

## ***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.***

**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 the roman font. Our answers are written in italics.*

Guenet and co-authors present a thoughtful, well written analysis that examines different versions of the ORCHIDEE model to evaluate what processes may be necessary to simulate observed declines in soil C pool in England and Wales. I'd love to see more

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papers like this that look at different modeling approaches and evaluate their ability to simulate non-steady state soil C dynamics. After minor revisions I feel the paper will be appropriate for Geoscientific Model Development.

Overall, I would encourage the authors to: 1) Provide greater detail about model structures being evaluated; and 2) Expand discussion / evaluation of model structures necessary to capture observer SOC trends from the National Soil Inventory.

Without being too familiar with ORCHIDEE (or O-CN) it's unclear what aspects of CENTURY are simulated here. It's implied throughout the text that the structure of ORCHIDEE is like CENTURY (e.g., p. 3662, l. 26). Several comments throughout the text make me think that ORCHIDEE is really CENTURY-like, and missing some key features of CENTURY that could be included?

*More information has been added, see details below.*

Finally, there seems to be some tension in the discussion and conclusion about what the authors really think needs to be done to capture soil C dynamics. The simple priming parameterization in AR5-PRIM seems promising for capturing the sign of soil C pools over the 20th century (although not over the NSI observation period). The timing of productivity changes simulated by O-CN may be more realistic- suggesting that model structures need to consider both priming and N dynamics to capture transient soil biogeochemistry dynamics. Guenet and others insinuate as much (p. 3669, l. 25-28), but conclude that such dynamics are “not straightforward”. I'd agree- but there seems to be a rich experimental literature that increasingly encourages consideration of revising soil biogeochemical models in ESMs to better align with emerging theories of SOM formation and stabilization.

*The discussion has been changed and we hope improved to take into account the referee's comments. For instance at l 375-377: “However, regarding the several processes controlling SOC dynamics as well as the numerous feedbacks between these processes, we do believe that a more mechanistic model is needed to reproduce the*

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trend observed in the data.”

at line 389-399: “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”

and at line 416-425: “But, as in our case, the results sometimes show that the major drivers are either unknown historical drivers (including drivers of priming changes) or mechanisms that are not yet well quantified and thus are not represented in our study. However, some of these mechanisms are already identified and should be incorporated in the next generation of land surface models. For example the breakdown of aggregates due to tillage, erosion, export of dissolved organic carbon, etc. Finally, the interactions between these mechanisms, such as through priming and N dynamics, must be incorporated in soil carbon models to tackle the difficult science question of attributing observed soil C changes to “biotic”, climatic or land use drivers.”

More specific comments follow: Title: England, Wales, and ORCHIDEE should be capitalized.

Done

P 3657, L 21-26. I very much like this statement about the ability of models to capture transient soil C responses, but feel like the ideas are poorly developed in the main text. Guenet and others do a good job providing a framework to test this idea, but

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fail to adequately explore the theory, structures, processes, or challenges that may be necessary to improve our confidence in soil C models. I realize this type of discussion is rather speculative and my not provide definitive answers, but I would encourage he authors to explore such idea.

*We tried to improve the discussion to further explore this idea (see answer to the last general comment).*

P 3659, L 7. The phrase “Nitrogen mechanisms” is vague and awkward. Can more precise language be used? Also, see suggested references that could be included here, or elsewhere.

*We precise this part at 80-86: “Moreover, by providing energy to decomposers through the exudation of labile compounds, plants may stimulate the soil organic matter mineralization to obtain N stored within the soil organic matter by providing the energy to decomposers through exudation of labile compounds (Philips et al., 2012). If more N is available due to an increase of N deposition or an increase in soil N storage from increasing fertilizers applications, plants can reduce C allocation to roots, and therefore reduce soil C inputs (Högberg et al., 2010).”*

P 3660, L 15-19. Is there a citation available for ORCHIDEE-AR5?

*The first version of ORCHIDEE has been presented in Krinner et al., (2005). Since 2005, all the modifications were presented one by one by different papers. Unfortunately, no paper describing the details of this version has been published but we added the website where the documentation can be found.*

P 3662, L 26-27. This would be one place to specify what CENTURY structure are used in ORCHIDEE (e.g., above and below ground pools, litter C pools, what environmental scalars modify rates of decomposition, does soil texture or pH modify decomposition rates, are effects of cultivation used?)

