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Earth System Science Data Discussions

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

Anonymous Referee #1

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Review of "Apparent ecosystem carbon turnover time: uncertainties and robust features"

General comments

Fan et al., have estimated the bulk ecosystem level (i.e. combined vegetation and dead organic matter) carbon turnover (or residence) time at global scale. Fan et al., combine multiple remotely sensed estimates of above ground woody biomass with observation orientated estimates of gross primary productivity (GPP) and soil carbon stocks at different depths. Additional information is estimates from these data to estimate the total woody carbon content (though estimating the below ground) and carbon content of herbaceous layer and soil carbon at its maximum depth. The analysis provides and update to a previous study led by co-author Carvalhais from 2014 with the primary

update appearing to be the inclusion of the impact of soil carbon stocks at greater depths than those typically considered in soil inventory or simulation model (i.e. >2 m depth).

The paper provides a novel combination of datasets and a high quality spatially explicit estimate of uncertainty for their estimate which provides valuable information. However, that is not to say the manuscript is without issues.

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 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? 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.

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.

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 the worlds 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).

The methods are thorough and cover each of the input data products and associated methods.

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).

Overall, this paper should be published after some improvements to the manuscript text. The analysis is novel in that it introduces new information to update an existing assessment. The study is also rigorous through the use of multiple datasets and quantification of their collective and partitioned uncertainties on ecosystem turnover time.

The estimate of global turnover and time its uncertainty is also useful, providing constraint on the bulk ecosystem turnover against which other approaches and models can be compared.

Technical comments

Abstract

L14: "...controls..." -> "...is an important determinant of..." Turnover time is not a singular control.

L14-15: "...poorly simulated..." as this paper itself shows there is still plenty of uncertainty in turnover time estimate not just ESMs please rephrase.

L16: "...new, updated ensemble..." Somehow this reads slightly odd to me. I am not sure you should say both new and updated. I think it is clearer to say that you have created a state-of-the-art update to an existing map.

L19: what confidence level are the uncertainties given at? Same for L21.

L19: "...longer than the previous..." at the moment it has not been made clear what the previous is.

L22: remove "merely"

L22: "Cveg (0.05 %)" I find this very surprising and I think others will too. You need to support this somehow, e.g. showing the relative difference in the uncertainty of Cveg estimates vs Csoil. Also, the uncertainty proportions reported leave $\sim$20 % unaccounted for. This should be made clear and some hypotheses as to what might account for this is useful.

L24: "...full depth Csoil..." at the moment it is not clear what this means. As in full depth relative to what? Obviously in the context of the overall paper this is compared to assuming soil depth of 1 or 2 m. Somehow this needs to be made clearer in the abstract.

L29-32: “Our findings show that the. . .” consider moving these statements further up in the results component of the abstract as I think this is the take-home information. So I would make a bigger deal out of it.

Introduction

L37: “Terrestrial ECOSYSTEM carbon turnover time”

L39: “Ecosystem turnover time is an emergent. . .” I would suggest that it is a good idea to quickly reinforce the research object to the reader.

L39: “. . .better. . .” better than what? Should be made clear.

L41:43: Some context on the steady state assumption needed either here or in the discussion.

L49:55: Introduces the importance of ecosystem turnover and its climate sensitivity to the response to climate change. But only soil carbon stocks mentioned. There should be some introduction of each of the main components just mentioned in the previous paragraph, i.e. C update via photosynthesis, Cveg and Csoil. Friend et al., 2014 (cited in text) does cover vegetation simulation in models so you may not even need a new reference, just fill out the text.

L81: “global estimate of ecosystem turnover time” at what spatial resolution?

Datasets

L100: “availability of field data”

L108: “The dataset. . .” not clear which dataset. SoilGrids, S2017 or both?

L112: “PH”-> “pH”

L167: “Ge et al., 2014” not in reference list

L175-180: How many ensemble members in the FLUXCOM experiment? I think it would be good to give information on the ensemble mean uncertainty in absolute and

relative terms. The final statement “. . .we derived the long-term mean. . .” also makes it slightly ambiguous as to whether you also averaged across the ensemble. Given you have quantified the uncertainty I know that is not the case, but I would revise the text here to make that clear.

Methods L207: herbaceous carbon stock estimation, is the uncertainty in GPP propagated here too?

