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Interactive comment on "Ecosystem carbon transit versus turnover times in response to climate warming and rising atmospheric CO_2 concentration" *by* Xingjie Lu et al.

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Dear Referee 1:

We greatly appreciate your time and effort to read, understand, and make comments on our manuscript. We have carefully studied your comments. Hope our responses have adequately addressed your concerns.

Xingjie Lu and Yiqi Luo

On behalf of all co-authors

Anonymous Referee #1

Reviewer 1: 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 nonstationary 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 CO2 on transit and turnover times, separate simulations are performed where either the temperature forcing or the photosynthethically relevant CO2 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.

Response: We greatly appreciate the reviewer for carefully reading our manuscript. The above paragraph is a good summary of what we did in our study.

Major Remarks

Reviewer 1: 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.

Response: We thank the reviewer for the great point. First, we totally agree that "something hasn't been done does not mean it is scientifically relevant". The reviewer is a critical thinker. He or she may agree that any sentence out of a context may not make sense in a manuscript. A sentence, however, carries meanings in connection with other sentences. For our manuscript, the first sentence of the abstract is "Ecosystem carbon (C) transit time is a critical diagnostic parameter to characterize land C sequestration." Combining the first sentence with the third sentence in the abstract can form a sentence "Such a critical parameter 'has not been carefully examined under transient C dynamics in response to climate change."' This new sentence by placing "not been carefully examined" in the context, we think, identifies an important knowledge gap and thus makes our study scientifically relevant. Nevertheless, we take the criticism seriously from the reviewer and changes the sentence to be "However, we know little about whether transit time or turnover time is a better diagnostic parameter to represent carbon cycling through multiple pools under non steady state."

We also agree with reviewer's statement "the study should not be published if its contents would not be new". Since the reviewer did not elaborate this point in reference to our manuscript, we consider her or his comment is a general statement.

Reviewer 1: 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 under estimation 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.

Response: We greatly appreciate the reviewer's effort to identify the motivation of our

study in the introduction section. The last sentence in the first paragraph of the introduction section states: "It is not clear how much estimates of C turnover time deviate from mean C transit time and what cause their deviation under climate change." If this is not clear to the reviewer, we were wondering if the reviewer has any more specific suggestion to revise our sentence so as to make our motivation of the study apparent to her/him.

The reviewer is very knowledgeable and knows the technical detail very well in the study by He et al. (2016). The reviewer is right that in He et al. (2016) study, "turnover times" were derived from decay parameters of a model and not by carbon stock divided by carbon flux. Our sentence "Up to 40% of soil C sequestration potential can be overestimated due to underestimation of C turnover time in current CMIP5 models (He et al., 2016)" reflects a main conclusion from that study. Besides, He et al. (2016) used the term "turnover time" in their paper. As Sierra et al. (2017) pointed out, there are many different ways to define "turnover time" in the literature. We used the study by He et al. (2016) as one example to highlight the need to understand turnover time better instead of to define the term of turnover time. We were puzzled why the reviewer thought the paper by He et al. (2016) "is not suitable for motivating" our study.

Reviewer 1: 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.

Response: The reviewer used another sentence in the manuscript to question the motivation of our study. The sentence "Recent model inter-comparison study indicated that a major cause of uncertainty in predicting future terrestrial C sequestration is the variation in C turnover time among the models (Friend et al., 2014)" was used to highlight the need to understand turnover time better as well. We were very confused by reviewer's point that because Friend et al. (2014) did not mention transit time in their paper, we could not use it to motive our study on turnover time in relation with transit time.

Overall, we are grateful to the reviewer for his/her critiques, which made us to carefully examine our manuscript again.

Reviewer 1: 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?

