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Biogeosciences Discussions

Interactive comment on "The relative importance of decomposition and transport mechanisms in accounting for C profiles" *by* B. Guenet et al.

B. Guenet et al.

bertrand.guenet@ua.ac.be

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Answer to comments from the reviewer.

Comments from the reviewer were left intentionally in this document and written in roman font. Our answers are written in italics.

Comments from J. G. Wynn :

This paper provides a very comprehensive and inclusive model study of the mechanisms of soil organic matter transport and mineralization, using a long-term bare fallow experiment as model input. The study includes for the first time the combination of an analysis of two approaches to soil organic matter decomposition (first order kinetic, and first order kinetic with "priming"), and the three possible permutations of soil

organic matter transport mechanisms (diffusion, advection and both). The modeling study uses Bayesian statistics to assess the validity of the variously combined approaches to soi organic matter transport for control profiles and experimental profiles in a uniquely long-term study (58 yrs). This approach to model-data fusion (with superb data quality) is encouraging, and the goals are appropriate for such a study. However, I find the manuscript, as written, a bit rough, and in need of moderate revision before I could suggest publication.

Major comments: I would suggest a change to the title. First of all, the word soil should appear somewhere, since a reader is not likely to immediately distinguish this work from (for example) a study of similar processes in oceanic depth profiles , which would very much be relevant to Biogeosciences. Also, I think it would be useful for the title to distinguish that this is a study of organic carbon (as opposed to inorganic carbon, for which similar methods could be employed). So, I would suggest a change to the title "The relative importance of decomposition and transport mechanisms in accounting for soil organic carbon profiles"

We changed the titles following the referee comment.

On p.14150, the authors describe their method for dealing with the fact that the fallow profiles are likely to have compacted over the 58 yrs of the study due to loss of mass of organic matter, and rearrangement of soil particles, as compared to the control profiles. The method used concerns me, because it is one of the first permutations done to the primary data (SOC vs. depth), and could very much affect the model outcomes.

More importantly, I think the method used to "decompact" the profile data is flawed in the assumptions made. Furthermore, the amount of decompaction is not insignificant (10cm in 100 cm), and therefore the flawed assumption could bias the entire study.

The entire analysis should likely be repeated after using a more theoretically-based approach to soil compaction/decompaction. Although not enough details are provided on how this was done (how were each of the LTBF plots of different ages dealt with?),

the authors used a simple linear function to decompact the fallow plots. We know from a theoretical and observational point of view, that compaction of sediments or soils is not a linear function of depth, but rather depends on the initial bulk density (horizons with more pore space are more susceptible to compaction), and the change in organic matter content (horizons which lose more mass of organic matter during LTBF decomposition, are more susceptible to rearrangement of soil particles and "densification). This is widely known from studies of sediment compaction (predominantly from the field of geotechnical engineering), and various theoretical approaches have been employed to reconstruct the pre-compaction profiles. I would leave it up to the authors as to how to deal with this issue, but clearly the effects of compaction would have preferentially occurred in the surface horizons (upper 30 cm or so). Not properly accounting for this could have severely biased interpretations of the importance of various transport mechanisms, by inaccurately shifting the curve of the SOC depth profile, especially when account for transport between "shallow" and "deep" layers, as is the focus of this study. Finally, the conclusions paragraph needs to be entirely rewritten.

Since we calculated the C stocks kg C m-2 the compaction effect is integrated in the data using the bulk density. The same data in kg C m-2are used to optimize the parameters and therefore the compaction effect on transport is implicitly taken into account.

We used a "decompaction" function only to better represented on the graphics what is observed on the site (the bare soil floor is under the steppe floor). Because this part was not that clear, we modified as following: "To take into account graphically the compaction effect on soil depth, we define the point at 0m depth as the floor of the steppe and then the soil layers were assumed to be linearly compacted through time since 1947 to reproduce the observed final difference of 10 cm between the two bottom horizons."

We also added in the section 2.3: "The compaction observed on site and its effects on transport are taken into account through the use of the bulk density in the stocks calculation in the dataset. The compaction effects are implicitly represented in the

model as the optimization was performed with the stocks expressed in kg C m-2."

