

**Interactive comment on “Evaluation of modeled global carbon dynamics: analysis based on global carbon flux and above-ground biomass data” by Bao - Lin Xue et al.**

**Anonymous Referee #1**

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This study uses IBIS in a calibration then validation study to examine AGB, GPP, and ET. This is not a very novel contribution but has the potential to be a solid iteration. My main concerns are (1) the lack of detail on calibration, (2) the lack of a pre-calibration baseline, (3) the lack of variable  $\tau_w$  even though this is known and has already been done for this model, (4) the relevance of ET, (5) the lack of detail on the flux data, and (6) language issues. These are all detailed in the specific comments below. Assuming these are addressed this study could be suitable for publication in Biogeosciences.

Reply: Thank you very much for your comments. We have now revised our manuscript with substantial changes. As shown in the revised manuscript, major changes of our revision include:

1) We conducted manual calibration for the parameters; explanations are now mentioned in the revised manuscript (MS).

2) We now add the model simulation of GPP and AGB with default IBIS parameters as baseline. A comparison of baseline modeled GPP and AGB with results from calibrated model run have been provided as new figures (Figs. 2 and 3 in MS).

3) We estimated pan-tropical woody residence time ( $\tau_w$ , years) by collected values using a random forest method. We then estimated the pan-tropical AGB using the estimated  $\tau_w$  (Figs. 5 and 6 in MS). Independent validation shows that by using spatial variant  $\tau_w$ , the model can simulate AGB better with lower RMSE compared with using single set of  $\tau_w$  for a specific PFT (Fig. 7 in MS).

4) Results related with ET were all deleted in revised MS.

5) Details on fluxnet data were provided.

6) We further polished the MS by native speakers.

A detailed explanation of our revision is provided by a point-to-point response below.

L24: You go from “the simulation of carbon dynamics on regional and global scales” to biomass. But carbon dynamics are more than biomass! Also, later in the paper you look at ET, this is not mentioned in the abstract until you get to “Independent validation” (L31).

Reply: we actually mean AGB; and we now revised. Results related ET are all removed in our revised manuscript according to your suggestion.

L40: I’m having a hard time with your: “The conclusions of our research highlight the necessity of considering the heterogeneity of key model physiological parameters in modeling global AGB.” This is not a new insight. What have you added here? How has the body of knowledge on this point been expanded? And I think the community knows that getting fluxes and stocks correct is a good idea to enhance predictive skill. More generally, I am struggling with the novelty of this study. I feel like I’ve read dozens of such papers before;

Reply: Thank you for your suggestion. We added more numerical experiments:

1, we generated a gridded  $\tau_w$  map (1 km resolution) for pan-tropical forests by collected  $\tau_w$  using a Random Forest method (Fig. 1). The pan-tropical area was selected because it is easier for us to collect enough  $\tau_w$  observation within a short time (i.e. review period). All observed  $\tau_w$  values used in our calculation are from Galbraith et al. (2013).

2, we simulated the pan-tropical AGB using the estimated  $\tau_w$  and we validated the simulated AGB using plot level AGB observations (Fig. 2).

Our new results further indicate that model simulated AGB was improved with a lower RMSE when estimated gridded  $\tau_w$  was used. These figures and corresponding explanations are now added in the revised MS.

