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Interactive comment on "Can we model observed soil carbon changes from a dense inventory? A case study over england and wales using three version of orchidee ecosystem model (AR5, AR5-PRIM and O-CN)" *by* B. Guenet et al.

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Answer to comments from the reviewer.

Comments from the reviewer were left intentionally in this document and written in roman font. Our answers are written in italics.

General comments

The manuscript compares the performance of three variants of the ORCHIDEE model to reproduce an observed decrease of soil organic carbon (SOC) stocks in England and

Wales. Firstly, the authors use the original ORCHIDEE model (ORCHIDEE-AR5) to test if soil moisture and temperature dependent SOC decomposition together with dynamic litter input (CO2 fertilization, climate dependence) is able to explain the observed SOC loss between 1980 and 1995. Secondly, they test if microbial interactions are able to "prime" the decomposition of SOC when the amount of readily available carbon is increasing due to increasing litter input (ORCHIDEE-AR5-PRIM). Lastly, the O-CN model which includes interacting carbon and nitrogen cycles is tested against this potential benchmark dataset from the National Soil Inventory. Although ORCHIDEE-AR5 and O-CN have been published before, the test of all three model variants against the England and Wales SOC dataset is a valuable and novel model experiment. Especially, the ORCHIDEE-AR5-PRIM model theoretically shows a lot of promise for modeling the observed changes. However, the authors could be more specific for this variant (see specific comment 3), in order to increase the traceability of results (also see specific comment 7). Overall, I think that Guenet et al. (2013) did a good job at highlighting the possible advances of the three ORCHIDEE model variants over previous modeling attempts. At the same time they acknowledge in the discussion that ORCHIDEE-AR5-PRIM is a first, but not conclusive attempt at including microbial mechanisms into a ecosystem model. The manuscript is generally well organized and well written. I would recommend publication in GMD after the authors have addressed my (minor) suggestions () Specific comments) and Technical corrections.

Specific comments

1. In Eq. 1 the authors do not account for the rock fraction or the coarse fraction (cf. for example Rodeghiero et al. (2009)). Is there a reason for that?

These data were not available.

Were bulk density data from 1980 and 1995 available at all sites? The authors should state if changes in bulk density between 1980 and 1995 were insignificant, so that the observed changes in C stocks can solely be traced back to changes in C concentration.

The authors could maybe provide box plots of bulk density data, C concentrations and C stocks from all 415 sites of both years (1980 and 1995) to convince the reader that the C stock changes hinge mainly on changes of C concentrations.

The bulk density was measured only once; we are therefore not able to provide the details asked by the referee. The C changes observed have been fully discussed in other publications that we read carefully (Kirk and Bellamy, 2010, Foereid et al., 2012, Smith et al., 2007, Hopkins et al., 2009). We are therefore aware that the absence of measurements of bulk densities for each site at each sampling events is a drawback. However, Hopkins et al., (2009) argued that modifications of bulk density due to land use changes are well known but the quantification of other factors is not straightforward even if some indications exist in the literature. In this study, we used sites with no land use change to limit this problem the most as we can.

Hopkins, D., Waite, I., 2009. Soil organic carbon contents in longâĂŘterm experimental grassland plots in the UK (Palace Leas and Park Grass) have not changed consistently in recent decades. Global Change . . . 1739–1754.

Other references are cited in the manuscript

2. On page 3661, line 14 the authors state that they used a layer thickness of h = 0:2 m due to the fixed topsoil depth of 0.2 m in ORCHIDEE. In the description of the National Soil Inventory data in Bellamy et al. (2005) the topsoil is sampled from 0-0.15 m. The authors should clarify if the C concentrations kg C (kg soil) and bulk densities that were used to calculate the C stocks stem from this 0-0.15 m depth increment. If this is the case, the authors might want to discuss possible consequences.

We added details at line 141-145: "The C concentration was measured in the 0-15cm horizon (Bellamy et al., 2005). We therefore assumed that the concentration of C in the 15-20cm horizon was the same as the concentration measured between 0-15cm. We acknowledge that this assumption may induce an overestimation of the stock since concentrations typically decrease with depth."

3. In the description of the ORCHIDEE-AR5-PRIM model the authors could be more specific which model pools relate to fresh organic matter, FOM, in Eq. 2. Does the rate limiting term affect all three SOC pools equally, even the active organic C pool of CENTURY with a turnover time of 1-5 years (Parton et al., 1987)? Doesn't this pool also represent something like fresh organic matter (FOM) given the short turnover time and the description of this pool as active (Parton et al., 1987)? Could it also make sense to include the active organic matter in the definition of FOM, and let the rate limiting term only influence the slow and passive pool? If indeed the active organic matter pool does not contribute to the the FOM pool, the authors should give more arguments why the decomposition of specific pools should be affected by the rate limiting term. Furthermore, it might be interesting to provide the adjusted parameters of Eq. 2 in an additional table.

