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Discussions

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Interactive
comment

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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General comments 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 content. This point is important because many SOC models still retain 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 microbial 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. 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. 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. Specific comments I would like to see several points about the model addressed before acceptance for publication, as indicated below: Abstract p.2. l.15: a strong effects ? Introduction p.2 l.29: But note more rapid soil N mineralization, 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. p.4 l.16: Soil grinding and mixing will increase microbial access to SOC from that in a natural soil, likely raising decomposition rates. p.4 l.18: A BD of 1.8 is inconsistent with a porosity of 0.45. One or the other must be checked. p.4 l.21: The saturated value of 0.25 is less than the porosity of 0.45. p.5 l.6: This statement is valid as long as the model simulates experimental protocol (e.g. duration of treatments).

Modelling approach 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. p.7 l.7: Check variable names. p.7 l.13: But temperature sensitivity of fmr is different from that of growth. 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).

Model Calibration Was a spinup run used to enable key state variables to stabilize at

C2

values independent of those initialized? This is standard modelling protocol. p.8 l.25: This is a commendable objective because some SOC models still retain first order algorithms. 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. p.10 l.4: MPa

Results 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. 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.

Discussion p.12 l.10. Specify these changes as noted in Results to establish how robust the model really is. p.12 l.19-20. Would this problem be addressed by a cold temperature inactivation term in eq. 19?

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.

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.

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.

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

p.13 l.32: models

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.

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?

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