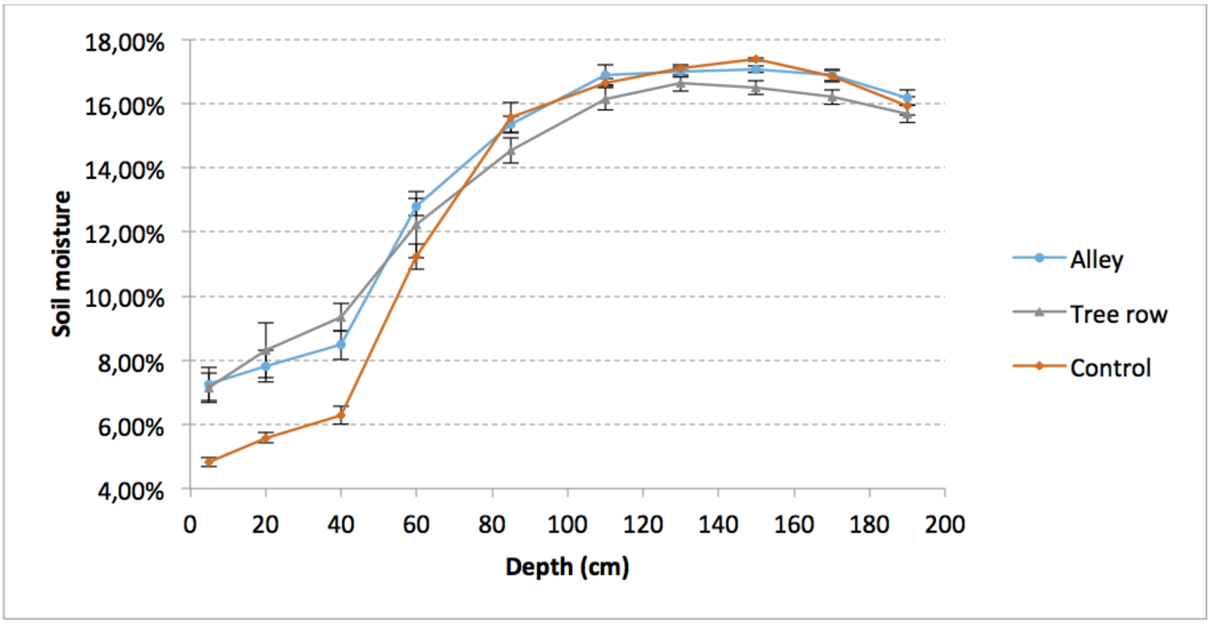
Referee 2

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.

Response: We thank you for your interest and you positive comments on our work.

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.

Response: We agree with this comment, it is a pity that soil moisture and soil temperature sensors have not been installed in both fields, and on the long term. But this trial was first established to study crop yield and tree growth in association, and questions on SOC dynamics came very recently. In May 2013 (late Spring, about 15 days after the last rain), we sampled 40 soil cores in the tree rows, 60 in the alleys, and 93 in the control, and we measured soil moisture on 23 of them. Soil cores were first taken in the agroforestry plot, and then in the control plot, under sunny conditions for both plots. The results showed that soil moisture was lower in the first 40 cm of soil in the control plot, but that there was no difference below:



During the last sampling day in the agroforestry plot, some cores were also taken in the control plot, and the same difference in terms of soil moisture was observed, suggesting that the lowest soil moisture in the control plot were not due to the sampling delay. Trees in the agroforestry plot probably slowed down the soil evaporation due to the shade. Most of the additional SOC storage in the agroforestry plot was observed in the topsoil. The lower topsoil soil moisture observed in the control in May 2013 would induce a reduction of SOC decomposition compared to the agroforestry plot, and then would reduce the observed SOC storage. But we can not conclude with this punctual observation, this phenomenon probably alternates during the season. For instance, we could hypothesize that in summer, deep soil will be drier in the agroforestry plot than in the control due to tree water absorption. Due to these uncertainties, we thought it was wiser to consider a mean annual soil temperature and moisture identical in both fields.

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.

