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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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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 (CO_2 fertilization, climate dependence) is able to explain the ob-

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served 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 ($\Rightarrow$ 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? 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.

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)$^{-1}$ and bulk densities ρ 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.

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, $(1 - e^{-c \cdot FOC})$, 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.

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.

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 ORCHIDEE-AR5-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.

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

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

Technical corrections

Suggested changes in *italics:*

- Title: *E*ngland and *W*ales should start with capital letters

- Title: using three version*s* of *the* orchidee ecosystem model

- Title: *ORCHIDEE* instead of orchidee?

- P3657, L7: *heterotrophic* respiration instead of soil respiration?

- P3657, L7: *net* primary productivity?

- P3659, L5: was found to increased SOC mineralization $\Rightarrow$ *increase*

- 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

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

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

- P3665, L20, L22: $1 - \sigma$ standard deviation should probably be typeset as 1-σ standard deviation?

- P3667, L3: 0.310^{-3} ± 0.9 g C m^{-2} yr^{-1}. The exponent seems strange: $0.310^{-3} \approx 33.6$, please check.

- P3667, L10: when *comparing the* biomass

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

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

- P3668, L3-4: over estimated should be one word $\Rightarrow$ overestimated

- P3666, L3-4: over estimated should be one word $\Rightarrow$ overestimated

- P3667, L10: over estimated should be one word $\Rightarrow$ overestimated

- P3666, L5: under estimated should be one word $\Rightarrow$ underestimated

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