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Interactive comment on "The effects of carbon turnover time on terrestrial ecosystem carbon storage" *by* Yaner Yan et al.

Anonymous Referee #2

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This manuscript presents ecosystem carbon turnover times calculated globally using MODIS-based GPP and NPP combined with observation-based datasets of plant biomass, litter, and soil carbon. Contemporary turnover times and their potential to change in response to warming and other climatic factors are an important issue in understanding the earth system. However, I think this paper has some issues with its analysis and interpretation that should be addressed.

One major issue is that it isn't clear what the major new advance was in this analysis compared to previous, similar analyses. This analysis seems very similar to that of Carvalhais et al (2014), which is cited several times in the manuscript. In fact, Carvalhais et al was arguably more comprehensive than this analysis because it included direct comparisons to earth system model simulations. I think there were some new features

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in this analysis, such as the inclusion of litter estimates, comparing whole ecosystem vs. soil MTT, and looking at changes over the 20th century, but I think the paper could do a better job of highlighting which things are new and how they changed the results relative to previous, similar studies. If the litter estimates are new, then maybe there could be more discussion of how and why including that pool changed the results relative to previous analyses.

Another issue is the potential for bias in some of the results due to the datasets used for GPP and NPP. While MODIS-derived GPP is constrained by satellite observations, it also depends on assumptions about climatic and environmental factors that affect plant growth and photosynthesis. For example, the efficiency parameter that converts absorbed light into GPP varies with VPD and temperature. MODIS NPP includes maintenance respiration that is calculated based on estimates of plant biomass and a temperature-dependent Q10 relationship. This raises questions about the temperature and moisture relationships shown in Figures 4, 5, and 6, as well as the related estimates of changes in MTT over time. It is difficult to tell how much these relationships are affected by the underlying assumptions of the MODIS NPP algorithm. Since the estimates are not completely measurement based, it is harder to be confident about their meaning.

The estimates of changes in MTT over the 20th century are also problematic because NPP in 1901 was modeled rather than measurement-based. This means that all the changes in NPP from 1901 to 2011 are based on a comparison between average output from several models (1901) to a measurement-based (but partially modeled) estimate (MODIS in 2011). How much of the difference was due to climatic factors that changed over that time period and how much was due to differences between the different sets of NPP estimates? I wonder whether the results in Figure 7b (difference between models in 1901 and MODIS NPP in 2011) would look compared to the change in NPP from the model ensemble between 1901 and 2011. In the end, if models of NPP are being compared, what is the advantage of this MTT approach compared to just analyzing the change in carbon stocks from the actual model output over time?

The analysis depends on a space-for-time substitution (developing temperature and precipitation relationships based on spatial patterns and assuming they also apply to changes over time). What is the potential for bias in this assumption? Processes like acclimation of microbial respiration to warming or shifts in plant species ranges could make changes over time quite different from those that would be expected from observed spatial patterns.

Comparing GPP and NPP as separate and independent metrics doesn't make much sense since both are derived from the same MODIS product. The difference between GPP and NPP is entirely determined by the assumptions of the MODIS NPP algorithm, so I'm not sure I would expect that distinction to provide much useful information in this type of analysis.

In general, I think the Discussion doesn't say enough about why this analysis is useful compared to existing models and previous analyses. The suggestions given for incorporating these results into earth system models and land models are not very useful because most of these factors (e.g., temperature dependence of turnover rates) are already included in all existing models. I do think that there are some useful outcomes from this type of analysis, but I think the Discussion needs some more interpretation of the specific results in the context of ecological factors rather than general statements about how models should take these results into account.

The manuscript also could use some proofreading for English usage.

Specific comments:

Line 56-57: This analysis generally discusses NPP and mean C turnover time as independent, but they could also be related. For example, faster plant growth could accelerate soil C turnover via priming effects, or there could be correlations between plant growth rates and the longevity of vegetation.

Line 62-63: It seems like Carvalhais et al (2014), which this analysis largely follows, did do a pretty good job of quantifying this spatial variation at global scales.

Line 66-68: Another recent radioisotope paper to cite is He et al (2016)

Line 78-82: This suggests that the main contribution of this paper is comparing different versions of MTT calculations. But it's not really clear later on if that is meant to be the focus or not. The paper is also about changes in MTT over time, but doesn't really connect these two parts together.