Response: We were not sure if the reviewer asked those two questions philosophically or practically. In practice, knowledge about the deviation between C transit time and turnover time in different regions under different scenarios (warming and [CO2] rising) is useful for us to understand time characteristic of the ecosystem carbon dynamics. When we lump all pools and fluxes together to calculate turnover time by "stock over flux", the time characteristic is different from that of transit time when individual pools and fluxes are considered to be networked together to form compartmental dynamical system. Thus, our results provides information on how turnover time deviates from transit time in specific region and ecosystems. Because "stock over flux" is still the easiest way to measure how fast C cycle through ecosystem, it is not our purpose to persuade the community to completely give up the method. Instead, we only provide information about how carbon transit time deviates with the turnover time in the non-steady state. We estimated both C transit time and turnover time globally. We assume the deviation between them is due to the lost information by lumping pools and fluxes to calculate the turnover time. The temporal and spatial estimates on the deviation tell us whether we can still use turnover time at a specific place and time.

If those are philosophical questions, we may not be able to answer either of the questions correctly. In this case, it is only reviewer's perspective that matters.

Reviewer 1: 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 [CO2]". 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 CO2 and/or temperature rise one could understand why transit time would develop differently.

Response: We thank the reviewer for her/his carefully examining the separation of Mean Age Change (MAC) from Age Composition Change (ACC) in our study. We agree that the contributions by MAC and ACC are not independent with each other, similarly as pool and flux influence each other. But the separation of pool and flux is so fundamental for any research to understand carbon cycle. In this study MAC is related to the change caused by carbon age, which is equivalent to pool change in the pool-flux separation whereas ACC is more about the change caused by flux.

We understand that it may take time to fully comprehend new concepts about MAC and ACC. The separation was made according to derivative of an equation with two components. This method is very commonly used in many studies. For example, Koven et al. (2015) used this method to separate relative contributions of NPP vs. turnover time in influencing carbon sequestration. By separating the two terms, we can better understand mechanisms underlying the changes in transit time.

Reviewer 1: 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 (xi(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.

Response: The reviewer "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". We do not agree that contributions from different reasons cannot be separated only because they are both partly affected by the same factor. There are many opposite examples in C cycle study. For examples, C pool and flux may both respond to change in C input. However, their responses have different ecological meanings.

Moreover, eq. (6) is the major equation to show the differences between MAC and ACC. MAC is mainly contributed by the change in age and ACC is mainly contributed by the change in composition.

Reviewer 1: Only if one could understand how climate and CO2 act differently on MAC and ACC this separation could contribute to a better understanding of transit time development in transient simulations.

Response: Reviewer was wondering "how climate and CO2 act differently on MAC and ACC". Our results have shown different responses of MAC and ACC to climate and CO2. Figure 3d shows that contribution from ACC (in blue) changes from negative to positive in response to C balance change, whereas MAC (in red) keep increasing. These features are very similar to C pool and flux that C flux response faster than C pool to input change. More evidence that contributions from MAC and ACC vary spatially (Figure 4) also indicates the separation is helpful for our understanding.

Reviewer 1: 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.

Response: The reviewer also doubted that "MAC and ACC have no individual meaning" because the sum of individual effects (warming effect and [CO2] rising effect) does not equal to the combined effects. The non-additive MAC or ACC in response to warming and [CO2] rising is possibly due to the non-linear or interactive effects, which have commonly been found in other studies. For examples, climate and rising atmospheric [CO2] affect GPP together, but usually their co-effects are not equal to the sum of their individual effects (Zhang et al., 2016). However, it does not necessarily mean climate effects and rising atmospheric [CO2] effects on GPP should not be separated.

Meanwhile, MAC and ACC do have individual meaning. In theory, Eqn (6) has illustrated that MAC represents the contribution from change in age and ACC represents the contribution from change in composition. In practice, results from last response (Figure 3d and Figure 4) have also confirmed they are completely different.

Reviewer 1: – 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.

Response: In practice, the contributions of MAC and ACC should be different among models. None of previous studies have diagnosed those two contributions separately. Although total change in C transit time has been compared recently (Sierra, 2017), a thorough assessment on their individual contributions would provide more useful information. Otherwise, there is still a chance for models to get the right answer with wrong reasons. By combining compartment models with different types of measurements via data assimilation techniques, we may be able to better constrain MAC and ACC respectively. Therefore, modelled C cycle can be better calibrated by constraining MAC and ACC against measurements in the future.

Reviewer 1: 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).

