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Discussions

Interactive comment on "Diffusion based modelling of temperature and moisture interactive effects on carbon fluxes of mineral soils" *by* Fernando E. Moyano et al.

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We thank R. Grant for a critical evaluation of our work. Below we address each comment individually.

Reviewer comments are in quotation marks followed by author comments and, where relevant, changes to the manuscript.

"This is an interesting model study that makes a key point that higher order kinetics are needed to model respiration responses to changes in temperature and soil water con-

tent. This point is important because many SOC models still re- tain first order kinetics when making projections of climate change effects on SOC and hence on climate feedbacks, with a possible risk of error. Of particular interest to me but not often considered in modelling was the reduced sensitivity to temperature of mi- crobial decay vs that of uptake and growth, and the point raised in the Discussion about density-dependent microbial decay, as both these processes affect microbial biomass and hence decomposition rates in higher order models."

We agree. We found it particularly interesting that from a process perspective we can demonstrate that respiration dynamics do not necessarily reflect decomposition dynamics in the short and mid-term.

"The authors have done a good job in testing model performance against experimental results from soil incubations. However a more constrained testing of its performance would be achieved by comparing the actual time courses of CO2 emissions measured during the incubations, rather than just the totals measured over the duration of each treatment as done in this study."

Soil respiration was determined by measuring the accumulated CO2 in the flask's headspace at irregular intervals. As such, the data represent a time course, although irregular and of low frequency (we unfortunately did not have the capability to measure high frequency respiration rates). This actual measured data was what we used for modelling purposes. While this is already described in the methods, we added text in order to make the procedure clearer.

Changes in manuscript: added text near P.4 L.28-29, P.8 L.10-14

"Two key areas need to be developed for this model to be capable of more robust performance and hence wider application: the coupling of C with N for all transformations,

because kinetics of decomposition and respiration are strongly affected by SOC quality, and the simulation of O2 limitations on microbial activity rather than fitting declines in activity with higher soil water contents. Both these areas are already well developed in some ecosystem models."

We fully agree that these two aspects are potentially important for building predictive soil carbon models for general application. There are other processes that should be included in predictive models as well, such as oregano-mineral associations and vertical transport of SOM/DOC. However, this study has a defined focus and purposefully ignores many process that could be relevant under a variety of situations. This study does not present a generally valid predictive model. On the other hand, we believe the model showed a robust performance for the purpose of simulating our observations. Diffusion limitations as implemented here can, in future studies, be integrated in more complex predictive models and validated against larger sets of data. The variability in SR at high water content was captured by our diffusion-based model without a representation of O2 limitation. We note that this model does not include an empirical decline at higher moisture (as we understand is suggested in the comment). Only the saturation function in the alternative model we compare it with included such an empirical function. This does not imply that O2 limitations are not an important limiting factor in saturated soils and mechanistic simulations in that area are useful. In summary, our approach was to start from a simple model and add complexity until the observations could be reproduced, specifically testing the effect of adding diffusion. Adding further complexity would introduce parameter identifiability problems and add no relevant information. That said, we understand the comment addresses the “wider application” of the model and we now further address this by extending the discussion.

Changes in manuscript: added text near P.13 L.1-7

“P.2. L.15: a strong effects ? Introduction P.2 L.29: But note more rapid soil N miner-

alization, uptake, NPP and litterfall that may offset this feedback. N cycling very much needs to be included in any soil SOC model and the authors need to acknowledge this.”

We now acknowledge this in the discussion making the model limitations more explicit.

Changes in manuscript: added text near P.13 L.15-18

“P.4 L.16: Soil grinding and mixing will increase microbial access to SOC from that in a natural soil, likely raising decomposition rates.”

Agreed but unavoidable in this setup. To minimize such effects, we allowed samples to rest during pre-incubation.

“P.4 L.18: A BD of 1.8 is inconsistent with a porosity of 0.45. One or the other must be checked.”

Bulk density was incorrect and was changed.

Changes in manuscript: bulk density corrected to 1.4 g cm-3. P.4 L.18

“P.4 L.21: The saturated value of 0.25 is less than the porosity of 0.45.”

Please note these are gravimetric moisture values, so 0.25 in g/g is close to the 0.45 volumetric content.

“P.5 L.6: This statement is valid as long as the model simulates experimental protocol (e.g. duration of treatments).”

Agreed. Our simulations reproduced the exact incubation protocols we used.

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“P.7 L.3: Although if diffusion limitations reduce FPD you will also reduce CD and hence uptake, so make sure there isn’t a duplicated effect caused by direct diffusion limitation to CD.”

Since in the model diffusion affects enzyme pools and the availability of CD for uptake, there is a double effect on uptake, one indirect and one direct. This is intended. However, because CE decays and CD does not, long term effects will result from the former but not from the latter limitation. Unless CD is lost through another path such as leaching (here not considered).

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“P..7 L.7: Check variable names.”

Changes in manuscript: corrected variable name.

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“P.7 L.13: But temperature sensitivity of fmr is different from that of growth.”

Equations 15 and 16 had not been updated to the latest model version and were incorrect. They have been corrected. (But note that other manuscript sections were correct, e.g. parameter table). r_mr is in fact temperature dependent, as expected. Temperature dependencies were calibrated, since we did not find strong evidence for fixing these parameters.

