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Interactive Comment

Interactive comment on "Divergent predictions of carbon storage between two global land models: attribution of the causes through traceability analysis" *by* R. Rafique et al.

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Anonymous Referee #1 Received and published: 28 September 2015 This manuscript examines the carbon cycle of two models, CLM-CASA' and CABLE, using a framework to trace carbon through the system. The traceability framework has been used and published previously – it is useful for comparative studies of the simulated carbon cycle in ecosystems. I appreciate the utility of the traceability analysis for identifying structural or parameter uncertainties in the models, and I think this paper provides a clear comparison between two widely used models. However I have two main concerns with the manuscript as it stands now. These are given below, followed by some specific comments/questions for the authors. First, for a fair comparison between the models the same climate forcing should be used. Even if the mean values of temperature and precipitation are the same, it's more likely the extremes of these that have the largest impacts on the NPP and residence times. I am not convinced that some of the differences between the models is not due to the different climate data sets used.

Response: We acknowledge reviewer's concern for not using similar forcing data as the extreme values may produce different results for different variables. This makes more impact if we focus on the temporal analysis of variables under consideration. In this study, we mainly focused on the global means of all variables after spin-up period to the steady states. We think the use of different forcing data, if their long term means are comparable, will not largely affect the results; however, we agree that the use of similar climate data produces the most ideal results. We have addressed the potential source of uncertainty due to difference in climate data in discussion section of the revised manuscript.

Second, the diagnosis that the differences in NPP are due to differences in Vcmax and SLA is speculative (at least as it appears in the text). External factors can affect the NPP such as temperature, radiation, or precipitation, and as I stated above I am not convinced these differences are not important. Also, internal model differences can affect NPP such as how canopy radiation and moisture stress are handled. Last, five biomes have either a lower SLA (ENF, EBF) or a lower Vcmax (DBF, C3 grass, Tundra) in CLM-CASA' despite that model having a higher NPP – so it is not clear to me the role of these parameters in determining the relative NPP.

Response: Throughout the manuscript we have tried to convey the idea that in the two carbon cycle models there are major structural commonalities. Conceptually both models are similar: organic matter decomposition was a function of temperature, soil moisture, organic matter quality, and soil texture, therefore the differences in model output were not due to structural differences in the models, but due to differences in parameter values. However, it seems not to be the case for NPP: as reviewer notes,

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CLM-CASA' NPP is always higher than CABLE NPP, which is not consistent with Vcmax being lower in CLM-CASA' for some biomes. The formulation of the photosynthesis modules differ between the two models: unlike CABLE, CLM-CASA' has moisture limitation (but it doesn't reduce NPP); CABLE uses combined model for C3 and C4 photosynthesis, whereas CLM-CASA' separates C3 and C4 photosynthetic pathways; the formulation of the RuBP-limited photosynthesis rate is different between the two models [section 3.2.4 c in Kowalczyk et al. (2006) and section 8 in Oleson et al. (2004)]. Autotrophic respiration was simulated differently between the two models: in CABLE wood and root respiration was proportional to their carbon pool sizes, and leaf respiration was proportional to the carboxylation rate; CLM-CASA' calculated whole plant autotrophic respiration as half of GPP. These differences also may have contributed to the differences in whole plant NPP. We included these remarks in section 3.3 and the first paragraph of the discussion.

Specific Comments: 1. I think some clarification on the role of PFTs and biomes in the carbon pools would be helpful. I have a few specific questions: - I am curious how you handle multiple PFTs within a gridbox in the traceability framework. How many pools does each model have in total (what is n in the equations)? For example, does each PFT in CLM-CASA' have 3 plant carbon pools? Are the litter and carbon pools shared between PFTs? - How is the translation from PFTs to biomes done? A table showing the 16 PFTs of CLM-CASA' and which biome each fits into would be useful. - Please provide the full names of the pools in the caption of Figure 3.