The active pool is considered as FOC as it is now explained in line 184-186: "Litter is divided into a metabolic and structural pool, differing in their turnover rates, and subsequently separated into aboveground and below ground inputs, resulting in four litter pools."

4. The authors could also discuss if it is reasonable to assume a temperature/ moisture dependency of the parameter c. Todd-Brown et al. (2012) assumed, for example, a linear temperature dependency of half-saturation constants when modeling microbial interactions with with Michaelis-Menten kinetics.

Since Ghee et al., (2013) showed that priming was insensitive to temperature, we decide to not add a temperature dependency on the parameter c. We added this information in line 205-206: "Ghee et al., (2013) recently suggested that priming was not sensitive to temperature we therefore decided not to define a temperature dependency on the parameter c."

5. In the Conclusions (P3670, L7) the authors state that "...none of these (models) could explain the observed decrease in C stocks". In my opinion this statement is a little

bit too strong, because the parametrization and structure of ORCHIDEEAR5- PRIM is not overly well constrained (c.f. specific comment 3). The authors could suggest, for example, that the parameters governing the decomposition of SOC in ORCHIDEE-AR5-PRIM (e.g. c in Eq. 2) could be calibrated against the observed SOC decrease as a possible improvement in the future.

We agree with the referee and a paper presenting an optimized set of parameters of ORCHIDEE-AR5-PRIM is in preparation. To avoid a too strong statement about the capacity of ORCHIDEE-AR5-PRIM to reproduce the data we added this part in line 389-403: "Our study further did not investigate the effect of soil C priming and N effects on C allocation. Under the rather rigid set of hypothesis of this study, the simplest interpretation of our results is that recent changes in land use must be the most plausible explanation for the observed C stock decrease over the UK. At face value, our results also show that that the sign of simulated changes in C pools is sensitive to inclusion of priming. This suggests that priming cannot be ruled out as a driver of the observed UK soil carbon changes, but that interactions between priming and land management (e.g. tillage bringing in FOC to deeper horizons and accelerating the decomposition of slow carbon pools, or a temporal change of priming intensity due to a modification of the C allocation in roots in response of plant N demand) would need to be simulated more realistically to address this question. Moreover, the parameters of ORCHIDEE-AR5-PRIM were not well constrained since they were adjusted to obtain the same initial C stock at equilibrium than ORCHIDEE-AR5. An optimization of these parameters against actual data may enable the ORCHIDEE-AR5-PRIM version to reproduce the trend observed over England and Wales."

6. In Figure 1 the distribution of the observed C stocks seems to be positively skewed - it might make more sense to report medians and interquartile ranges throughout the manuscript.

We agree that the use of mean $\pm$ sd was probably not the best approach in this case. However we prefered to compare the mean stock obtained with the model with the

mean $\pm$ confidence interval at 95

7. Links to documentation, homepages and download areas should be given for the established ORCHIDEE-AR5 and O-CN models, where available.

Done

Technical corrections

Suggested changes in italics:

Title: England and Wales should start with capital letters

Done

Title: using three versions of the orchidee ecosystem model

Done

Title: ORCHIDEE instead of orchidee?

Done

P3657, L7: heterotrophic respiration instead of soil respiration?

Done

P3657, L7: net primary productivity?

Done

P3659, L5: was found to increased SOC mineralization) increase

Done

Eq. 1 and P3661, L12-13: mismatch of units: the C concentration should be kg C (kg soil)-1 instead of g C kg-1 soil

Sorry for the mistake. Corrected.

P3661, L13: layer sampled (m), meter not in italics

Done

P3663, L17: I could not find Xiao et al. (2013) in the References.

Sorry, it was mistake, the paper by Xiao et al is under review but we removed this reference since our paper will be probably published before Xiao et al.

P3665, L20, L22: 1 - s standard deviation should probably be typeset as 1-s standard deviation?

It was the case in the docx file we sent but it has been changed before publication in GMDD.

âĂć P3667, L3: 0.310-3 ± 0.9 g C m-2 yr-1. The exponent seems strange, please check.

Sorry for the mistake 0.3 was correct (in previous versions the rates were in kgC m-2 yr-1).

P3667, L10: when comparing the biomass

Done

P3677, Table 1: mean ± variance should probably be mean ± sd

Done

P3666, L27: insert extra space between C and m in g Cm-2 yr-1

Done

P3668, L3-4: over estimated should be one word) overestimated

Done

P3666, L3-4: over estimated should be one word) overestimated

Done

P3667, L10: over estimated should be one word) overestimated

Done

P3666, L5: under estimated should be one word) underestimated

Done

Interactive comment on Geosci. Model Dev. Discuss., 6, 3655, 2013.