

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

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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 soil 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,

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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”

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 “densifi-

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ciation). 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. 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.

Minor, text-level corrections:

p.14146,l.4: Soil Organic Carbon is not a proper noun, and need not be capitalized.

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).

p.14147,l.1: Replace “continental” with “terrestrial”

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

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

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

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

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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.

p.14148,l.18: remove “just”

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

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

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

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

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

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

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.

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

p.14156,l.27: “moves”

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

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

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

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.

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

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

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.

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

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

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