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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 2 dedicated to review our manuscript. In the text below we quote the referee's comments n italics and provide our response below in blue:

Despite an encouraging title and a promising abstract, I find the study led by Ceballos-Núñez & al. quite disappointing and I doubt that the modelling approach proposed here can be used by a larger community to constrain carbon dynamics in terrestrial

vegetation model.

It is unfortunate that the reviewer was disappointed with this manuscript, but we are convinced that this issue can be easily resolved, because there seems to be certain misunderstandings that will be unveiled in the following points.

The "vegetation" model described in this study is in fact a very simple box model, calculating fluxes between different carbon storage compartments, as well as the carbon stock of each compartment, comparing three model structures (i.e. increasing the number of carbon pools). The model was forced by a constant input of carbon (GPP=1400 gC m-2 year-1) and run on a yearly time step, with no change in environmental forcing (climate, CO2,etc.). The results are shown during the transient spin-up (e.g. fig4) or at steady state (most figures). In my opinion, this approach (i.e. yearly time step, constant GPP, no external forcing) is absolutely not appropriate for "predicting carbon dynamic" as claimed in the title. The actual dynamic of the carbon cycle, i.e. the increasing terrestrial carbon sink, is happening because of transient changes in external environmental conditions affecting the terrestrial carbon dynamics. Therefore it is impossible to draw any conclusion from this study, with respect to the actual dynamic of the system.

It is important to clarify that autonomous systems, as those modeled here, are still dynamic, and an useful tool to assess processes that occur within the vegetation, which in this case is the distribution of carbon among different compartments. If we were interested in predicting the effect of a specific disturbance such as time-varying atmospheric CO2 or temperatures, we could still predict time-varying ages and transit times distributions. However, this is not the objective of our study; we are rather interested in presenting **the concept** of ages and transit times as useful diagnostics of model performance and as a tool to explain mixed ages of non-structural carbohydrates previously reported in field studies. For the case of transient simulations, which the reviewer advocates here, formulas do exist to calculate mean ages and transit-times (see Rasmussen et al. 2016), and if we would have knowledge on the time evolution of process rates at the Harvard Forest for the simulation period, we could have calculated the time evolution of age and transit time distributions. But as an introductory paper on the main concept, we do not consider appropriate to include the additional complexity inherit of the time-evolving formulas. For this reason, we decided to use the autonomous case to introduce our concept.

I also disagree on the way the authors claim they evaluated their model using observation. [i.e. 'We found a good fit of the three model structures to the available data' [abstract]]. First, I understand that the data used for model "evaluation" are the same that the one used for model optimisation as these are the only data mentioned in the manuscript. Is the model calibrated against Harvard Forest data (i.e. results in Table 1) and then compared to the same data for evaluation (Figures 3 and 4)? Or did I miss something? Second, there is no information on how the simulations were done for evaluation.

This is a good point with regard to the "evaluation of the models", since in case that we actually wanted to evaluate them we should have used another data set. However, we actually never mentioned that we evaluated the models. In the figure 3 we simply showed that the model simulations fitted the data points, but this is only to give an idea that the predictions of C stocks are in accordance to a particular forest. We understand that this might be a source of confusion, but it is important to highlight that this work is a theoretical exercise, and the fit of the models to the data is only to have a rough example that can be related to a 'real' forest. We made our intentions clearer in the methods and in the results, as can be seen in the material that we included as supplement of this response.

Figure 4 clearly shows that the model is in transient conditions from 1950 to 2010, with wood carbon stocks increasing and being comparable to the observations in 2010. That would make some sense if the model actually started in 1950, with external forcing (climate, CO2, land use, etc.) changing from year to year. My understanding is that this is not the case here. The model is simply spinning up, slowly reaching steady state.

The agreement in 2010 is hence completely artificial. Cstock wood does not seem the have reached equilibrium, running another 100 years and it would be well above the observations. Unless I missed these two elements, there is strictly no evaluation in this paper.

Again, we are not interested here in finding the best model that reproduces the entire history of C accumulation for the Harvard Forest site as modified by changes in atmospheric CO2 concentrations and climate change, but rather to find a set of realistic parameters that at least can reproduce the trend in carbon accumulation for some of the measured pools in this site. The data shows that this forest is not in equilibrium yet, and our transient simulation approaches these dynamics well. We consider this is enough to obtain a set of parameters that allows us to show some examples of the main concepts we want to introduce: age and transit time distributions of carbon. Please keep in mind that this is a conceptual paper, and we make no claims regarding the accuracy of the predictions for the specific site. We are rather interested in introducing a new set of model diagnostics that can be very useful for more specific simulations.

Finally the co-authors conclude that 'Differences in model structures had a small impact on predicting C stocks in ecosystem compartments, but overall they resulted in very different predictions of age and transit time distributions'. I will argue here that considering the fact that each of their model parameters was constrained using the same carbon stocks, this is a result one should expect as a direct outcome of their methodology and no conclusion on the reality of the processes can be drawn from it.

We agree that sentences such as the one cited can be interpreted literally as "the model structures had a small impact on predicting C stocks differences". However, the C stocks were not listed as one of the metrics that we used. Thus, what we meant was that although all the models had similar predictions in C stocks, they had important differences with regards to other metrics. To avoid this confusion, we made our point clearer in the abstract, results and discussion. It is anyway noteworthy that the results of the sensitivity analysis show that these data is an insufficient constraint, since different combinations can result in the prediction of similar C stocks, hence the equifinality section.

We hope that we addressed the comments of Referee 2 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-AC2-supplement.pdf

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