Biogeosciences Discuss.,
doi:10.5194/bg-2017-125-RC1, 2017

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "High organic inputs explain shallow and deep SOC storage in a long-term agroforestry system – Combining experimental and modeling approaches" *by* Rémi Cardinael et al.

T. Wutzler (Referee)

twutz@bgc-jena.mpg.de

Received and published: 19 April 2017

The study presents a new model development and calibration to an interesting horizontally heterogeneous system. It is based on an impressive compiled data set of observations and derivations of relevant inputs and state variables to compare. The main conclusion is that the observed increases in SOM stocks in an agroforest system are due to higher litter input compared to an agricultural control. The modelling exercise is interesting to the soil modelling community, and to the community researching interactions of vegetation components and management. I state several main points

Printer-friendly version

Discussion paper

followed by more detailed comments.

1 Main points

1) One calibration aspect of the study that convinces me (and probably other readers as well) of the validity of the study is currently not well highlighted. The model was calibrated to the control plot only. Despite of simplifying assumptions on similarities in climate and vertical transport between the control and the agroforesty system, the model predicted the differing C-stocks in tree rows and alleys and its depth distribution well. This is a strong validation.

2) The quantification of the priming effect (PE) seems to be a bit complicated with running the no-PE model variant with a decomposition rate that was calibrated with the PE-model variant. To my opinion there are more straightforward quantifications already in the data (see detailed comments). I suggest highlighting the result that the priming model variant in Fig 4 was able to capture the depth distribution of C-stocks while no-PE model variant did not.

3) While the mathematical model is well described, information is missing on the solution of the forward model, i.e. the solution of the presented partial-differential equation given a set of parameters. Which method has been used? What was the spatial grid, the same grid as the measurements? Was this grid sufficient to represent the steep concentration gradient in the top soil? Have different grid sizes been tested?

4) To my understanding of the study, the increased C stocks at the walnut tree lines are explained in a big part to increase of the above-ground carbon input by the herbaceous summer vegetation between trees (Fig. 3). I would like to read some discussion on this point. Was there an organic layer?

2 Detailed comments

L 412: Instead of interpolating parameters of several fits, I suggest fitting a single equation to the entire dataset with an additional variable “distance to tree” and parameters a and b depend on this distance. However, the simplified procedure here seems to work and this point does not affect the conclusions.

L 444: Please specify exactly which observations and which predictions have been used for calibration.

Table 7: The prior knowledge in eq. 19 was specified as normal distribution. Table 7 instead reports a range of values instead of a mean and a variance (x_b and diagonal of P_b in equation 1). Moreover many ranges span several orders of magnitudes suggesting that the parameters should be log-transformed before estimation. Where does the variance of the posterior come from? And what is the meaning of “prior values” in the posterior column?

Table 7: Where did the prior information come from? Are these uninformative priors or does it affect the results if you take different priors?

Eq 21: Please explain the derivation. Usually the $BIC = ln(n)k - 2log(L)$, which involves the Likelihood instead of the mean squared deviation. From a Bayesian perspective $-2log(L) \propto J_{data}(p)$, where J_{data} is the first term of J of eq. 19 (excluding the prior term).

L 478: Please, clarify terminology of spin-up vs model calibration. To my understanding you calibrated 4 or 5 parameters depending on the three model variants so that equilibrium stocks, i.e. simulations after 5000 years, were close to observed C-stocks (n=?) of the control plot in 2013. I suggest putting this content to the calibration section.

L 508: This derivation of the effect of priming is hard to grasp. To my opinion its more straightforward is compare predictions of the PE-variant model versus the non-

PE variant; each consistently calibrated and applied for prediction:

- Effects of litter inputs: predictions of no-priming variant only: agroforestry stocks vs control stocks
- Combined effect: prediction of the priming model variant only: at agroforestry plot versus the control plot
- Effects of priming only: prediction of the priming model variant versus the predictions of the no-priming variant for the agroforestry system

Since the profile was not matched well with the no-priming model one can focus on sums.

Fig 3: Please, note that the largest above ground input comes from herbaceous vegetation. Is this an important aspect for C-stocks of the agroforestry system?

L698 (3.4.2): Please, remind the reader that C-stocks of the agroforestry plot were not part of model calibration (that used the control plot only) but are used here for validation.

Fig. 4: This is a nice demonstration of priming formulation being able to match the depth-shape. Although uncertainty of the mean (standard error) is low due to the high sample number, you may add the standard deviation across 93 measurements in order to get an impression of the variability.

I would like to see a figure, where C-depth profiles can be compared between cases without being dispersed across facets. Maybe zoom in to 5 to 15 stock range.

Fig. 5: Please, use a color scale with a clear zero.

Fig. 6 Please, add difference in measured stocks to the “Inputs+PE” column for comparison.

L 753: Suggest: “Despite of these simplifying assumptions, the model calibrated to the control plot was able to ...”

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2017-125, 2017.

Printer-friendly version

Discussion paper