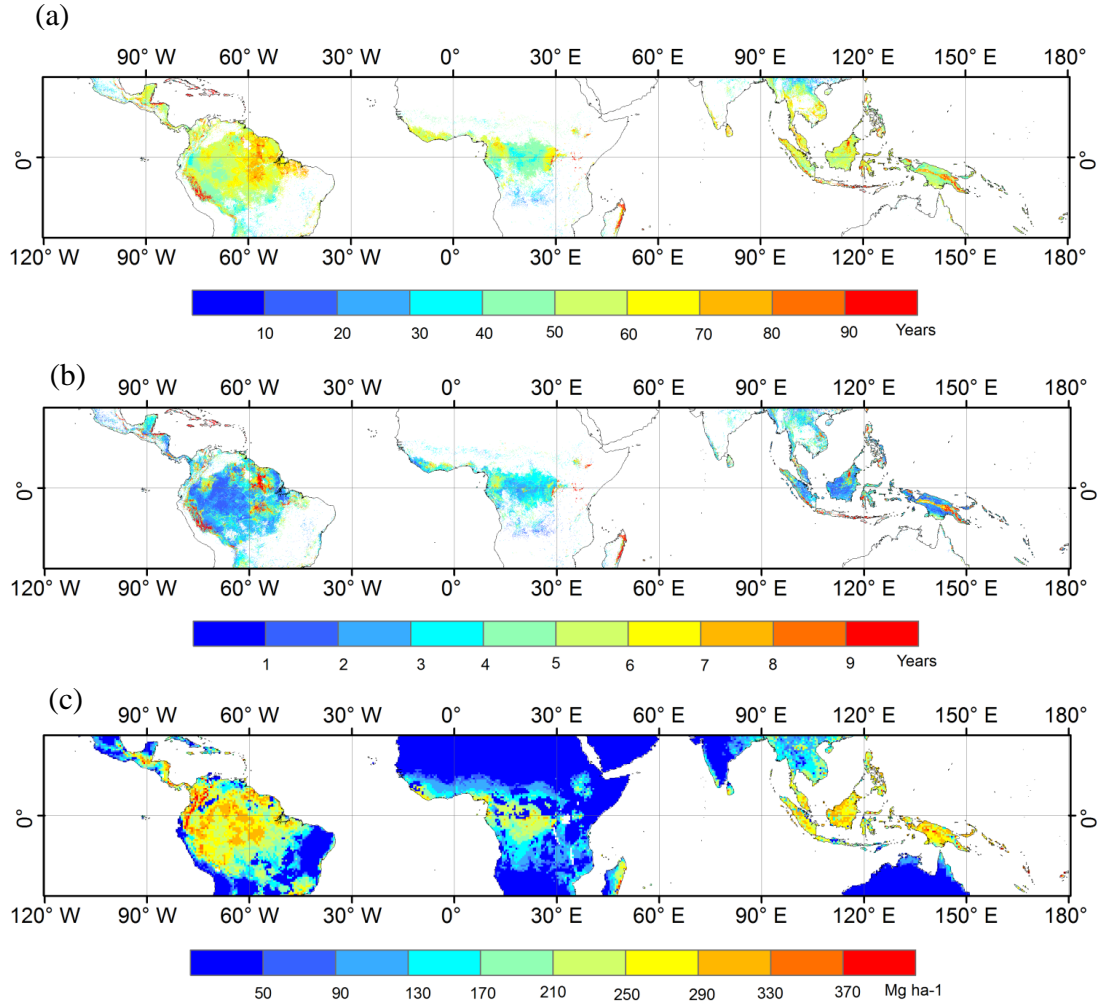


Fig. 1 (a) spatial pattern of woody residence time ( $\tau_w$ , years); (b) uncertainty of estimated  $\tau_w$ ; (c) simulated AGB by estimated  $\tau_w$ .

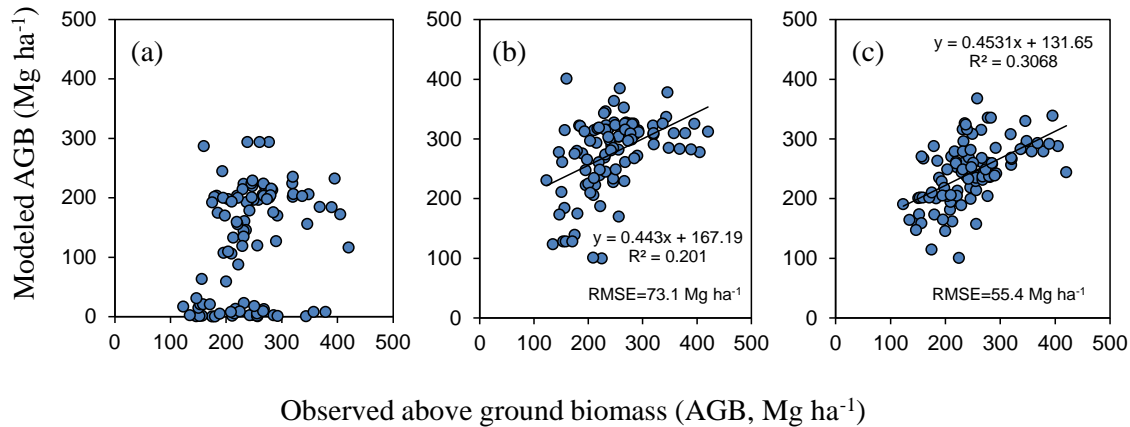


Fig. 2 Comparison of observed and modeled AGB for (a) baseline run with default  $\tau_w$ ; (b) calibrated run with calibrated  $\tau_w$  and (c) estimated gridded  $\tau_w$ .

