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Interactive comment on "Soil fauna: key to new carbon models" *by* J. Filser et al.

Anonymous Referee #1

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This MS makes the important argument that understanding the exchanges and fates of carbon between soils and ecosystems requires deliberate consideration of soil animals and their actions in determining the build-up of soil organic matter. Carbon modeling has been dismissive of animal effects largely because of an under-appreciation of the multiplier (knock-on) effects that animals can have on geophysical processes. The MS provides an almost encyclopedic, detailed review of the myriad, mechanisms by which soil animals influence the amount and fate of organic matter by altering geophysical processes in terrestrial soils. This clearly-written MS covers the relevant literature and thereby provides the reader a useful entre into the scope of work on soil organismal biology as it relates to soil carbon.

The MS is intended to convince carbon modelers that they need to consider the effects of soil fauna in order to enhance the predictive power of the models and thereby develop more accurate accounting of soil carbon. But, here is where I think the MS

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Discussion paper

falls short. While it presents important ideas, the material isn't communicated in a way that can be appreciated by modelers, who routinely tend not to be empiricists, let alone experimentalists, and therefore likely wouldn't concern themselves with the fascinating, but minutiae of, detailed mechanisms presented in the MS (or wouldn't know where to begin to incorporate the details into models). The MS instead would likely resonate most with soil biologists who are fascinated by the details, and thus the MS tends to "preach to the choir" so to speak. The MS would be strengthened if it played more directly to the perspectives or needs of modelers. But, here is where the authors need to decide on which direction to go.

From where I stand, making a convincing case for including effects of soil animals in modeling needs to provide one of two important pieces of information (if not both).

The first would be to provide modelers with a clearer sense of how discrepant their model predictions are because they don't include animal effects. That is, how much does the presence vs. absence of groups of soil animals influence the amount of soil carbon that is stored or lost. The MS gets at it a bit when discussing earthworm effects (line 344-346). But, there is scant other evidence provided to support the argument (beginning on line 347) that without considering the role of animals models will be less accurate. It would be helpful to know quantitatively how inaccurate the models will be if animal effects are not included (i.e., how much of a difference in carbon balance estimates is there?). I appreciate that this may be tough to do because empiricists and experimentalists aren't accustomed to examine soil biology and relate it to quantitative estimates of carbon balance in a form that is useable by modelers. (There is a lesson for empiricists here too).

Second, as the MS correctly points out, many of the biological details presented in the MS are ignored or simply subsumed as "parameters" in models that describe big-scale processes. This is typically done as a matter of mathematical convenience because abstracting a complex process as a parameter keeps the model reasonably tractable. But, accounting for animal effects in ways described in the MS requires characterizing those processes in terms of model functions, functions in which the levels of soil carbon due to a specific mechanism (e.g. soil bioturbation, or aggregate formation) vary with the abundance of the animals performing the mechanism. However, converting parameters into new functions can be a daunting exercise from two standpoints. First, empiricists and experimentalists tend to examine processes in terms of effects due to presence/absence of species and often do not vary animal abundance to measure what form a function should take. Second, empiricists are enamored by biological details, but often don't give priority to which details might matter more than others. This can cause concern to modelers because including each and every detail can make the models vastly complex, therefore making model output extremely difficult to validate and therefore understand. So, given heterogeneity in soil properties across large geographic spaces, do we need to know accurately variation in local soil molecular structure, or local root processes, or local physical heterogeneity or local aggregate formation to inform regional carbon budgets? If yes, then how? What I am driving at is that the MS would be strengthened if it provided a better road map of what processes should be an immediate priority to include in modeling and what level of detail needs to be included in the models. This road map could be strengthened if the MS could offer a sense of what the functional forms of the processes might look like (i.e., can we assume linearity? Must we consider nonlinearity? If nonlinearity, then what form should the nonlinear function take?). Most importantly, if accounting for spatial variation in animal effects matters, then what is a reasonable spatial scale over which one can assume that animal effects are reasonably similar. That is, it would be impossible for models of regional carbon budgets to account for heterogeneity on a mˆ2 x m2 basis. What spatial scale could be reasonable: kmˆ2 x kmˆ2, 10 kmˆ2 x 10 kmˆ2? Solving this scaling problem is perhaps the most critical issue when trying to align models with empiricism. In my experience, this is what causes the biggest rift between modeling and empiricism: empiricists again tend to focus on details of very fine spatial heterogeneity and disagree with efforts to subsume that heterogeneity in to a reasonable large-scale spatial approximation.

Ultimately, the issues raised in the MS are not merely issues that should be of concern just to modelers. Empiricists need to appreciate the challenges and demands of modeling and provide empirical input that can help meet the challenge by tailoring empirical estimates and analyses to explicitly inform modeling. There was a large movement afoot in ecology in the 1990's to do a better job of melding modeling and empirical work. Modern ecology seems not to have heeded that too much. Perhaps the important message of this MS is that we need to begin heeding this a lot more going forward.

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