

Interactive comment on “Disentangling residence time and temperature sensitivity of microbial decomposition in a global soil carbon model” by J.-F. Exbrayat et al.

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The authors used a reduced complexity model (one pool soil decomposition with a temperature and moisture dependency) to study the sensitivity of the carbon stock projections to first order uncertainties. The relative contributions of decomposition (k) and temperature sensitivity (Q_{10}) to the change in soil carbon stocks are relevant and interesting. However I have a major concern with the use of this particular RCM in examining the change in soil carbon stock.

I'm concerned that the proposed RCM would not be a good predictor of the change in ESM soil carbon as implied by the manuscript. I would like to see a comparison with the

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RCM predicted dC and the full ESM simulation run. Todd-Brown et al 2013 a,b did show that the RCM explained the initial distribution of C well, assuming steady state (Todd-Brown et al 2013a), and the distribution in Rh both spatially and temporally (Todd-Brown et al 2013b). However this is unlikely to directly translated into well explained dC over the time period since the RCM only capture the first order differences and dC is the result of relatively small differences between inputs and outputs.

Either the authors need to go into greater detail on this caveat in the discussion and a justification in the methods section or show explicitly that these first order differences do, in fact, govern dC in the ESMs. Alternatively the paper could be refocused on Rh instead of soil carbon to sidestep the problem of second order contributions to dC .

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