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Interactive comment on "Ages and transit times as important diagnostics of model performance for predicting carbon dynamics in terrestrial vegetation models" *by* Verónika Ceballos-Núñez et al.

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We appreciate the time that Referee 3 dedicated to review our manuscript. In the text below we quote the referee's comments in italics and provide our response below in blue:

The manuscript by Ceballos-Núñez et al. describes a study on transit times of C through a temperate forest, with the help of a framework with a number of pools and transfer time coefficients between these pools. The number of non-structural pools is

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varied to investigate the importance of these for representing the transfer. Whereas the concept used in the manuscript is generally well explained, more information should be provided on the details. I have some reservations to the chosen methods, but this may also be partly caused by misunderstanding due to the too concise description. Some parts of the study need to be clarified, and slight alterations to the methodology may be required. This manuscript can become an interesting contribution to the literature on modelling carbon allocation in (temperate forest) ecosystems, but I would recommend the authors to clarify the methodology, and also to rethink some of the concepts that they apply in their setup. I hope that the comments below can support the authors in revising their work.

We thank referee 3 for an accurate summary of our manuscript. We agree that in the concise Methods section we left out some important details that could have increased the clarity of our work. Therefore, we made major modifications to the Methods section, which we have included as a supplement to this response.

Major remarks

A general issue with the methodology is the limited amount of observations that is used to constrain the transfer matrix B. Firstly, these data should be described more carefully, e.g., Figures 3 and 4 seem to suggest that wood or biomass numbers for multiple years are available, which does not appear in the Methods section. Secondly, and more importantly, it seems to me that the available data make it impossible to separate the non-structural pool(s) in the models, which leaves the system underdetermined. The authors also seem to refer to this in their methods (p 7/l 5). It is unclear how a Monte-Carlo setup for sampling the parameter space could help to constrain these, and in fact the authors seem to suggest that it does not. I would suggest to either explain more carefully how the Monte-Carlo setup was applied, which parameters were estimated with it, and which constraints were used, or to skip the Monte-Carlo setup and replace it by a more simple parameter estimation based on literature values, which seems more appropriate for a system that cannot be constrained by observations.

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We agree with the reviewer in that there was limited data to constrain values for the transfer matrix $\mathbf{B}$, and that we did not carefully explained what data was used for the parameter estimation procedure. We included a description of the data that we used and the way in which we calculated the C stocks from it, which addresses the concern on the use of the wood biomass numbers. However, the issue of limited availability of observations is an issue that remains and for which we can do little about. We did in fact used reported values from the literature to fix some of the parameter values (P6L11) or to set ranges for the parameter optimization routines. Nevertheless, we had non-identifiable issues and we think it is important to report them in our analysis in a transparent way. In this sense, the Bayesian optimization we performed, helped us to honestly report uncertainty ranges for possible parameter values, but it is in no way a method to fix the non-identifiable problems we found. We think that by reporting uncertainty ranges for the parameters and the predictions we can better deal with the uncertainty related to the lack of observations and values from the literature.

Although the setup of the three steady-state models is generally well described in the methods, there is one aspect that I have not been able to resolve with regard to the photoassimilates pool. The text seems to suggest that this is not a real pool but just partitions the input vector u to the other pools and to autotrophic respiration, leaving it in essence a flux (or sum of fluxes) rather than a pool. However, in Fig. 2, C_P receives its own box, and the presented results (e.g. Fig. 8) indicate that the photoassimilates pool also has an age. If this is indeed the case in the model, there would be one nonstructural pool (namely, C_P) in Storage 0, and two respectively three non-structural pools in Storage 1 and Storage 2. Maybe the authors can clarify whether this is the case or not. If it is, I would recommend to remove it as a pool, to reduce the size of the vector x and thereby reduce the number of parameters that need to be estimated.

This is a compartment that we decided to keep from the original model proposed by Richardson et. at. (2013). This is in fact not a storage compartment, it is one of the two types of foliage compartments. Since the foliage is divided into photoassimilates and

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structural foliage, the former is strictly a compartment, and has its own state variable. However, this compartment does retain the carbon for a short time, giving the impression to be a component that partitions the input flux. Since this confusion might have been originated from the model schemes in figure 2, we changed the name of the variable C_f to C_{Strf} and we included a short statement about these two compartments in the legend of that figure. Also, to clarify, u is not a vector, it is a scalar, the partitioning vector is β.

The discussion section describes the limitations of the method to some extent, but it should also be used to discuss the validity of the assumptions that are made in the methods. E.g., how important is the annual timestep, and would the results look very different when resolving the annual dynamics? How would varying input u or varying transfer matrix B (as a function of meteorology) change the conclusions? How would a representation of changes in forest structure or regeneration affect the conclusions?

