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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.

Anonymous Referee #3

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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 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

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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.

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.

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 non-

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structural 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.

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?

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?

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

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.

- 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.

- 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.

- 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?

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

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

- p 9/l 11: replace "stronger" with "larger"

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

- 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?

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

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

- Figure A4: Some lines appear to be missing.

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