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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 #2

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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.

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

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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.

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. 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.

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.

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