Review on

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

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It is well known that carbon turnover time as computed by carbon stock divided by carbon flux has a well defined meaning only for stationary states, while the meaning of transit time as mean age of carbon released from the system remains valid also for non-stationary states. On this background the submitted paper investigates how strongly transit and turnover times deviate in historical and RCP8.5 scenario simulations. The simulations are performed using the land surface model CABLE in offline simulations forced by CRU-NCEP (historical) and CLM (scenario) data. To separate the physical and biogeochemical effect of CO_2 on transit and turnover times, separate simulations are performed where either the temperature forcing or the photosynthethically relevant CO_2 level are kept fixed. To determine transit time using the approach by Rasmussen et al. (2016), the authors equipped CABLE with a diagnostic to follow changes in carbon stocks of all pools of their land carbon model and the fluxes between them. The authors show that the expression for changes in transit time based on this approach can be separated into two components, one arising from changes in the mean age of the carbon in the different pools (abbreviated in the following as MAC – Mean Age Change), the other arising from changes in the age composition of the carbon fluxes 'respired' from the different carbon pools (abbreviated in the following as ACC – Age Composition Change). The authors show how the MAC and ACC contributions to transit time change in their scenario simulations.

Major Remarks

1) The study is not well motivated

In the abstract the authors motivate their study by writing that considering transit and turnover times "neither of them has been carefully examined under transient C dynamics in response to climate change". This is not a very convincing argument for their study since (i) the study should not be published if its contents would not be new, and (ii) that something hasn't been done doesn't qualify it as scientifically relevant. And also the introduction is not clear about the motivation of the study, except that from the subtext the authors let arise the impression that results from other studies using turnover time for a non-steady state situation cannot be trusted. Here e.g. a study by He et al. (2016) is cited with the result that in CMIP5 simulations the "soil C sequestration potential can be overestimated due to underestimation of C turnover time". But in this study "turnover times" are decay parameters of a model and not a diagnostic turnover time computed by carbon stock divided by carbon flux so that this study is not suitable for motivating the study reviewed here. A similar remark concerns the study of Friend et al. (2013) cited in the introduction: Friend et al. indeed calculate turnover time as carbon stock divided by carbon flux but as a diagnostic to test the validity of their simple carbon model, but not for drawing any other conclusions from it that could be improved by using transit time. Hence, in my opinion the authors handle the cited literature inappropriately to motivate their study and thereby give a wrong impression of their relevance.

2) The relevance of the results of the study is unclear

According to section 5 ('Conclusions') the study has two major results. The first (lines 357-361) concerns the development of transit and turnover time in their simulations, in particular that they increasingly deviate from one another during the 21st century, and that the deviations are stronger in some regions than in others. But what to conclude from this? Is this a useful knowledge?

The second result (lines 363-368) is that transit time can be separated into contributions from MAC and ACC because the residual is small (see eq. (6) and Fig. 3). Therefore on first sight I indeed thought that this separation is an interesting idea that could help to understand better how transit time behaves under different forcings. The authors claim (lines 364/365) that MAC is determined by carbon input and ACC by "differential responses of various C pools to climate warming and rising atmospheric [CO_2]". While this latter formulation is rather cryptic, I take from it that the authors think that one could pin down what affects MAC and ACC so that e.g. for scenarios of different CO_2 and/or temperature rise one could understand why transit time would develop differently. But I very much doubt that this separation helps understanding anything since there is no way to see how MAC and ACC are separately affected by the carbon inputs or the forcings: this is because MAC and ACC are not independent from one another since they are both derived from the same development of carbon stored in the compartments ($x_i(t)$ in eq. (2) and eq. line 166). Hence a change of the carbon input into the system changes both MAC and ACC, and also a change in pool turnover time parameters by a changing climate (temperature, moisture) changes both MAC and ACC. Only if one could understand how climate and CO_2 act differently on MAC and ACC this separation could contribute to a better understanding of transit time development in transient simulations. That MAC and ACC have no individual meaning can also be seen directly in the simulation results: In the simulation with both forcings combined (simulation S3) the contribution to transit time from ACC is not even approximately the sum of the ACC contributions from the simulations with forcings separated (S1, S2), and the same is true for MAC. Hence, the behaviour of simulation S3 cannot be understood as combination of results from S1 and S2. – In conclusion, I think this separation is only technical and pretty useless. In order to convince me from the opposite, the authors had to show me a case where it leads to an improved unterstanding.

Concernig the other remarks related to the second result in lines 366-368, I think they are all wrong: (i) The calculation of turnover time by dividing stocks by fluxes is not assuming anything, it is simply a diagnostic that in the case of stationary states has a well defined meaning, but can, as a diagnostic, still be a useful concept (see my remark on the study by Friend et al. (2013) above). (ii) Surely turnover time changes when MAC and ACC change, so that contrary to the authors claim it accounts for such changes. Hence (iii) contrary to the claim by the authors one cannot conclude that transit time is a “better parameter”. – A similar claim is found in the last scentence of the abstract where the conclusion is even weirder by saying that the use of turnover time instead of transient time may “lead to biases in estimating land C sequestration” – how could the mere calculation of a time scale affect the estimation of C sequestration?