Changes in manuscript: corrected a wrong version of equations 15 and 16. Now F_MP= C_M * r_md and F_MRM = C_M * r_mr

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“P.8. eq.19: Low and high temperature inactivation terms are often used with Arrhenuis equations to give greater Q10 at low temperature and must lower Q10 at higher while

using biologically realistic values of Ea (typically ca 65 kJ mol-1).”

With a simple Arrhenius function, we found that the observed Q10 can vary between low and high temperatures (see supplementary figures). For the range we used, between 4 and 35 degC, a more complex temperature function was not justified.

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“Was a spinup run used to enable key state variables to stabilize at values independent of those initialized? This is standard modelling protocol.”

A spinup would be valid only if a steady state at initial conditions is assumed. Our soil was from arable fields and pre-processes in the lab. Because of this we did not assume steady state at time 0 and instead estimated the initial pool sizes through calibration, as done in other studies (Menichetti, et al., Biogeosciences, 2016). This is stated in P.8 L.14-16.

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“P.8 L.25: This is a commendable objective because some SOC models still retain first order algorithms.”

Agreed.

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“P.9 eq. 20, 21: Reductions of f (theta) and f(psi) at higher theta and psi are caused by O2 deficiency as noted later in the text, and are better modelled as such because these reductions are temperature-dependent.”

We commented on the O2 limitations above and in the paper discussion.

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“P.10 L.4: MPa Results”

Changes in manuscript: spelling corrected

"P.11 L.6: How were C inputs evaluated, as in natural ecosystems these also vary with temperature and swc. In fact, these inputs are the most important part of a SOC model as they are the main drivers of microbial activity."

We used a fixed value of 1.2 g d-1 C, which we found to be realistic for cultivated temperate soils. Steady state was calculated analytically (supplementary equations) so inputs, temperature and swc needed to be constants, as described.

"P.11 L.13-14. This is a nice test of the model. Describe how values for initialization of C pools and threshold swc were determined for this study. How did these values affect model results, particularly without model spinup? Ideally you should just change total SOC as determined from the soil measurement, and develop rules for allocating total SOC to initial C pools depending on site conditions, and then spinning up the model to equilibrium before comparison with observed values. An even better test of the model would be against the actual time course of CO2 effluxes measured during each incubation, as has been done in earlier modelling studies (e.g. Soil Sci. Soc. Amer. J. 58:1681-1690). This test lets you see whether the model is really simulating the temporal dynamics of respiration at different water contents under changing temperatures."

As stated in P.9 L.23 – P.10 L.4., the C pools were initialized not by spinup but by calibration, given that also in the validation case we could not determine if initial conditions were in steady state. The reason to calibrate the swc threshold during validation is that this parameter is expected to change between soils but we do not currently have a reliable means to estimate it, as stated in P.10 L.1-4. We expanded the discussion where we address the issue of how pool sizes may be affecting the modelled and observed values

Changes in manuscript: text added near P.14 L.2–8

"P.12 L.10. Specify these changes as noted in Results to establish how robust the model really is."

P.12 L.10 reads "We note that few studies were found with data on moisture and temperature interactions and this was the only validation attempt carried out." It is not clear what changes are referred to.

"P.12 L.19-20. Would this problem be addressed by a cold temperature inactivation term in eq. 19?"

A further decrease in activity using an inactivation term, while realistic, would probably exacerbate the problem here, since it seemingly already is the result of the lower rates under colder conditions. A solution to this problem is however out of the scope of this study.

"P.12 L.29-30. The absence of O2 limitations is likely causing the reductions in Ea and Q10 at higher swc in Fig. 5. Modelling these limitations should be a key next step in model development. These limitations are already simulated in some other ecosystem models."

We believe it is likely not a O2 limitation for two reasons. First, the decrease occurs sharply at ca. 50% saturation. At this water content and in small samples O2 should not be limiting. Second, our model reproduced this decrease quite well without O2 limitations, showing that it is the result of pool dynamics.

"P.13 L.4-5: The reduced temperature sensitivity of microbial and enzyme decay needed to model realistic biomass at different temperatures is an important finding of this study."

Yes, our results are compatible with such lower values.

"P.13 L.15: Experimental determinations of Ea are often in the 65 kJ mol-1 range. The larger value modelled here may have been required in the absence of a cold temperature inactivation term in eq. 19."

If 4 degC is "cold" then this may be the case. It should be noted, however, that experimental determinations are "apparent" values. Apparent values given by our model are also in the lower range. However, our focus is more on the distinction between the prescribed values (parameter values) and the apparent ones and how these may interact with moisture. A detailed analysis of temperature effects is outside this study's scope.

Changes in manuscript: added text near P.14 L.2-8

"P.13 L.16-18. Lower values probably arise from O2 limitations. The authors realistically address the current limitations of the model."

We do not discard a O2 effect, especially near saturation. But see our responses above.

"P.13 L.32: models"

Changes in manuscript: spelling corrected.

"P.14 L.8-9. Why not make the percolation threshold depend on soil water potential (e.g. -15 MPa)? This might improve model robustness by reducing reparameterization for each soil."

The reason is that the published value was not valid for this soil, as explained in the following lines P.14 L.22-25.

"P.14 L.30-21: Would CUE decline at higher temperatures if Rm (fmr in (16)) increased exponentially with temperature, as it is known to do?"

As noted above, the equations using fmr were outdated and have been corrected. The parameter r_mr is temperature dependent and determines the Rm flux. If CUE changes with T may depend on its definition. Here we define it as f_ug, so it remains constant.

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