*Galbraith D, Malhi Y, Affum-Baffoe K et al. (2013) Residence times of woody biomass in tropical forests. Plant Ecology & Diversity, 6, 139-157.*

L54: One of many language issues: “The large carbon stock in the terrestrial ecosystem indicates the need for a reliable description of its current distribution and prediction of future variations”. This is a non sequitur as written. I think I know what you want to say here but I should not have to divine intent and meaning. But please have this edited by a native speaker.

Reply: sentence revised.

L57: Is not the main reason it is hard to get global scale carbon stocks a lack of good observations?

Reply: Yes, we agree and have revised the sentence.

L73: Try “alter global biomass”. The full sentence here (L72) is also a non sequitur as written.

Reply: sentence revised.

L87: Here you only mention potential whereas above you also had present-day. Please be careful! Trivially we are nowhere near some idealized potential state such that a model might be more concerned with getting present-day correct.

Reply: thank you. We know that it is hard to derive idealized potential state. It is therefore unreasonable to directly compare the modeled and present-day AGB. We now mentioned this in the discussion.

L100: Starting here in this paragraph I feel you have already made these points. Also, where is ET. It’s thrown in at seemingly random times and is not part of the story. Why do you even include ET?

Reply: original manuscript included ET to explain the land surface scheme can simulate the water cycle coupled with carbon cycle by stomatal conductance. According to your suggestion, all results related with ET are deleted in the revised version.

L116: I need more detail on how you calibrated IBIS parameters. It's not explained to sufficient detail anywhere in the text.

Reply: The parameters were manually calibrated using a try and error method until the model simulation results of GPP and AGB matched the observations. In brief, we use the observed GPP and AGB to constrain model parameters until  $R^2$  for most sites have large values.

**Trial and error**, or trial by error, is a fundamental method of solving problems. It is characterized by repeated, varied attempts which are continued until success, or until the agent stops trying (Source: [https://en.wikipedia.org/wiki/Trial\\_and\\_error](https://en.wikipedia.org/wiki/Trial_and_error)). Though this manual calibration method is time consuming, it is commonly used among the modelers (ecologists and hydrologists etc.). Some automation calibration methods are proposed; however, these methods also have the problem of low efficiency such as the Monte Carlo method (See Xiao et al., 2014). These explanations are provided in the revised MS now.

*Xiao, J.F., Davis, K.J., Urban, N.M., Keller, K. (2014) Uncertainty in model parameters and regional carbon fluxes: A model-data fusion approach. Agricultural and Forest Meteorology, 189-190, 175-186.*

L122: You mean hydrological?

Reply: thank you. Revised.

L125: Remove "In detail,"

Reply: Removed.

L143: Remove both uses of "the".

Reply: Revised.

L191: Is not there a more recent version of CRUTS?

Reply: Thank you. We only found the version we used when we run the model. It seems that it is impossible to reform the new version into IBIS inputs within the review period. But we hope we can do this in future research.

L197: "The Princeton..." reads awkwardly. Maybe "The Princeton forcing data..." is better.

Reply: thank you. Revised.

L207: If IBIS can't do croplands why perform any comparison?

Reply: croplands are treated as grasses in IBIS; we actually calibrated parameters for understory.

L214: Same awkward "In detail, : : :". Perhaps you want "In brief, ..." instead.

Reply: We have deleted this sentence since this figure is removed in revised version.

L216: A minor point but you might want to capitalize Random Forest as you use forest as a PFT/biome in many places.

Reply: we use “Random Forest” now.

L219: I am still unsure if you are doing global runs here? The preceding text references the Amazon. Also, despite your description I am unclear how “plot-level AGB data from the literature” was used to tweak IBIS out-of-the-box parameters. That is, how was Objective (2) satisfied?

Reply: we apologize for our ambiguous expression. We indeed run the model for global scale and calculated AGB. We further run the model for the pan-tropical forest areas with estimated  $\tau_w$ .

L221: Break into 2 sentences please.

Reply: Revised.

L228: no “the” before GPP.

Reply: Revised.

L232: a rho of 0.6 means 36% variance explained. And this is a lower bound? Can you elaborate here? How can you reconcile the lower bound you use in the text with your “the model can simulate the energy balance well” (L236) statement.

Reply: it is actually determination coefficients ( $R^2$ ). Note that for eddy covariance method, energy closing is usually around 80%, i.e. latent heat flux + sensible heat flux =  $0.8 \times (\text{net radiation} - \text{ground heat flux})$ .

L245: How does a small value have a large intercept?

Reply: we think that large intercept was caused by the overestimation of small values.

L247: Try “When GPP was below...”

Reply: Thank you. Revised.

L248: Try “simulated GPP was: : :” GPP is not used as a plural. Fix this throughout the paper.

Reply: Revised.

