Reply to comments on "Bayesian calibration of a soil organic carbon model using ^{14}C measurements of soil organic carbon and heterotrophic respiration as joint constraints" by B. Ahrens et al.

Our final Author Comment are generally set in blue, while the original comments by the referee are set in *black italics*.

Reply to Referee #2

General comments

We would like to thank Referee #2 for his constructive comments and hints. Below we replied individually to the general and specific comments in more detail.

Dear editor,
This is an interesting work presenting a very thorough analysis of the uncertainty of the parameters of a simple SOC decomposition model. It links a variety of observations including carbon stable isotopes, carbon stocks and respiration fluxes with model outputs under a Bayesian framework utilising MCMC procedures. It explores how well constrained the parameters were under the use of different number of observations and how the turnover rates were effected by relaxing the assumption that a steady state exist for soil carbon pools.

***General comments**: A very well written manuscript, with a large amount of information to present.*

We would like to thank Referee #2 for his comments.

Reading through however, felt somehow overwhelming because of this large amount of information the authors were trying to put through. It certainly is not an easy bed-time read. However, this did not reduced the value of the manuscript. On the contrary, the manuscript can be seen as a useful demonstration on how to explore and present the uncertainty of parameters and how to investigate the impact of "relaxing" the assumption of steady-state. The lack of clear sectioning between results and discussion made the latter less obvious in parts of the manuscript which really doesn't help getting the point across and giving definitive answers to the otherwise clearly stated objectives. The manuscript is certainly well written and for I could recommend its publication, however, I would strongly recommend the authors doing some changes such as 1) Maybe reduce the number of results presented by choosing those that directly answer the questions they have set from the start 2) split the results and discussion section for a more clear and focused discussion.

Given the comments of Reviewer #1 ("Good job including" section 3.2 "Correlations between parameters" and the obvious need to discuss the *bias* parameters we would refrain from largely reducing the number of results presented, i.e. dropping complete sections. However, we could for example drop some of the correlations between parameters as discussed in section 3.2.

However, we will try to make a clearer distinction between what is a result and what belongs to the discussion.

Below you can find some ***more specific comments/recommendations*** *regarding the manuscript:*
1. At the moment the way the second objective of the article is given in the abstract does not reflect the very important issue of relaxing the steady-state assumption. I find this very important issue, as the authors claim in the introduction it does affect the estimation of the turnover rate. I suggest the authors change the wording to make it more clear that this is what they are trying to do. It should be clear to the readers from the abstract.

We will add one or two sentences in the abstract to emphasize the relation between estimating the turnover time and a deviation from steady state.

2. Soil incubations: Why the authors used different methods for collecting soil samples. Does leaving the small roots in the cores cause an extra addition of carbon which was not included in the first site? Maybe the authors would like elaborate a bit more as to why they thought this would not be a problem.

The differences in methods between incubations at Howland and at Coulissenhieb+Solling can be traced back to different ideas how partitioning of soil respiration into heterotrophic respiration and root respiration should be done. The incubations were not performed in a coordinated fashion at the different sites because they were done in different projects.

At Solling root fragments were left in the soil core under the assumption that roots would not respire autotrophically after 10 days. Arguably the dead roots would already start to decompose and thus contribute the $\Delta^{14}C$ signature of HR. However, these dead decomposing roots would already qualify as soil organic matter and thus could be included in the portioning of soil respiration.

3. Measurments at Howland Forest: Since there were no data for belowground litter input, why the authors thought that fixing the input to a similar order of magnitude to the aboveground litter would be sufficient? Maybe they can elaborate on this. They could also have also included it as a variable in the MCMC procedure.

Just by comparing the relation of belowground litterfall and aboveground litterfall at our two other sites (Coulissenhieb and Solling), it seemed like a valid assumption that also for Howland root litter input is in the same range as aboveground litterfall (also McClaugherty et al. (1984) and Persson (1978)).

With the bias factor for aboveground and belowground litterfall we already allowed for uncertainty in the litterfall input. In a revised version we will include a larger bias for belowground litter inputs than for aboveground litter input.

4. The authors have done an amazing amount of work and even greater to be able to present in a concise and clear way the results. However, when they combined the results and the discussion, sometimes I felt sometimes like results were taking over the discussion, in some parts of the manuscript (e.g., section 3.1). Maybe a better approach would have been to have a separate section for the results and for the discussion. This helps keep the focus of the manuscript and making more clear the message it wants to take across.

Thanks for this hint. We will try to follow your suggestions and separate results and discussion better.

5.Was the correlation of the parameters known prior the analysis? I understand that an assumption was made that nothing is known about the correlation of the parameters. However, it has been shown that ignoring correlation between the parameters can significantly alter the resulting posterior distribution of the parameters. I suggest some further exploration using multi-variate prior information based on covariance.

Scharnagl et al. (2011) basically did a sensitivity analysis to derive the correlation structure of a hydrological model. This information about the correlation structure was used to parameterize a multivariate normal distribution of priors.

In our opinion the approach taken by Scharnagl et al. (2011) is very promising to improve the identifiability in Bayesian calibration exercises.

In our study we could also have performed a sensitivity analysis beforehand to quantify possible correlations between parameters. Alternatively we could, for example, have taken the reported correlation between a deviation from steady state (f_O) and the turnover rate of the old pool (k_O) as prior knowledge for correlations between these two parameters.

However, if we adopted this approach and at the same time wanted to stick to the probability distributions we used for our priors (lognormal, truncated normal, logitnormal), the definition of a proper joint (multivariate) lognormal-logitnormal-truncated normal distribution has to be well thought out (Chen, 2002;Fletcher and Zupanski, 2006;Toma, 2008).

References

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