3) Some suggestions for improving the paper
There is one result of the paper – surprisingly not mentioned in the conclusions – that in my opinion makes an important contribution to land carbon research: This is the comparison of the CABLE results for transit time with the observational results by Carvalhais et al. (2014) in Fig. 2. When the Carvalhais et al. paper appeared, I thought it’s nice that they produced a map of stocks divided by fluxes so that this turnover time can be used as a diagnostic to easily compare with results from model simulations. But with the study under review here, we now know that despite the non-stationarity of todays carbon cycle, turnover times agree well with transit times (Fig. (5)) so that the observational turnover times of Carvalhais et al. (2014) can indeed be interpreted as proper carbon ages. And that the zonal distribution of CABLE results matches those of Carvalhais et al. quite well provides additional credit to this conclusion.
Hence, what I propose is that you focus your study on the question to what extend the observational estimates of turnover time by Carvalhais et al. (2014) (and if possible also those by Bloom et al. (2016) Fig. 3) can be interpreted as proper ages. In this respect it also interesting to see that shortly in the future this doesn’t work any more. For your paper this would mean that you drastically shorten it by dropping anything else (i.e. in particular the simulations S1, S2 and all stuff relating to the separation of transit time into contributions from ACC and MAC). With such changes I think a resubmission could make sense.

Minor Remarks

- For a resubmission a better polished text would be appreciated. The current text is mostly understandable but the English shows quite some deficits (an annoyingly plentitude of missing articles; wrong grammar (lines 41, 47, 127, 154, 156, 175, 177, 178, 261, 357, 479; Supplement scattered with errors); incomplete formulations (lines 56, 139, 167, 210); wrong or missing preposition (lines 99, 333), ununderstandable formulations (line 168, 196, 274)). And results should be presented in present tense not in past tense as e.g. in the abstract.
- I do not see why in addition to the terms ’transit time’ and ’turnover time’ one needs the equivalent use of namings ’Olson method’ and ’Rasmussen method’, respectively, this is only confusing – by whatever method you compute turnover or transit time,

they remain the same.

- In the model description for CABLE you refer for the photosynthesis part to a paper by Farquhar that refers to the C3 pathway only. What does it mean for the realism of your simulations that CABLE is not accounting for C4 photosynthesis, happening at huge areas wordwide?
- What about land use change? This seems to be not accounted for in CABLE, but replacement of forests with agricultural lands could in principle speed up the land carbon cycle by one magnitude (maybe forests: 30 years vs. agriculture 1 year).
- What about natural vegetation? How does it change in your CABLE simulations?
- For the historical period CABLE was forced by CRU-NCEP data, while for 2006-2100 simulation data from CESM were used. How good do these simulated climate data fit at the transition period around 2005/2006 to the historical values, concerning e.g. the global and zonal levels of land temperature, precipitation, and radiation?
- When you introduce the Rasmussen method to calculate transit time it would be good to mention that this approach works only for linear box models. Is CABLE really of this type? You could demonstrate this by listing in the appendix the box-model equations for CABLE (like in section 8 of the Rasmussen et al. paper) – this would also help to make precise what at all your mathematical symbols mean. – I wonder about the applicability of the Rasmussen et al. approach because I would think that e.g. the phenology introduces some non-linearity in the dynamics of leaf carbon since leaf area cannot grow beyond a certain value depending on vegetation type – but maybe CABLE works differently. And what about structural allometries between different plant parts (stems, roots, leaves) that are also non-linear?
- Fig. 1: (i) Title Fig. 1a: Transient→ Transit. (ii) Make the scale numbering for both plots of Fig. 1 better readable (e.g. in steps of 5 or 10 years, or, if logarithmic, use other round numbers, but definitely not something like 3623). (iii) Are the colors at the edges of e.g. Antarctica and Greenland really a result of your simulations, or is it a plotting artefact e.g. from your grid cell interpolations?
- Lines 147-150: I guess that this paragraph should say that the authors solve equation (2) by an Euler method starting from zero land carbon – this should be stated more clearly.
- Fig. 2: Why don't you also plot turnover time from CABLE? This would make it even more clear that transient and turnover time match well for this period of time.
- Why do you talk of "permafrost areas" instead of e.g. "high latitudes"? I guess that CABLE is not accounting for permafrost.
- Fig. 6g: You attribute the small difference in turnover and transit time for the stationary state to the presence of the seasonal cycle that makes the system non-stationary. Can this explain the increase of this difference beyond 60°N?

Literature

A.A. Bloom et al. *The decadal state of the terrestrial carbon cycle: global retrievals of terrestrial carbon allocation, pools, and residence times*, PNAS 113 (2016) 12851290.