Response: We appreciate that this reviewer clearly shows her/his view and perspective. He or she stand strongly for using turnover time. We agree that turnover time is a good and useful diagnostic, since both stock and fluxes are easy to measure. Our manuscript have admitted the advantages of C turnover time "C turnover time can be easily calculated from C stock over flux, both of which can be easily measured." (Line 342-343) However, would his/her statement "the calculation of turnover time is not assuming anything" be contradictory to "it is simply a diagnostic that in the case of stationary states has a well defined meaning"? Would "in the case of stationary states" be an assumption? But we hope the reviewer agrees that turnover time is calculated by lumping pools and fluxes together. As a scientist, he or she, we hope, will not be against research to explore other ideas related to time characteristics of carbon cycle.

Reviewer 1: (ii) Surely turnover time changes when MAC and ACC change, so that contrary to the authors claim it accounts for such changes.

Response: We mentioned "C turnover time does not account for changes in age structure and contribution fractions of different pools to ecosystem respiration." (Line 366-367). This sentence did not say "turnover time does not change". Obviously, C turnover changes when MAC and ACC changes. However, we care about whether the diagnostic really accounts for the certain critical information. The "change" in C turnover time with MAC and ACC is not really sufficient enough to accurately quantify the contributions from MAC and ACC to C transit time.

Reviewer 1: 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?

Response: The reviewer seems to believe that there is no better diagnostic than turnover time. We partly agree but it should depend on the specific case we are studying. Eg. Friend et al., (2014) identified that the source of the uncertainty in predicating land C sequestration C is mainly from turnover time. Qualitatively, turnover time and transit time behave similarly. Thus, the turnover time can be used in this case to point out a direction for model improvement. However, turnover time may not represent the time characteristic of carbon dynamics if we are interested in carbon sequestration in multiple pools. It is because turnover time uses lumped pools and fluxes for calculation.

Reviewer 1: 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.

Response: Thanks for the great suggestion to improve our manuscript. We will include this point in the discussions to support Carvalhais et al. (2014).

Reviewer 1: 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.

Response: Although this reviewer clearly has her/his own preference, these are very constructive suggestions. We fully understand the reviewer's point and do see some interesting conclusions being drawn in this way. It is very interesting that the reviewer states "In this respect it also interesting to see that shortly in the future this doesn't work any more." We thought we just did what the reviewer suggested us to do. Our analysis indicates that turnover time works quite well now and until the middle of this century before it significantly deviates from transit time. However, to do what the reviewer suggested us to do, we need the full length of the manuscript. Anyway, we will certainly revise the manuscript to address reviewer's comments.

Minor Remarks

Reviewer 1: 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.

Response: We will carefully check grammar, formulations, etc. and will use present tense. We will have a native English speaker help polish the whole text.

Reviewer 1: 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.

Response: Thanks for pointing out the confusion. In the revision, we will use only transit time and turnover time avoid the confusion.

Reviewer 1: 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?

Response: Sorry for the misleading, but CABLE does have C4 pathway photosynthesis as well, which follows the method by Kowalczyk et al. (2006). In the revision, we will add this information to clarify.

Reviewer 1: 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).

Response: Land use change is not accounted for in CABLE and we have discussed the possible bias caused by this in the discussion section 4.3.

Reviewer 1: What about natural vegetation? How does it change in your CABLE simulations?

Response: CABLE is not a dynamics vegetation model, which means natural vegetation distribution is static. In the revision, we will add some discussions on how changes in natural vegetation distribution in response to climate change will influence the estimates of transit time.

Reviewer 1: 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?

Response: We have realized this and adjusted the CESM future simulation results to make the connection between historical climate and RCP8.5 smooth and also to make the trends and seasonal and daily variability in the climate data simulated by CESM follow their patterns in the historical data in CRU-NCEP. For example, Figure R1 shows how global mean annual temperature changes before and after being adjusted. In the revision, we will add these figures in the appendix.

