**Review on**

**X. Lu, Y.-P. Wang, Y. Luo, and L. Jiang**

**"Ecosystem carbon transit versus turnover times in response to climate warming and rising atmospheric $CO_2$ concentration"**

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## General comments

I very much appreciate that the authors took intense action to respond to all of my remarks and questions. Overall I see an improvement of the paper, although I still think that the separation of transit time into the components MAC and ACC (see below) still found in the revised paper is only of academic interest so that I do not see why to publish this. Nevertheless, its technically correct so that upon publishing one could leave it to the scientific community to follow my opinion or not. Another thing concerns the appraisal of 'transit time' over 'transient time' also in the revised paper. I can understand that the authors burn for their subject, but in my opinion they overshoot, so that I suggest that they revise the respective parts of the paper (see my more detailed comments below). A final thing is the multitude of language errors also present in the revised paper.

## Detailed comments

In my review I had three major remarks. In the following I comment separately on the author's answers to them.

**Remark 1) from my review: The study is not well motivated**
From the response of the authors and also the remarks from reviewer #2, I understand now that by referring in the introduction to the papers of Friend et al. (2014) and He et al. (2016) the authors want to point out that much of the uncertainty for a realistic simulation of the land carbon cycle arises from the fact that the internal time scales are not well known. Unfortunately, they use here the term 'turnover time' that has a very specific meaning in their paper instead of e.g. a more neutral term like 'time scales', 'memory' or so. Thereby the introduction can be misunderstood as if the authors would like to justify their research by claiming that in the literature (particularly in those two papers) turnover time is used instead of – in the authors opinion more appropriate – transit time. This invited for the critique I expressed in my review to use those two papers for a justification of their study. So I suggest the authors revise their introduction to prevent such a misunderstanding. Thereby it will hopefully also get clearer that the main argument for their study may be found when following the hint from the end of the first paragraph in the introduction, namely that under transient conditions transit

time is different from turnover time – and pointing this out this may result in a proper justification for their study when it is added that only transit time has under these transient conditions a proper inner-theoretical meaning in the context of compartemental models.

In this connection I want to point out that I do not see any reason to qualify the use of turnover time against transit time – both have their advantages and disadvantages, as the authors well know (see their answer to my review). Therefore I find it inappropriate to call the difference of turnover time to transit time a 'bias' – it is simply a difference. And I also cannot follow the authors claim that when using transit time instead of turnover time one would solve the problem of the not well known internal time scales of the carbon cycle as e.g. expressed in the sentence (lines 376/377): "Estimating C transit times in the real world can help constrain projections in land C sequestration by C cycle models because C turnover time is a major source of model uncertainty (Friend et al., 2014; He et al., 2016)." Here the authors play with false cards, since they leave the impression that the use of turnover time instead of transient time is the problem that makes our knowledge of the carbon cycle so uncertain. But it is no question that Friend et al. (2014) and He et al. (2016) would have come to the same conclusions independently of using transit or turnover time – the problem is our incomplete knowledge of the internal time scales of the carbon cycle, not the use of transient time instead of turnover time. Moreover, land C cycle models could be well constrained with a good knowledge of either transient time or turnover time so that in this respect none of them has an advantage. The only advantage of transient time is that it has a proper inner-theoretical meaning even for transient states, but I do not see how this could lead to a practical advantage, except maybe in connection with the processing of labeled carbon ($^{13}C$, $^{14}C$) in vegetation. Therefore I suggest that the authors rethink their advocation of transient time thoughout the paper and revise the respective parts.

In addition to this, with the new second sentence in the abstract "However, we know little about whether transit time or turnover time better represents carbon cycling through multiple compartments under non steady state." the authors let the reader expect that the paper would answer the question which of the two times would 'better' represent the carbon cycle under these transient conditions – but this is not a question answered in the paper nor do I expect that it could be answered.

**Remark 2) from my review: The relevance of the results of the study is unclear**
I very much appreciate that in the resubmited paper it is now much clearer that a major result of this study is the matching of turnover and transit time during the historical period and their divergence in the future – and this should also clearly be expressed in the abstract. But concerning the relevance of the separation of transit time into MAC (mean age change) and ACC (age composition change) I have the feeling that we live on different planets so that I don't think that we could come to an agreement. None of the author's comments to my claim that this separation is useless convinces me of the opposite. In particular I still do not see where this separation could lead to an improved understanding. Nevertheless, in answering to this remark, the authors now indicate where

this separation may be useful (this is not 'understanding' but already something). In the revised paper they now write in lines 409-411 that by constraining "transit time through its two components [from] observations, modeled C cycle and land C sequestration can be significantly improved". I completely agree that if one could separate MAC and ACC one would have another diagnostic for comparison with models. But I very much doubt that ever these components could be measured because in view of the continuum of time scales in the land carbon cycle the discrete pools of models have no proper counterpart in our environment. It would be great if the authors could explain in their paper how to measure the two components in order to justify their claim that their method has the potential for 'significant' improvement.

**Remark 3) from my review: Some suggestions for improving the paper**
The authors have not taken up my suggestion to drop all material related to the separation of transit time into MAC and ACC, and I don't expect any agreement on this point (see previous comment). But I appreciate that most of my other suggestions have been taken up.

## Minor remarks

- For completeness it would be good to explain that in the formulas (1) to (5) the number of compartments $d$ depends on the application: In the calculation for a single grid cell it is the number of pools from all vegetation types in that grid cell, while for global numbers it is the total number of all carbon pools in all grid cells worldwide – this would make clear that e.g. the transit time shown in Fig. 3a is not a global average of Fig. 1a (for the particular years).
- Line 81: What are 'contributing fractions'?
- Line 174: What is "age-mass C"?
- After now dropping the two terms 'Olson method' and 'Rasmussen method' in the revised paper, the authors may want to use different indices than 'o' and 'r' to distinguish turnover and transit time.
- The caption of Fig. 2 has been expanded, explaining now that for the current dacade land carbon uptake is much larger than for previous decades – I don't understand the reason for this explanation. And why is it your 'assumption' that today's C cycle is close to equilibrium – all your paper is about the transient state.
- I find the caption of Fig. 6 hard to follow. Maybe one could help the reader by adding names to the rows and columns of the figure by e.g naming the first row 'transit time', the second row 'turnover time', the third row 'comparison transit/turnover time', and first column 'steady state', second column '$\Delta\tau/\tau_{1900s}$', and third column '$\tau_{2090s}-\tau_{1900s}$'.