L251: How does a tower focus on high-production ecosystems when it’s sited in a marginal system? This makes no sense.

Reply: Revised.

L252: I found this section rather hard to read. One example: “the model simulation results are around  $100 \text{ mm year}^{-1}$  smaller than those from Jung et al. (2011), especially for low ET areas (Fig. 4).” The “especially” fragment makes no sense. And why “model simulation results” as opposed to just “simulated results”? I would encourage you to look at the full manuscript for clarity and flow.

Reply: This section was deleted according to comments from reviewer 2; He/she believes this comparison is not meaningful.

L254: Remove the comma.

Reply: Revised.

L277: Your “scale difference” argument here is not clear. Please elaborate.

Reply: we use spatial resolution instead.

L279: What is “plot accuracy”?

Reply: Deleted.

L286: In this section you overuse the word “shows”.

Reply: expressions revised.

L292: What is “below -30°S”? The minus sign is redundant here.

Reply: thank you; revised.

L294: How did you pick the bounds for your latitudinal regressions? And why is this relevant here? In general, the writing has a tendency to walk the reader through the figures. That’s not bad per se but it’s overly verbose as is. Let the figures do some of the talking. I would recommend less length here.

Reply: thank you for your suggestion. This paragraph is much shortened now. Actually, we shortened all the results part based on your comments.

L312: Language again, the hanging fragments of “according to the two meteorological datasets” and “with large heterogeneities in different areas” simply do not add value, only volume.

Reply: Revised.

L325: “Comparisons of carbon densities have the advantage over AGB comparisons that they eliminate the uncertainties induced by global vegetation areas used in different studies.” Not sure I buy this. It seems you are assuming that density is the same across vegetation gradients. And we know this is not the case. And your L336 goes against this anyway.

Reply: yes, as you said, we assumed the same carbon density across vegetation gradients. In light of different vegetation areas we compared in the following text, we deleted this sentence.

L361: There has been more work on tau beyond the IBIS work. And if this is known as the “most important parameter in determining the spatial variance” why was it not addressed here?

Reply: additional results provided; please see above.

L374: “The interannual variations in the GPP were mainly caused by different meteorological drivers.” This is an odd sentence. How is IAV “caused” by different forcings? And drop “the” before GPP.

Reply: According to Jung et al. (2007), if we use different meteorological datasets as model drivers, the simulated GPP would be with substantial differences. We revised the sentence.

L379: Figure 8 does not show this. There is no “transfer function” from GPP to AGB (or vice versa) shown in the figure!

Reply: Deleted.

L422: “The research also shows that to simulate large-scale carbon dynamics, both carbon flux and AGB data are necessary to constrain the model.” Fine, I think we knew this before this study was executed. But why not extend this. Add results using the out of-the-box parameter values, so pre-calibration. What is that effect size? How does parameter-induced spread compare with forcing data-induced spread and spatial heterogeneity. Your second question (L116) is: “Can a single set of calibrated parameters accurately map the patterns of GPP and AGB?” It strikes me that to answer this query we need a baseline without calibration.

Reply: addition results and baseline numerical experiment results are provided in revised manuscript; please see above.

Table 2: Please add time period so we have a better sense of sample size. How did you navigate the La Thuile fair use data policy? I’m assuming that’s where you harvested the FLUXNET data.

Reply: we add time period coverage for the flux datasets.

Figure 1: Label x-axis. What are the 20, 40, 60, 50, 100, 150 values for?

Reply: original Figure 1 is now deleted, since it is redundant information (same with Table 1 and revised figures).

Figure 2: Add labels for y- and x-axis. Get rid of the “or”. Make sure inset text does not overplot.

Reply: Revised.

Figure 3: Minor point but the use of the same colormap in both plots (one inverted, one not) makes this harder to read than needed. A third panel showing relative differences( $100 \cdot b/a$  in effect) would be a good add. Fix subscripts on colorbar units.

Reply: figure removed according to comments from reviewer 2.

Figure 4: Same comments as for Figure 3.

Reply: figure removed.

Figure 5: Same comments as for Figure 2.

Reply: Revised.

Figure 6: Fix subscript. I’d like to see (b) flipped so that the lat bands match how one would read map. Why not add a mean zonal vector for both forcing cases?

Reply: thank you for your suggestion; revised.

Figure 7: The (c) panel occupies a bit too much space. It’s really not that exciting anyway, you could drop it. Also, ylabel is misspelt. Or, map the relative differences as I’ve

suggested in preceding figures.

Reply: we deleted the figure.

Figure 8: Same comment as for Figure 7.

Reply: we deleted Fig. 8 (b).