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Biogeosciences
Discussions

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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This manuscript describes a sensitivity analysis of residence time and Q10 in a global soil carbon model. The analysis examines responses of soil carbon stocks and future soil carbon change. The study is valuable because it allows for direct comparison of changes in residence time versus Q10 on soil carbon processes. The conclusion is that residence time is most important for determining standing stocks and the magnitude of C change, whereas Q10 determines the direction of response to temperature change, locally and globally.

I think the comparison is valuable, but I don't think it should be surprising that residence

time controls carbon stocks in a first-order model. I suggest shortening the discussion of this overall result. It is more interesting that the initial stocks, as driven by the residence times, determine the magnitude of response to temperature change. This point is somewhat buried in the discussion, but I think it should be emphasized more clearly.

In the introduction, I am missing a statement of the key question this work seeks to address. Clearly the goal of the paper is to determine the relative influence of turnover versus Q10 variation on soil carbon stocks. But what is the motivation for doing this? Can this work be placed in a broader context of improving the global models? Is the assumption that if models used accurate turnover times and Q10 values that matched observations, then the model predictions would be valid? I'm not so sure, given that most models, including the one used here, do not replicate spatial patterns in soil carbon very well.

There are some clear patterns in this analysis that raise questions about the validity of the underlying model (or any similar first-order model). It is good that the authors compared the stocks to the HWSD, but there is no further discussion on the realism of the model outputs. One issue is the overall size of the historical and current soil carbon sink (see comments below). Another issue is the spatial distribution of soil carbon change. The authors contend that mid-latitudes will determine the sign of soil carbon balance, but that's only true if the model assumptions about zonal drivers of soil carbon storage are correct. For example, even with the highest Q10 values, the models predict large carbon storage in boreal/tundra latitudes. Yet most empirical and biogeochemical evidence suggests that high latitude soil C is highly vulnerable to climate change (see work by Schuur and others). Conversely, soil mineralogy could constrain the temperature response of decomposition in tropical soils. Some of these issues are discussed in another recent paper by Todd-Brown et al. in BG. In short, current biogeochemical models lack important mechanistic details and produce questionable predictions about zonal soil carbon change.

I think that discussing the plausibility of some of the results in terms of other empirical data (in addition to the HWSD) would strengthen the paper. Still, I like the analysis because it represents a controlled analytical approach for examining two important drivers in detail.

Specific comments:

4997:21: It's not so much the model parameterization that's criticized, but the model structure and specifically the first-order, substrate-driven nature of decomposition losses. Same for line 28.

5001:8-10: What is the basis for the choice of these parameter ranges? They seem reasonable, but perhaps some citations can be included.

5003:20-21: This result seems unlikely. Is there any evidence that soils have accumulated C at this rate over the historical period? The highest estimates would require rates of ~2 Pg/yr, which is nearly the size of the entire current land sink. This result seems to question the validity of the underlying model processes, at least for the longest residence times.

5004:5: Use of the word "significant" implies statistical significance; better to choose a different word here.

5008:15-16: I am skeptical of the size the soil C sink in the current analysis. Many of the studies cited here are other modeling analyses, and all the models are quite similar in their response of NPP to CO2 and their response of soil C to NPP. I don't think there is compelling empirical evidence yet that the land sink has that much of a soil component. Can we really rule out that the land sink is all driven by vegetation?

5009:18: I'm not sure I think that this is counterintuitive. It's clear that turnover controls the equilibrium pool size and Q10 controls the temperature response. The temperature response is a fractional value, so it makes sense that you get a bigger absolute change if you apply the same fractional change to a larger pool size.

Fig. 2: The caption needs to clarify that the dashed lines are the model runs that

produced soil stocks within the 95% CI of the HWSD. The way it's written now makes it seem like the HWSD has soil C change in it.

References:

Belshe, E. F., Schuur, E. a G. & Bolker, B. M. Tundra ecosystems observed to be CO2 sources due to differential amplification of the carbon cycle. Ecol. Lett. 16, 1307–1315 (2013).

Schuur, E. A. G. et al. Vulnerability of permafrost carbon to climate change: Implications for the global carbon cycle. Bioscience 58, 701–714 (2008).

Todd-Brown et al. 2014: http://www.biogeosciences-discuss.net/10/18969/2013/bgd-10-18969-2013.html

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