Line 165-166: "interpret the quantity as an emergent diagnostic at the ecosystem level": What does this emergent diagnostic actually tell us? There isn't any discussion of how it should be interpreted or what kind of bias would occur as a result of the steady state assumption being violated.

Line 180: The equation for MTT looks like it's fitting a ratio of MAT/MAP, but I think this is actually meant to say either MAT or MAP. It's very confusing the way it's currently written.

Line 214-216: If most of the carbon was in soil, then total ecosystem MTT would be largely determined by soil MTT. What are the implications of this when comparing those two estimates?

Line 220: I would expect permafrost soils to have much larger C stocks in places with very deep organic soils. It's not unusual for deep permafrost to have >100 kgC/m2 (Schuur et al., 2015). Could that lead to bias in these results?

Line 224-225: He et al (2016) used radiocarbon analysis to estimate a mean soil C residence time of about 3000 years, which they found to be consistent with several other published estimates. What explains the 2 order of magnitude difference from the estimates here? Turnover time for tundra also seems very short, given that permafrost soils are known to have been steadily accumulating carbon for thousands of years.

Line 256: It doesn't seem like the increase in R2 was really that significant.

Line 261-262: It would be nice to include a map of temperature changes along with MTT and NPP changes so all driving factors could be compared. Also, why was only temperature and not precipitation included in this part of the analysis, even though both looked like they had significant relationships with MTT?

Line 268 and 271: I think these units should be PgC, not PgC/year

Lines 270-275: This might be a good place to discuss whether the whole ecosystem patterns differed from the soil C patterns if there were any interesting patterns there

Line 293-297: I think a lot more could be said about the ecology behind these results. What features of dominant plant species and soil contributed to these differences? Differences in plant lifetime? Tissue lifetime? Susceptibility to decomposition?

Line 299: Since the ratio of GPP to NPP is entirely determined by the assumptions of the MODIS NPP algorithm, I don't think this result has a lot of real-world meaning.

Line 377-379: Why would this reduce the uncertainties?

Line 381-382: Doesn't aggregating everything to the biome level violate the assumptions behind calculating change in MTT over time? This would suggest that MTT could only change if the spatial extent of different biomes was shifting.

Line 390-391: This would be a good place to discuss alternative soil databases like Hengl et al (2014) - available at soilgrids.org

Line 392-393: This is arguably the primary purpose of all land surface models. They all already consider this.

Line 397-398: All land surface models already include temperature functions that affect pool turnover times.

Line 401-404: Land surface models already include these processes. In general, this whole section about improvements to land models isn't supported by any comparison between this study and actual land model output. Carvalhais et al (2014) did explicitly compare their MTT results to earth system model simulations, and I don't think it makes sense to discuss these model-related suggestions without doing a similar comparison here.

Line 421-422: Data availability would require putting all the MTT data somewhere that readers can access it.

Figure 1: The colors need to be rescaled, especially for soil C. It's really hard to see anything in that map. Also, the soil C has some obvious artifacts, like the sharp change in soil C on the border between Alaska and Canada. What does this mean for the results? It would also be nice to have a map of NPP here so all the drivers could be seen together.

Figure 2: Since all three of these look about the same, I don't really see the point in including all of them as separate metrics

Figure 4: Panel a: The regression looks like it underestimates the slope of the curve by a lot. Panel d: The exponential fit does not do very well at the high precipitation end. What does this mean for the results?

Figure 7: The titles on the figure say from 1991 to 2011, but the text says it goes from 1901 to 2011.

References:

Hengl T, de Jesus JM, MacMillan RA, Batjes NH, Heuvelink GBM, Ribeiro E, et al. (2014) SoilGrids1km âĂŤ Global Soil Information Based on Automated Mapping. PLoS ONE 9(8): e105992. https://doi.org/10.1371/journal.pone.0105992

He, Y., S. E. Trumbore, M. S. Torn, J. W. Harden, L. J. S. Vaughn, S. D. Allison, and J. T. Randerson (2016), Radiocarbon constraints imply reduced carbon uptake by soils during the 21st century, Science, 353(6306), 1419–1424, doi:10.1126/science.aad4273.

Schuur, E. A. G. et al. (2015), Climate change and the permafrost carbon feedback,

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