Biogeosciences Discuss.,
doi:10.5194/bg-2017-125-RC2, 2017
© Author(s) 2017. CC-BY 3.0 License.

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.

Anonymous Referee #2

Received and published: 28 May 2017

This is a comprehensive study that uses an impressive set of field data to build a model for exploring agroforestry impacts on soil organic carbon (SOC). The topic is of interest and fits the scope of the journal. The combination of both field and modeling data is a key strength of this paper and provides interesting results regarding the spatial distribution of SOC in an agroforestry system. The modeling further highlights the potential negative impacts of priming on SOC storage. The methodology, results and most of the interpretation is sound. I therefore recommend this manuscript may be published after addressing the concerns and comments outlined below.

Printer-friendly version

Discussion paper

CC BY

Major comments: 1) Due to lack of data, the authors assume that 'soil temperature and soil moisture conditions were the same in the agroforestry tree rows, alleys and in the control plot (L388ff)'. Given the otherwise extensive data collection at this site it is surprising that these key variables have not been measured. As the authors acknowledge at various places, the impact of agroforestry on the SOC is primarily a result of the altered soil abiotic conditions. In my view the lack of these data hamper the understanding of the true controls and mechanisms responsible for change in SOC in the agroforestry system compared to the agricultural control field.

The sensitivity analysis performed by the authors in an attempt to address this limitation cannot replace the missing information on soil abiotic controls since it merely reflects the model sensitivity to these parameters rather than their actual control on SOC.

This shortcoming also limits some of the discussion. In my view, the related conclusions that 'that OC inputs is the main driver of SOC storage (L752)', that 'a decrease of SOC mineralization due to the agroforestry microclimate is not obvious (L753)' and that 'soil microclimate in the agroforestry plot are not major drivers of the SOC storage (L766)' are therefore not justified.

2) The SOC stock is the product of C concentration per unit soil multiplied by the amount of soil per volume (i.e. bulk density). The study however is entirely focused on explaining changes in SOC due to changes in C concentration (as a result of C input/output) whereas changes in bulk density are not reported. It therefore remains unclear what the separate roles of changes in C concentration and bulk density are in controlling the changes in the total SOC stock (L743ff). While the authors acknowledge that the presence of trees (roots) could modify soil structure (L820), the effects of tree planting on such physical soil properties and subsequently SOC stocks are not well addressed in this study.

3) The authors argue that the two pools model with priming effect was the best one, as shown by the BICs (Fig. 4, Table S1) (L704). However this is not true for the

Printer-friendly version

Discussion paper

CC BY

agroforestry alley which had a similar BIC and RMSE than the noPE model in Fig.4. Since the alley covers most of the area in an agroforesty system, this indicates that the priming effect might be overall less significant for this system as proposed by the authors.

4) Overall I find that the ms is too long, especially the method section is exhaustive (16 pages incl. Figures and Tables) but also parts of the results could be condensed. Given that the compilation of the C stock data is not a primary study goal (L118ff), I suggest that methods and results related to these data could be considerably shortened and partly moved into the supplementary part or refer to by references. For instance, data shown in Table 4 is already published (Cardinael et al., (2015b) and thus there is no need show this Table once more. Section 3.1 and 3.2, specifically the equations developed here should be moved to the Method or Supplementary section. Details of Section 2.7 could also be moved to the Supplementary part.

Minor comments: Line 658: Here and at other places the authors use the word 'globally' which seems inappropriate in the given context.

L706: 'The spatial distribution of SOC storage was also well described (Fig. 5)' – I disagree, Fig.5 shows the 'additional' SOC in the agroforestry system relative to control but not the absolute amount of SOC storage.

L725: 'The priming effect increases the decomposition rate when more FOC is available' – provide a reference for this statement or use past tense to indicate that this is a result from this study.

L772, 797, 873: At the several places the authors refer to 'the model' while several models (or model variations) were used in this study. Please clarify in each case which of the models (model variation) is meant when referring to one specific model.

Figure 4: It would be helpful to add separate legends to the middle and right column sub-figures in Fig 4; also how is it possible that the model PE follows the measured

Printer-friendly version

Discussion paper

CC BY

SOC profile most closely but results in similar BIC than the noPE model?

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

CC BY