Response: We worked with the model output, so we assigned 1 pft per gridbox, i.e. the pft that occupied most of the area in a gridbox (CLM-CASA' simulates up to three pfts per gridcell). CLM-CASA' has 12 carbon pools (simulated at the pft level), and CABLE has 9 carbon pools. We included a table as supplement to the manuscript and provided full names of the pools in the captions to Figure 3.

(TABLE INCLUDED IN SUPPLEMENTARY MATERIAL)

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2. At the end of Section 2.1, the q10 values are given but not their context – do these apply to soil respiration?

Response: The context of Q10 value has been added in section 2.1 of revised manuscript.

3. What determines the baseline residence time? Why do deciduous needleleaf and evergreen needleleaf forests have the highest baseline carbon residence times? And why is there such a large difference in the baseline residence time for tundra between the models?

Response: Baseline residence time is determined by the matrices of A and C as well as vector B. Where, A represents the carbon transfer among pools, C represents the respirational losses and B represents the partitioning coefficients. The A, C and B components are determined by soil texture and vegetation lignin fraction. The deciduous needleleaf and evergreen forests showed highest baseline residence time because they partitioned the largest fraction of NPP to woody biomass. For tundra the baseline residence times differed also likely due to the partitioning coefficients, because both models simulated similar environmental scalars of 0.1. This has been clarified in discussion section of revised manuscript.

4. The residence time is higher in CABLE – and this is attributed to higher residence times in wood and a higher allocation of NPP to wood. Could another reason be that once carbon enters the passive SOM pool in CABLE, it does not interact with the slow pool? What is the effect of the more complicated interactions between soil pools on the residence times in CLM-CASA'?

Response: The more complicated interactions between soil pools in CLM-CASA' slightly increase the residence time (but not significantly), because instead of leaving the system, carbon returns to another pool, thus staying in the system longer. To demonstrate that, we can introduce similar interaction to the CABLE model structure (i.e. return of carbon from passive pool to slow pool, and from slow pool to fast pool),

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using the same two transfer coefficients as in CLM-CASA' (0.45 and 0.4). If we assume NPP is 600 g C/m2/year, and the parameters in CABLE are those listed on Figure 3 in the manuscript, the ecosystem carbon residence time will be 13.06 years. If we modify the model structure to include the interactions mentioned above, the ecosystem carbon residence time will be 13.68 years. This experiment illustrates that interaction between soil pools increase carbon residence time, but not significantly. This has been clarified in the discussion section of revised manuscript.

5. At the beginning of Section 3.5, I think it should be reiterated that a lower environmental scalar limits decomposition and turnover and therefore increases the final ecosystem residence time of carbon.

Response: The suggested information by reviewer has been incorporated in the beginning of section 3.5 of revised manuscript.

6. What is meant by the last sentence in the first paragraph of the Discussion: "Longer ecosystem residence time in CABLE was mainly attributed to higher environmental limitation of the organic matter decomposition." This seems to be in contrast to what is shown in Figure 6 – which shows a larger difference in the baseline residence time than in the environmental factors.

Response: After taking consideration of reviewer's comment, the confusing sentence has been removed from the revised manuscript.

7. In general the abstract and Discussion/Summary sections communicate the main results of the study very well. I have one suggestion: the Discussion mostly addresses the 3 objectives given in the Introduction, but it could improve the paper to more explicitly address these objectives and the main conclusions pertaining to each.

Response: We appreciate reviewer's constructive feedback. In order to make this study more consistent, we have clearly stated the objectives of the study. We have also highlighted the main take away messages of this study on abstract and discussion

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sections. We hope this will be helpful in conveying the overall message of this study.

Kowalczyk, E.A. et al., 2006. The CSIRO Atmosphere Biosphere Land Exchange (CABLE) model for use in climate models and as an offline model. CSIRO Marine and Atmospheric Research. Oleson, K.W. et al., 2004. Technical description of the community land model (CLM). NCAR Tech. Note NCAR/TN-461+ STR: 173.

Please also note the supplement to this comment:
http://www.earth-syst-dynam-discuss.net/6/C1137/2016/esdd-6-C1137-2016-supplement.pdf

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Interactive comment on Earth Syst. Dynam. Discuss., 6, 1579, 2015.

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