

Interactive comment on “Response of microbial decomposition to spin-up explains CMIP5 soil carbon range until 2100” by J.-F. Exbrayat et al.

Anonymous Referee #1

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General comments:

Exbrayat and co-authors present an important finding that initial conditions strongly control projections of soil C dynamics from the CMIP5 archive. Although well recognized in atmospheric sciences, I'm not sure similar insight is often noted in global biogeochemical dynamics. Beyond this important finding, however, the other analyses and discussion presented don't offer much new insight into refining our understanding or representation of soil C processes across scales, and display items (besides Fig. 1) are not that different from results already published by Todd-Brown and others (2013, 2014). More broadly, I'm concerned that some parts soil C community may be overly interested in constraining uncertainty in soil C projections, but not necessarily doing so for the right reasons. For example ideas discussed by Knutti & Sedláček (2013)

C962

relating to the physical climate system certainly apply to C cycle projections as well. In my estimation, discussing these considerations would improve the present manuscript.

Specific Comments:

I'm not sure why the authors report separate values from the same modeling centers (e.g., models E&F, G&H, I&J, K&L, N&O)? Given the similarity of results reported here (Table 3, Figs 1-4) and previous work (Todd-Brown et al. 2014), these don't really appear to independent observations. It also doesn't appear that soil biogeochemical or land models are different among these duplicated models. Thus, I would encourage the authors to consider repeating the analysis without unwarranted pseudo-replication.

Results presented here are astoundingly brief with three more figures presented in the discussion. I'd consider revising the manuscript so that analyses that are not introduced until the discussion but described in methods and results.

Figure 2 doesn't seem to present any valuable information, since the authors calculated SOC inputs (eq. 3), and by definition there is no change in initial SOC pools. Thus, the 1:1 relationship presented only confirms that the CMIP5 models were spun up correctly, such that $SOC_{in} = Rh$.

Similarly, the finding presented in Fig. 4, that initial global SOC pools are directly related to their residence time (calculated here as SOC/Rh , or the inverse of their decay rate) is also not that surprising. Moreover, this result is not markedly different from the reduced complexity model already presented by Todd-Brown and others (2013, 2014) that explains a most of the variation between CMIP5 models.

The logic supporting the recommendation that simulating initial / or present day soil C pools may improve confidence in future projections seems tenuous at best (p. 3489, l. 23-27 & Conclusion). I agree, this would reduce variation in model projections, but it provides no constraint on the process level representation in models. Moreover, soil C pools may be significantly underestimated in the HWSO, especially at high latitudes.

C963

Thus, following recommendations to initialize models to the HWSO dataset may omit critical permafrost C dynamics and climate feedbacks in this C rich region, but such considerations are never discussed in the manuscript. Separately, couldn't models achieve appropriate present day soil C stocks, but have wildly different environmental scalars (fT and fW , eq. 2) that would provide alternative sensitivities to environmental change in future scenarios (also see Friend et al. 2014). Could variation in soil C inputs drive divergent projections in soil C storage- especially in future scenarios? These parameters can also be constrained w/ observations (e.g. Rayner et al. 2011), but a thoughtful discussion along these lines is absent from the current manuscript.

Technical corrections:

Was N active in the BCC-CSM1.1 simulations used here? (p. 3485, l. 12)

This sentence seems awkward "We also averaged all realizations of the same model to retain one estimate per structure and account for model dependence". (p. 3486, l. 2-4)

What are the structures referring to? Were ensembles from the same model (Table 1) averaged to give a single value for each model (Table 3, Figs 1-4)?

What are the "outliers" referred to on P. 3488, l. 9? If this in reference to models outside the HWSO observations in Fig 1, the logic seems confusing since the next sentence (relating to Fig. 2) indicates that models have been spun-up appropriately ($R_h = SOC_{in}$).

References:

Friend et al. (2014). PNAS 111, 3280–3285. doi: 10.1073/pnas.1222477110

Knutti & Sedláček (2013) Nature Climate Change 3, 369–373.

Rayner et al. (2011). Phil. Trans. R. Soc. A 369, 1955-1966.

Todd-Brown et al. (2013). Biogeosciences, 10, 1717–1736.

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C964

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