

Interactive comment on “Apparent ecosystem carbon turnover time: uncertainties and robust features” by Naixin Fan et al.

Naixin Fan et al.

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Dear referee,

Thank you for making these constructive comments on our manuscript! I am glad you like the results we want to present. The responses to your comments can be found below. Please also take note on the marked-up manuscript in the attachment, where all the changes from the original submission are highlighted.

The responses:

I would be interested to hear more information about some of the derived datasets. For example, the creation of the herbaceous carbon stock map is described but what is the relative proportion of vegetation carbon found within the herbaceous layer is

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not stated? As the GPP ensemble is used in the estimation of the herbaceous layer what is the uncertainty in the herbaceous carbon content? How does the herbaceous carbon stock influence ecosystem turnover time vary in space, i.e. could it have been neglected? Answer: We added some analysis of herbaceous biomass in the text per request. Please see the updated manuscript in detail but the simple answer is that the herbaceous biomass plays a minor role in the estimation of τ since it is less than 1% of soil carbon stock and 5% of vegetation carbon stock.

Similarly, the soil carbon estimated to maximum depth would be interesting to investigate further. A really simple but nice addition would be a map of the maximum soil depths inferred by your analysis. Answer: We added the soil depth global distribution map (Figure S6). Please also note that the full soil depth is not inferred by our analysis but a global dataset (see Method). The current text is a little unbalanced towards Csoil sometimes to the exclusion of Cveg or GPP in the introduction, results and discussion sections. The introduction sets out the overall challenge and usefulness of such datasets in constraining Earth System Models and their role in quantifying the response of the terrestrial ecosystem to climate change. However, the fact that this is an update paper is not made fully clear. Doing so would I think make it straight forward to highlight the weaknesses of the previous analysis and how they are being improved here making a more robust and unique dataset. I honestly do support making updates and improvement to existing datasets as this provides a clear traceable advancement in the science. Because the current manuscript does not clearly highlight soil as a weakness / uncertainty of existing works the introduction reads as being very soil dominated with little introduction of the vegetation carbon stock challenges or the estimation of GPP.

Answer: Yes, we updated the manuscript to a more balanced way. Please see details in the line to line answer below and the marked-up manuscript.

The introduction does clearly state one of the key assumptions, that ecosystems are assumed to be in steady state. What is missing is an appreciation that much of

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the world's vegetation is not in steady state, either due to direct human intervention (biomass removal or other land use change) or as a result of increasing CO₂ concentration. Attempting to quantify this is out of scope but I think it would be useful to include either in the introduction or discussion the potential implications of this assumption leading to an underestimate in turnover times (e.g. Ge et al., 2018). Answer: Yes, we updated the manuscript to address this matter. Please see details in the line to line answer below and the marked-up manuscript.

The results section, like the introduction, seems to be biased towards soil carbon results rather than a complete overview. This should be addressed. Further information can be found below in the technical comments. The discussion lacks any discussion of the vegetation carbon stocks and almost any discussion of the GPP estimates. I also find it odd that figures 1-4 are not mentioned in the discussion at all. The discussion lacks sufficient comparison with existing studies / ESM outputs which this dataset should be constraining. One exception being the comparison with Todd-Brown et al., 2013 comparing soil carbon turnover times from CMIP5 models. Discussion of GPP importance is limited to its uncertainty contribution in the current analysis. While I have no problem with your choice to use FLUXCOM GPP estimates as observation-orientated. I do think it would be useful to include some discussion / context that compares your GPP estimate to alternate approaches e.g. remote sensing products (e.g. Zhang et al., 2017) or terrestrial ecosystem models constrained with remote sensing (e.g. Norton et al., 2019). Answer: Yes, we updated the manuscript to a more balanced way. Please see details in the line to line answer below and the marked-up manuscript.

Response to the technical comments: Please check the modified manuscript if the answer after the number of lines is 'revised'. Otherwise, the specific modifications or reasons are listed. L14: revised. L14-15: revised. L16: revised. L19: the interquartile range. L19: added the reference. L22: revised. L22: Actually, the supporting results are shown in section 4.5 and Figure 4. Yes, this is probably a little bit surprising that Cveg doesn't contribute much to the global uncertainty. This is because the uncertainty

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in soil carbon stock is dominant. L24: Added reference. L29-32: Great suggestions! The regarding part is moved up. L37: revised. L39: Indeed, revised. L41-43: They are added in the discussion. Please see Section 5.2. L49-55: revised. L81: revised. L100: revised. L108: revised. L112: 'PH' is deleted because of not related to the context. L167: revised. L175-180: Yes, it is added in Section 3.1. L214: No, but we propagated the uncertainty of different vegetation datasets into the turnover estimations. L220: revised. L223-234: revised. Now it should be clearer that we used the global full soil depth data which is listed in the data description. L255: Yes, it is added into the Section 3.1. L263: revised. L264: revised. L266: Please see the updated Table 2. L272: We have changed according to the suggestions. Now the sections on the spatial distribution of soil carbon is merged and a new section is added below on vegetation and GPP. L310: Added. Please see the updated Section 4.5 and Figure S5. L312: Revised. L328-329: Yes, as I mentioned in the manuscript, pattern maintains but the contribution is dampened when we used larger soil depth. L331-332: Revised. I found the numbers in the abstract is derived from an old experiment which is now corrected. L349: revised. L369: revised L368-370: Yes, revised according the previous similar suggestions. Now it is in the method. Also I added a paragraph of discussion on Cveg in Section 5.1. L375: revised L384-385: This is actually a part of the method where we extrapolated the soil and the results in shown in the supplement. We put it in the supplement not in the results section because it is too technical for the broad audience. We would like to leave it this way if it is possible. L386: revised. L388: Yes, we are aware of that. L388-389: revised. L396-403: It is a very good question. Unfortunately, we don't know the globally distribution of the metabolically active soil depth. The only thing we can do right now is to fit the vertical soil profile using statistical approach and that is what we did. But better understanding on this matter is truly important, however, beyond our ability to answer. L405-409: Yes, added in Section 5.2. L410: revised. L418-420: noted. L424-425: revised. L427: revised. Reference: revised and added.

Sincerely, Naixin Fan, on behalf of the co-authors.

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Please also note the supplement to this comment:

<https://www.earth-syst-sci-data-discuss.net/essd-2019-235/essd-2019-235-AC1-supplement.pdf>

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2019-235>, 2020.

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