Besides not being a concluding statement, the first sentence seems to a new topic of discussion (although loosely tied together and rambling). It is not clear what is meant by "crossing point between the dashed lines." Some of the text in this paragraph (once revised) could be a good ending to the discussion, highlighting the deficiencies of the models employed, and future research directions.

We included this part to the discussion and we modified the methods to calculate the variance of each parameter. More details are now given in the methods section and the discussion also deeply changed.

Minor, text-level corrections: p.14146,l.4: Soil Organic Carbon is not a proper noun, and need not be capitalized.

Done

p.14146,l.25: Begin the sentence "This suggests" with something more specific than "this." What about "this" suggests the conclusion reached (it is not clear from the previous sentences).

The sentence was modified: "Interestingly, the older the bare fallow is, the lesser the need for diffusion is suggesting that stabilized carbon may not be transported within the profile by the same mechanisms than more labile carbon."

p.14147,l.1: Replace "continental" with "terrestrial"

Done

p.14147,l.6: Replace "first soil layers" with "surface" or "surface horizons"

Done

p.14147,l.7: Replace "region of the soil" with "depth" (region could be interpreted as geographic).

Done

p.14147,l.14: “layers” should be plural.

Done

p.14148,l.1-2 (continued from previous page): “: : :during two consecutive time steps is proportional to the pool’s size: : :” This statement is very unclear, and it is not easy to deconstruct what the authors have done here. Please elaborate on how time steps are proportioned to pool size. Since this study is largely a description of a new model, these details are crucial to the utility of the research.

We modified this part: “In this formulation, the decay of each SOM pool is proportional to the pool’s size, thereby considering no interactions between two decomposing pools.”

p.14148,l.18: remove “just”

Done

p.14149,l.5: “plot” should be singular.

Done

p.14149,l.22: Replace “before” with “previously”

Done

p.14150,l.2: Replace “can be” with “is”

Done

p.14151,l.4: Remove “The different” and “tested” (in general, the section headings could be simplified throughout).

Done

p.14152,l.9: Remove “a” between “follow” and “first”

Done

p.14152,l.14: “decomposer” should be singular.

Done

p.14153, end of section. Somewhere in the methods section it should be specified what software was used to accomplish the model, method used, etc. It would be great if model code could be included as a supplementary document.

Done

p.14155,l.1: No new paragraph for this sentence.

Done

p.14156,l.27: “moves”

The results section has been deeply modified

p.14158,l.6: the meaning of “shows off” is unclear. Replace with more formal phrasing. Maybe “highlights”?

We change this sentence

p.14158,l.8: remove “one” after “kinetics”

Done

p.14158,l.20: Begin sentence with “This was : : :”

Done

p.14160,l.6-7: “However the oldest is the SOM” is unclear. I can’t suggest a rephrasing of this sentence, because I am unclear on what is meant.

This part has been deeply modified and this sentence has been deleted

p.14160,l.8: “parsimony principle” is singular.

Done

p.14160,l.14: Begin sentence with “This suggests : : :”

Done

p.14160,l.17: The statement “Diffusion is often used to account : : :” needs to be referenced. If the authors are saying something about other research methods, they need to cite this other research.

References have been added.

p.14160,l.27: remove “a” between “by” and “first”

Done

A few comments on figures: The text in Figure 3 will be difficult to read without some resizing for final form.

We improved the figure.

The text of the caption for figure 6 should be more specific about which model was used (one of the two decomposition modes, and two transport mechanisms).

All the models share the same input scheme. We modified the caption: ‘Fresh organic matter for the four profiles calculated by the model. The steppe, the 20YBF, the 26YBF and the 58YBF are represented by the green line, the blue line, the black line and the red line respectively. All the models share the same input scheme. We assume in the models that a fraction of the SOM decomposed may be as labile as the FOM and is therefore incorporated to the FOM pool’

Interactive comment on Biogeosciences Discuss., 9, 14145, 2012.