Thanks for the suggestion. Indeed, resolving annual dynamics and making the system time-depend would not have any impact on the main conclusions we derived from this study: 1) age and transit time distributions strongly depend on different carbon allocation schemes imposed by the model structure, and 2) observed mixes of carbon age in non-structural carbohydrate pools can be easily explained by the existence of age distributions in vegetation compartments. Explicitly resolving intra and inter-annual dynamics simply result in time-varying age and transit time distributions, but does not invalidate the existence of these distributions. We added a few sentences to the discussion to address this comment.

The results indicate that the slow non-structural carbon pool has lifetimes that exceed those of the slowest other pools in the models (notably the wood). I would expect the authors to comment to this, as it is at least counterintuitive to have the non-structural carbon of higher age than the structural carbon. This may also be because of the underdetermination in the setup (see my comments above). Could the authors comment on that?

We appreciate this detailed remark. Actually this is only an artifact of cropping the ranges of the distributions in the plots. Since this is clearly causing a confusion, we extended the ranges, so that it is possible to see that the tail of the age distribution for the wood compartment is longer.

The figures in the appendix need to be used in the paper. Is there a difference between Fig. 6 and Fig. A5?

Thank you for this remark; we added the missing references. The difference between figures 6 and A5 is the parameter set with which we run the models to obtain them; for the simulations presented in figure 6, each model was run with a different parameter set, while for figure A5 the same parameter set was used for the three models.

Minor remarks
- The abstract describes the model in very general terms before explaining the application to Harvard forest. Whereas it is fine to present the model in general terms first, I think it could help to mention early on that you are talking about forest ecosystems. You may consider adding "forest" to the title as well.

Although the examples presented in our manuscript correspond to a forest, the approach that we are suggesting is applicable to other systems. The use of age and transit time distributions as diagnostics can be implemented to models portraying other systems, as long as they follow the assumptions explained in the formulas. For this reason we kept the description of the model general enough so it can be applied to other systems.

- p 3/l 12: "random paths" - I do not think that "random" is appropriate in this context; the C can follow multiple paths, but it does not move randomly through a tree. Please consider replacing.

We understand that the term 'random paths' can be misinterpreted in the context of tree physiology. This is why we edited that line to read as follows: At each time step,

a particle may randomly stay where it is, or flow to the next compartment with a rate (speed) given by the transfer coefficients (also know as cycling rates).

- p 6/l 14: There is some repetition in this sentence, and the crucial information on which data sets are used appears to be missing. Please check.

We are grateful for the identification of this issue. We indeed had the same title for different links. We reduced the redundancy and explained more what kind of data we used.

- p 6/l 21: Please explain what the constraint is that is determined from "the NSC calculations from Wood". Do you have a number for the 2nd NSC pool that can be used?

We explained better that we calculated the C stocks of NSC by using equations that derive it from the C stocks in wood.

- p 7/l 6: What is the parameter set with the highest frequency?

The parameter with the highest frequency is the one selected more often by the Bayesian optimization method during the parameter exploration. We clarified this in the text.

- p 9/l 8: replace "tree" with "three" (I suppose) - p 9/l 11: replace "stronger" with "larger"

We appreciate this remark, we made the corrections.

- Figure 3: One of the foliage lines appears to be invisible (or hidden behind one of the other lines). Please check.

There was an overlap between the lines, so we fixed it by dividing that plot into two separate ones, one for wood and the other for foliage.

- It seems that Fig. 4 is not referred to in the text, whereas it is crucial for the paper. Please add a reference to it. - Figure 4: What causes the large spike in the root C

stocks? Is this an initialization problem?

In fact we referenced the figure, but only with a number, which is probably the reason why it was overlooked. We have corrected that. Also, it is probably true that the cause of the large spike in the root C stocks might be due to having the wrong initial values for that compartment.

- Figure 7: The median for storage 1 appears to be missing from panel A.

The line of the median was hidden behind the mean line of another model, we corrected this by lowering the range of the x-axis.

- p 17/l 13: How do you determine the 20-year longer growth? The curves should follow an asymptotic behaviour towards the steady state.

The age and transit time distributions were obtained under the assumption that the models were in steady state. Thus, we can say that the mean ages are predictions of the mean ages of the vegetation once it reaches steady state. Since in each model the mean ages are different, we can consider that their difference is the remaining time that it takes them to reach steady state.

- Figure A4: Some lines appear to be missing.

In this case it is more complicated to fix the overlap because an outcome of running the three models with the same parameter set was that they all had similar mean and medians in their distributions.

We hope that we addressed the comments of Referee 3 adequately and with that improved the clarity of this manuscript.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2017-308/bg-2017-308-AC3-supplement.pdf

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