Response: We tried to detail but also nuance our conclusions as suggested: "Despite these simplifying assumptions on similarities in microclimate but also on vertical transport between the control and the agroforestry system, the model calibrated to the control plot was able to reproduce SOC stocks in tree rows and alleys and its depth distribution well. This strong validation also suggests that OC inputs is the main driver of SOC storage at this site, and that a potential effect of agroforestry microclimate on SOC mineralization is of minor importance." (P40L732-737).

"A sensitivity analysis performed on these two boundary conditions showed that the model was not very sensitive to soil temperature and soil moisture (Fig. S4), but the real effect of these two parameters on SOC dynamics under agroforestry systems should be better investigated in future studies" (P41L747-750).

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.

Response: This is a very relevant point, soil bulk densities were only lower in the topsoil in the tree rows compared to the alleys and to the control plot. Bulk densities were published earlier (Cardinael et al., 2015b) and thus not reported here. In the model, we used the measured soil bulk densities for the control, tree rows and alleys from Cardinael et al., (2015b) (P8L172). We then expressed SOC stocks on an equivalent soil mass basis, and not at fixed depth. Therefore, the change in bulk density was implicitly taken into account 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 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.

Response: In this case, alleys occupied 84% of the agroforestry area. The BIC and RMSE were lower with the PE model than with the noPE model as indicated in Fig. 4, but we acknowledge the difference is small. In the alleys, the first soil layer (0-10 cm) was worse represented by the PE model than by the noPE model. As the BIC is calculated on the whole profile, this bad fit impacts the BIC even if the PE model performs much better for the other soil layers, this is well shown in Table S2. We therefore think that the PE is need to represent correctly the profile in the alley.

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.

Response: We agree that the MS is very long, which is mainly due to the huge amount of data that are compiled here. Moreover, it also includes the differential equations of a new model, which we think are better to be presented in the main manuscript than in the supplementary. We however performed the following changes in order to shorten the description and facilitate comprehension:

Tree fine root biomass data previously shown in Table 4 were moved to the supplementary part (Table S1). Moreover, Section 3.1.1 “Carbon stock in the walnut tree biomass” and Table 3 were deleted as results were already presented in Fig. 3.

Section 3.1.2 “Tree growth” was moved to the Method part and merged with section 2.6.2 “Interpolation of tree growth” (P18L400-403).

Section 3.1.3 “Crop yield” was also moved to the Method part and merged with section 2.6.5 “Aboveground and belowground input from the crop” (P20-24L457-524).

Section 3.2.1”Leaf litterfall” was moved to the Method part and merged with section 2.6.3 “Change of tree litterfall over time” (P18L407-415).

Section 3.2.2 “Tree fine root C input from mortality” was moved to the Method part and merged with section 2.6.4 “Tree fine root C input from mortality” (P18-20L418-454).

Section 3.2.3 “Aboveground carbon input from the crop” and section 3.2.4 were moved to the Method part and merged with section “Aboveground and belowground input from the crop” (P20-24L457-524).

Section 3.2.5 “Aboveground and belowground carbon inputs from the tree row herbaceous vegetation” was moved to the Method part and merged with section 2.6.6 “Aboveground and belowground input from herbaceous vegetation in the tree rows” (P24L528-541).

Section 3.2.6 “Organic carbon inputs and SOC stocks: a synthesis from field measurements” was however kept in the Results (now Section 3.1).

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

Response: “Globally” was replace by “Overall” (P28L634 and P33L683).

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.

Response: We now also added to Figure 5 both SOC stocks in the control and in the agroforestry plot. This sentence was modified to “The spatial distribution of SOC stocks and of additional SOC storage was also well described (Fig. 5), with a very high additional SOC stock storage in the topsoil layer in the tree row” (P33L685-687).

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.

Response: We added the reference of Fontaine et al., (2007) (P39L706-707).

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.

Response: We now specified it “the two pools model with priming effect” (P41L756, P42L787 and P45L864).

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

Response: As suggested, we added a common legend for all sub-figures at the bottom of Fig 4 (P35). In the alleys, The PE model has almost similar BIC than the noPE model only because the first soil layer (0-10 cm) was worse represented by the PE model: (Model – Measures)^2 = 7.71 compared to 1.28 kg/m3 for the noPE model. As the BIC is calculated on the whole profile, this bad fit impacts the BIC even if the PE model performs much better for the other soil layers, this is well shown in Table S2.