SOIL Discuss.,
doi:10.5194/soil-2016-19-RC3, 2016
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Interactive comment on "Soil fauna: key to new carbon models" *by* J. Filser et al.

Anonymous Referee #3

Received and published: 22 May 2016

GENERAL COMMENTS

This generally well written manuscript reviews the evidence for the need to incorporate soil fauna in SOM stabilization and dynamic models. It frames its review in the light of Schmidt et al.'s 2011 thought-provoking paper in Nature discussing the persistence of organic matter as an ecosystem property. As pointed out by other reviewers, the paper contains a wealth of biological detail. However, I think that this detail leads to the main message of the paper (which I assume is the need for the SOM modelling community to incorporate fauna) being lost.

To remedy this, I think the authors need to do four things:

a) If possible, clearly demonstrate how the presence or absence of fauna lead to changed model / empirical predictions of SOM dynamics. The controls for the examples in Table 1 are not always explained, so it is not clear how fauna are or are not

important. The modelling importance is touched upon around lines 347 but could be developed further, and explicit links could be provided in the earlier empirical review.

b) Clearly demonstrate that the already included model parameters (e.g. of climate, land use etc) do not adequately predict faunal composition (it is mentioned briefly with De Vries et al. 2013 on line 349). If land use/climate parameters can (generally) predict faunal composition then it is not immediately clear why fauna need including in SOM dynamic models if climate, land use etc can also predict SOM reasonably accurately [which I realise is not always the case]. As noted by another review, do the discrepancies in model results mean that fauna need including, or is it that other processes (e.g. leaching, particulate loss, litter inputs) could need refining to improve model accuracy instead.

Acting on this comment though does depend on model aims - if the aim of the model is to provide prediction, do they need to be mechanistically accurate (in the extreme, can they actually be statistical?). If however the model is aimed at mechanistic understanding, then the need for faunal incorporation potentially becomes clearer. As noted by the authors, this aspect may become even more important when trying to account for environmental change.

c) The authors mention in the Abstract that 'the contribution of soil fauna activities can be as high as 40 %' but it is not clear in the main text where this figure comes from. More importantly, what is the distribution of faunal importance to SOM dynamics/stability in ecosystems? Is it that there is one study demonstrating this level of importance, but others only show a negligible contribution? If so, then perhaps the importance of incorporation of faunal activity into SOM models is being overblown. [I personally think we do need to think about its incorporation, but the evidence presented here is not as clear as it could be] It may be that with the data currently available the distribution cannot be assessed. If so, then this aspect should, in my opinion, at least be discussed.

d) The title suggests 'new' models are required. It might be good if the authors could synthesize their review to put forward a conceptual model framework that contrasts with currently available frameworks e.g. RothC, CENTURY.

SPECIFIC COMMENTS

Abstract - line 60 - we suggest that inclusion of soil animal activities can fundamentally affect the predictive outcome of SOM models.

This is a very strong statement which I do not think has been clearly demonstrated in the paper at present. Perhaps addressing a) to c) above will help justify this. I can only see one example in the paper (referring to earthworms in CENTURY) and am not sure this is a 'fundamental' difference in prediction. What is a fundamental difference in prediction anyway - differing magnitude, differing direction (e.g. carbon source or sink), something else...?

Line 68 - I don't think there is any need to advertise the COST action in the Abstract (it occupies 6 lines out of 24), and it would be better to finish with a strong statement of what this review paper has found and its implications. I would also remove reference to this COST action at the end of the main text - it seems like a weak ending to the paper unless this is the main message you want to communicate.

MAIN TEXT

Line 104 - please provide more details on the correlative field study. Of what? What discrepancies?

Line 118 - point out regional differences in or of what? We already know about regional differences in climate, land use, soils?

Line 151 - "modifications in molecular structure have significant effects on its [SOM] dynamics." This is presumably the important point in terms of justifying incorporation for modellers yet there are no references backing this statement up. In relation to point c) above, what is the quantitative importance?

Line 155 - "the term humic substances is considered outdated"

This is a very strong statement yet many modellers continue to use these conceptual pools which reflect different rates of organic matter turnover. You could alienate readers with such phrasing, likewise on line 328 with "As there is no scientific support for widespread belief in humic substances".

Indeed, current research is utilizing mid infrared spectroscopy to examine particulate, humic, and resistant organic carbon, operationally defined pools which match well with conceptual pools in the models. These results have then been validated against observed changes in soil C following land use change (Paul et al. 2016 in review; see also Baldock et al 2013 Soil Research 51: 577-595). Does this means SOM models need a fundamental overhaul or does it depend on what you are trying to predict?

I don't think these statements are required for the broader message of the paper and would suggest tempering them.

Line 180 - what happens to fire derived carbon in absence of soil fauna?

Line 220 - what does discussion of soil depth mean for SOM dynamics. Make the links explicit here and in the other sections.

Line 241 - please put quantitative figures on 'a large contribution' i.e. define large.

Line 273 - why does tensile strength matter for SOM dynamics.

Line 289 - density of what?

Line 311 - how does Figure 2 demonstrate that specific effects of soil organisms differ across space? How has increasing importance in humid-warm and nutrient-limited conditions been demonstrated? Does the absolute / relative difference with and without soil organisms increase in these conditions? Or is it that soil organic matter dynamics are faster in humid warm conditions and so the presence of animals is confounded with these climatic conditions?

Line 344 - this sentence could be clearer - was the slow C pool maintained when earthworms were present in the model?

TECHNICAL CORRECTIONS

Line 393 - "A number of workshops" [not 'workshop'].

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