*More details are now given at line 183-190: “The simulation of SOC is based on the*

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equations of the CENTURY model (Parton et al., 1988). SOC is divided into three pools, which differ in their turnover rates. 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. Pool mineralization is described by first order kinetics. Furthermore, three scalars modify the turnover rate of each pool to represent the effect of soil temperature, moisture and texture on decomposition. To represent tillage, soil decomposition is increase by 20

P 3664, L 5. N limits decomposition rates in O-CN? Could an adjustment that reduces microbial growth efficiency in nutrient limiting conditions help agreement with observations in future analyses?

We do not think that an adjustment reducing microbial growth efficiency could help to better fit the data since O-CN underestimates the stock. If the microbial growth efficiency is reduced, more C will be respired for the same amount of substrate reducing the stock instead of increasing it.

P 3664, L 17. How are soil texture properties used, to calculate soil moisture?

No, not in the hydrology scheme we used.

P 3666, L 5. If N excessively limits decomposition rates (reduces k values) why would SOC stocks be too small? In OC-N either inputs are too low, or decomposition is too rapid.

The referee is right we changed the sentence to clarify at line 271-273: "The underestimation by O-CN could also be due to an underestimated representation of the N limitation on decomposition, leading to a too rapid decomposition."

P 3667, L 10-13. Modeled increases in productivity are only due to temperature and CO2 effects. My guess is that the drivers of increased agricultural yields from observations are more related to agricultural practices not simulated in ORCHIDEE. It seems somewhat misleading to compare these values without acknowledging this caveat.

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We added this part in the ms at line 309-311: "However, it must be noted that the increase of primary productivity in ORCHIDEE is only due to climate and CO2 modifications whereas the increase of crop yields is likely the results of changes in climate and CO2 but also in agricultural practices."

P 3667, L 15. The Schmidt et al paper is an amazing resource, however, it's excessively referenced in this manuscript any time the authors want to say something about soil biogeochemistry without even glancing at that literature. Some suggested references are listed at the end of this review.

We modified the reference to Schmidt et al., (2011) to Davidson and Janssens, (2006) and Conant et al., (2011)

P 3668, L 20-25. Could the dynamics and timing of NPP increases from O-CN improve PRIM results?

This was already discussed at line 370-377: "Had we included priming effects in O-CN, the acceleration of soil C inputs to the soil in the 1950's shown by O-CN but not in the other versions could have induced a priming-triggered acceleration of SOC mineralization but any conclusion about the quantification of such effect is not straightforward since there are several feedbacks between SOC mineralization increase, its associated release of N available for plants, the NPP and the C inputs into the soils."

P 3668, L 25-27. As parameterized N inputs don't show agreement with NSI results, but if these N dynamics were better represented (and included priming) could ORCHIDEE get closer to observations? Here's an opportunity to speculate on what could be / ought to be done with these models.

This is discussed later in ms (see answer above), however we did not give many details to avoid too many speculations. Nevertheless, we believe that a more mechanistic model is probably needed to really reproduce the data. We added this part at 375-377: "However, regarding the several processes controlling SOC dynamics as well as the

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numerous feedbacks between these processes, we do believe that a more mechanistic model is needed to reproduce the trend observed in the data.”

P 3669, L 1-7. I don't really understand how this analysis informs the questions being asked here. I gather that litter pools are explicitly simulated? But in my estimation these “trend” calculations don't answer the questions about how to simulated NSI declines in soil C pools using ORCHIDEE.

Indeed the litter pools are explicitly simulated. The ratio was calculated to compare SOC response independently of the litter dynamics, but since this calculation was rather uninformative we removed it.

P 3670, L 14-17. I really don't understand why the authors are so hasty to throw out priming and C-N dynamics from possible drivers of observed soil C declines across England and Wales, when both seem to potentially offer partial solutions to the problem. Yes, land use practices are also likely to blame- but don't unmanaged lands also show SOC declines in the NSI observations?

We modified the text to not totally throw out priming and C-N dynamics as possible drivers at line 389-399: “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”

P 3670, L 21-27. How do we deal with the complexity of soils in models across scales?

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What modifications to model structures are necessary to improve representation of soil C dynamics? This paper documents that we have a long way to go- but offers no suggestions on where we should consider heading.

To answer to the referee question we added this part at line 416-425: “But, as in our case, the results sometimes show that the major drivers are either unknown historical drivers (including drivers of priming changes) or mechanisms that are not yet well quantified and thus are not represented in our study. However, some of these mechanisms are already identified and should be incorporated in the next generation of land surface models. For example the breakdown of aggregates due to tillage, erosion, export of dissolved organic carbon, etc. Finally, the interactions between these mechanisms, such as through priming and N dynamics, must be incorporated in soil carbon models to tackle the difficult science question of attributing observed soil C changes to “biotic”, climatic or land use drivers.”

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

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