L214: Similarly, to me the manuscript is not clear whether the statistical uncertainty is propagated into the Cveg estimates?

L220: “. . .was used to optimize parameters of the models.” A reference is needed for the approach or the software and package used to do this.

L223-234: I am not clear from this description whether the extrapolation process was estimating the cumulative C stocks down to a predetermined maximum soil depth from a database or whether maximum soil depth emerges from the analysis?

Results

L255: I would clarify to the total number of ensemble members of ecosystem turnover time which has been created.

L263: “. . .and has a SMALLER relative uncertainty THAN. . .” I would be explicit that Cveg uncertainty at global scales is smaller than Csoil

L264: Be clear here and remind the reader that the different GPP products / estimates are all from FLUXCOM.

L266: Table 2. I would like to see the herb fraction or total given here along-side the Cveg.

L272: I suggest you include some typical uncertainty values of Csoil at high and low latitude to give context in the text.

Consider whether Section 4.2 and 4.3 should be merged or re-arranged (and titled) to make what they are actually discussion clear. As it is both “regional” and “spatial” headings suggest similar things.

Sect 4.4. It titled “global carbon stocks” but includes only soil.

There is no similar paragraph presenting the results of the other components of the analysis. There may not be much interesting to say about them but at the moment it looks odd to focus on soil without explanation to the lack of results on other components.

L310: Given the explicit comparison made here to the original 2014 paper. A clear and direct spatial comparison be of the previous map and the current may be useful.

L312: “higher”->”longer” time cannot be higher.

L328-329: “For instance, the uncertainty contribution from Cveg becomes smaller. . .” Does the spatial pattern in relative contribution between Cveg and Csoil persist despite the change in magnitude?

L331-332: “Overall,. . .” seems like the headlined result for the paragraph, should it not have come first with the details coming afterwards. Also these numbers appear to be different from those quoted in the abstract. Could you clarify?

L349: “Figure 6a and 6d)” There is no figure 7.

Discussion

L369: “. . .understanding carbon cycling-climate feedbacks (REF)”

L368-370: I would be clear over how many products you have which are to be made available. As I suggested earlier, that providing the derived datasets could be useful.

Sect. 5.1 titles for “global carbon stock” The entire section deals with Csoil alone. No Cveg.

L375: “. . .non-circumpolar region (Figure X)”

L384-385: This appears to be new information introduced in the discussion. You should introduce all your results in the results section first.

L386: “. . .global carbon cycle yet poorly understood (REF).”

L388: “Two model ensembles. . .” be careful with what you are referring to as an ensemble. There are multiple in the manuscript, the turnover time itself, GPP estimate.

L388-389: “Two model ensembles were selected that can best represent the soil vertical; distribution in circumpolar and non-circumpolar regions RELATIVE TO IN-SITU OBSERVATION”.

L396-403: A comparison with ESMs is good to have here. But As the models only simulated to 1 or 2 m depth. I think it would be fair to compare how the models agree with the soil C stock to that depth too. The question over to what soil depth we should consider needs to be discussed too. For example, at what depth does the soil become metabolically inactive? In high latitudes soil carbon does not turnover once it is frozen so a couple sentences highlighting the importance of the active layer depth would be interesting context. I know this is mentioned in one sentence in the next section but there is no numbers given or reference.

L405-409: Somewhere in here a couple lines to discuss the potential importance of different GPP estimates which are often much higher than those estimates by FLUXCOM would be appropriate. Again, I do not think that this undermines your analysis as FLUXCOM is an observationally orientated estimate but the context that other estimates can provide much larger GPP values. For example, you can highlight how the tendency for larger GPP estimates in ESMs will lead to errors in the turnover time estimation.

L410: “..remains inactive in the process of turnover (REF)” Reference needed and expand.

L418-420: Might be useful to indicate the typical range of soil depths simulated to by the current generation of models in CMIP6.

L424-425: Rephrase

L427: “. . .to quantify ITS CLIMATE sensitivity” be specific to improve clarity.

References

Ge et al., (2018) https://doi.org/10.1111/gcb.14547

Norton et al., (2019) GPP = 167 +/-5 PgC/yr https://doi.org/10.5194/bg-16-3069-2019, 2019

Zhang et al., (2017) GPP = 121.60 to 129.42âĂŁPgâĂŁC/yr https://doi.org/10.1038/sdata.2017.165

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