 28: Shouldn't it be "two C pools" instead of "three C pool"?

(3.10) p. 4902 ll. 15-16: The expression "to wait for a while" is too casual.

(3.11) p. 4902 l. 24: The upper limit cannot be negative.

(3.12) p. 4903 l. 16: The expression "The optimization algorithms revealed that" is unfortunate. I suggest using "Our results indicate that..." or simply discarding this part of the sentence.

(3.13) p. 4905 l. 20: This pool is officially called “inert organic matter”, not “inert organic material”.

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