Fig. R1 Global mean annual temperature changes from 1901 to 2100. Historical data were interpolated from 6-hourly (Qian et al., 2006) to hourly and re-gridded from a spatial resolution of 0.5o by 0.5o to 1.875o by 2.5o. From 2006 to 2100, the hourly meteorological variables were generated by Community Earth System Model version 1.0 (CESM) (Li et al., 2016; Hurrell et al., 2013) for Representative Concentration Pathway (RCP) 8.5. The red line from 2006-2100 represents original model results and the blue line represents the global mean annual temperature being adjusted to make the connection between historical climate and RCP8.5 smooth and also to fit the trends and seasonal and daily variability in the historical data.

Reviewer 1: 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?

Response: These questions and suggestions are very helpful to improve the manuscript. We will give more details about CABLE in appendix. Yes, C cycle in CABLE, even with phenology processes, can be considered as a linear model. In the deciduous plant functional type, CABLE's phenology only changes the leaf turnover rate and allocation fraction in spring and fall. When LAI grows over the upper limit, CABLE will set the allocation fraction to leaf to "0" in order to prevent further growth of leaf. The dynamics of both turnover rate and allocation fraction are determined by time-dependent environmental scalars. Because the environmental scalars are independent from C pool sizes in most cases, the model can be considered as a linear model. In addition, CABLE does not include the structural allometries. The C pool size only depends on the input (net primary production allocated) and output (turnover) and in most of time, the allocation fraction is independent from C pool sizes.

In the revision, we will add all these details in the appendix.

Reviewer 1: 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).

Response: We will make revisions as suggested.

Reviewer 1: (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?

Response: Those colors at the edges are the real results of the simulations. The red indicates that C transit time and mean age are really high in the high latitude region. In contrast, the edges of islands at lower latitudes, e.g., Hawaii, are relative low.

Reviewer 1: 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.

Response: Yes, we will follow the suggestion to describe more clearly.

Reviewer 1: 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.

Response: Thanks for the great suggestion. We will add turnover time in Figure 2.

Reviewer 1: Why do you talk of "permafrost areas" instead of e.g. "high latitudes"? I guess that CABLE is not accounting for permafrost.

Response: We agree that "high latitudes" is more accurate. We will change the wording.

Reviewer 1: 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 60âŮęN?

Response: Yes, conceptually, the significant bias in high latitude should be due to the seasonal cycle. We will illustrate it in the results.

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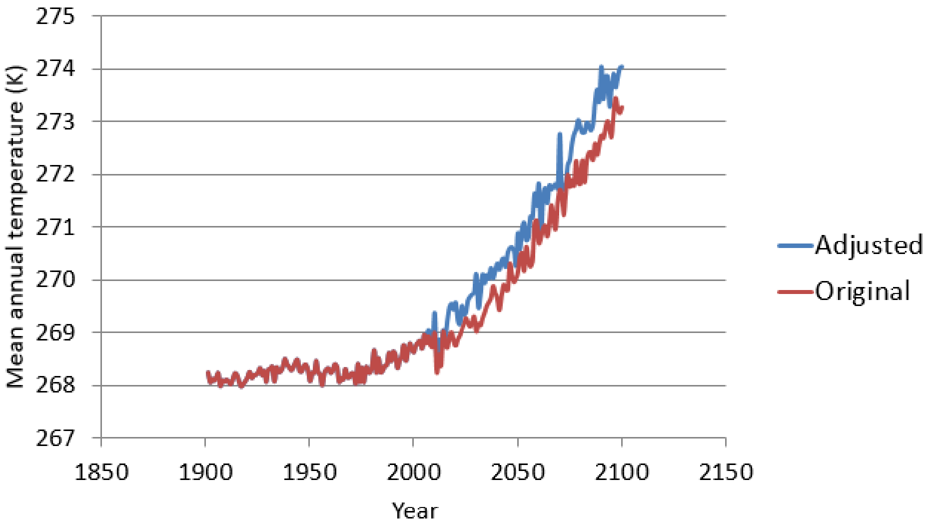

Fig. 1. Fig. R1 Global mean annual temperature changes from 1901 to 2100. Historical data were interpolated from 6-hourly (Qian et al., 2006) to hourly and re-gridded from a spatial resolution of 0.5o by 0.5