

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

Anonymous Referee #1

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General comments

This manuscript describes the calculation of mean carbon turnover time, for both ecosystem and soil pools, based on both GPP and NPP. The introduction does a good job of laying out why this is interesting, given uncertainties surrounding terrestrial ecosystem and climate change, disturbance, etc.

There are however many problems here. First, what exactly is the advance of this study over Carvalhais et al. (2014)? This wasn't clear to me.

Second, this analysis mixes (I believe) spatial and temporal trends, assuming that they're equivalent, but this assumption is never explored or even really discussed.

The steady-state assumption is also troubling. I understand why it may be necessary

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at a global scale, but the authors should at least estimate how much bias this might be introducing. For example, there are gridded disturbance and forest age maps available that could be incorporated into such a calculation.

The lack of any clear data availability statement is unacceptable. It's 2017, and I expect all code and data to be included as supplementary info, or (better) posted in a repository. It's not acceptable to produce results from a black box.

The figures should be improved. See comments below.

Finally, while I appreciate the difficulties of writing in a foreign language, the current manuscript is riddled with spelling and grammar mistakes. This is doubly frustrating as I know that the senior author, at least, is fluent in English.

Specific comments

1. Line 24: Why “Thus”? Doesn't seem to be logically connected
2. L. 28: “difference”
3. L. 47: “validated” probably not the best word to use here
4. L. 52: “amount of”
5. L. 62-63: Carvalhais et al. (2014) seems like a needed citation here
6. L. 86-87: unclear
7. L. 90: this language is used frequently in the ms. Is ecosystem C storage really “driven” by MRT? I would say that MTT is an emergent property of changes in fluxes; it can't “drive” anything
8. L. 116: by the definition above (pool/flux), it *definitely* would change

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9. L. 142: cite R correctly ("citation()"), including version numbers of all packages used
10. L. 166: at the biome level, do you mean?
11. L. 196-201: first, need to note that you're assuming that the current-day *spatial* correlation between temperature and MTT is identical to the *temporal* correlation between these variables. It's not at all obvious this would be true. Second, you're mixing models and remote sensing products; it would be good to document how much divergence these models have from MOD17 in 2011.
12. L. 211-221: are these really results? Aren't these just the GLC database numbers?
13. L. 225-: be consistent in using long/short or high/low or large/small in referring to MTT
14. L. 245: Q10 is 1.95 implies that MTT roughly doubles with a 10 °C increase? That seems nonsensical
15. L. 298-299: can you explain this more?
16. L. 338-340: see comment 7 above re language and causality
17. L. 365-366: is it possible to quantify, even in a back-of-the-envelope kind of way, how much error might be introduced by this assumption? That would be interesting
18. L. 389: but you're not measuring temporal variability (much), except for changes over time in the MOD17 product, right?
19. L. 406-419: this is all duplicative and can be removed

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20. L. 421-422: completely inadequate data availability statement. Elevation data?!?
21. Figures generally: maps are pretty but have limited utility. At least of these might be more informative if gives as e.g. Latitude versus MTT plots
22. Figure 6: not at all useful in my opinion

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