Geosci. Model Dev. Discuss., 8, C680–C687, 2015
www.geosci-model-dev-discuss.net/8/C680/2015/

Geoscientific Model Development Discussions

Interactive comment on "Improving the ISBA$_{CC}$ land surface model simulation of water and carbon fluxes and stocks over the Amazon forest" *by* E. Joetzjer et al.

E. Joetzjer et al.

emilie.joetzjer@gmail.com

Received and published: 7 May 2015

We are grateful to both reviewers for their helpful and constructive comments. Please, find hereafter our point-by-point response.

This paper presents results based on improvements made to various aspects of the ISBAcc model. The developments are grouped into improvements relating to photosynthesis and soil water stress (PS) and additional improvements to the respiration of various biomass pools (PS+R). With the PS+R version, the biases in latent and sensible heat flux, GPP, and ecosystem respiration are generally reduced. The model performs comparably to the ORCHIDEE model. To me the most notable improvements

are the carbon stocks and division between heterotrophic and autotrophic respiration. The paper shows important progress in simulating fluxes in the Amazon and should be published in GMD, I do have some suggestions for improving the manuscript and advise minor revisions.

The paper is well organized and the results are clearly explained. However, there should be more attention to the uncertainty in the observations and a link between the model results and site-specific processes. The authors state several times that there is large uncertainty in the flux measurements and particularly in the partitioning between GPP and RECO. I am in favor of using these measurements for model evaluation but I think uncertainty bars should be added to the figures. If not, a more quantitative discussion of the results within the context of observational uncertainty would help the reader judge the improvements in the model. My second major comment is the link between the site-specific results and processes at the sites. It is stated a few times that the differences in model biases between sites are possibly due to errors in the forcing data or observations (for ex. Around Line 20 on page 1308). I'm not sure I agree with this statement, especially based solely on the fact that the two models perform similarly. It seems more likely that these differences are because of fundamental processes that are different between the sites that neither model captures. I have some specific examples and suggestions below.

We thank the reviewer for the suggestion to look at the overall energy closure of each site. This helped us quantify what we suspected from modelling only. We also added a few references to link our results to the specificities of each site. Our initial goal however was to evaluate a land surface model over that is used within a global climate model. Having this goal in mind we needed to test if the model could reproduce fluxes and pools over a fairly wide range of sites within the Amazon forest (and the CTL version could not). Our goal was not to use the model to gain insight in the functioning of each site, which, we agree is probably more interesting scientifically.

Lastly, I recommend some proofreading and editing. There are several instances where

the wording is not precise or sentences are unclear. Ok had the manuscript read by a native english.

Specific comments Page 1297 (Section 2.1): What method was used to calculate GPP and RECO from the NEE? And why was this not done for the GFG site?

The fluxnet data are available online, the partition between GPP and RECO is done following Reichstein 2005. For the GFG site the PI (Damien Bonal) did not release the GPP data.

Page 1298 (end of section 2.1): This would be a good place to mention potential problems with energy closure at the Fluxnet sites. How well do these observations close the budget (ie what is LE+H/Rn for each site)?

We did calculate the overall energy closure according to Wilson 2002. Thanks for the suggestion because it shows a priori that the Manaus and, to a lesser extent, Jaru sites have observation data that are less coherent with each other than the other sites.

Page 1299 (Section 2.2): I think there are some typos around Lines 20-23. Are the 3 carbon pools active, passive, and slow? Or is the 3rd pool both slow and passive? Also do you mean 'labile' instead of 'liability'? This should be reworded.

Indeed, corrected.

Page 1300 (Section 3): The text in this section explains the different model versions in a clear way, but I found it difficult to follow Table 3. For one thing it's not clear what the "tolerant" and "linear" experiments are. Also the columns seem to switch halfway down, from depicting differences between CTL and PS to differences between gm and f0 calculations. Other suggestions: The sentence about Table 2 should be earlier in the paragraph, and it should be explicitly stated that Table 3 refers to the parameters used in the PS experiments.

Indeed, the table was modified during the edition process. We changed it to improve clarity. Page 1302 (Section 3.2, Near Line 15): Perhaps to help with Table 3, the

equations for soil water impact on f0 and gm can be moved here. Also it's not clear how these equations changed between the CTL and PS experiments.

We prefer to keep them in the Table (although reorganized) because this paper does not focus on the water stress functions (see Joetzjer et al. 2014). And there is very little moisture stress in these 5 sites during the time frame studied.

Page 1303 (Section 3.3) In the description of B4 pool: is this pool for the sapwood of the roots? If so there needs to be an apostrophe after roots to clarify (roots' sapwood).

Indeed, modified (also modified for B6 root's heartwood)

Page 1304, Equation 7: Double check this equation. Should the LAI term be part of the exponent?

Indeed, thanks, modified.

Page 1305 (Section 3.3.2): Typo in heading name (change 'trunc' to 'trunk').

Indeed (modified).

Also what are the values for βwood, E0 (mention that the values are given in Table 4)?

The values are now in the text.

Is the β the same in Equations 11 and 13?

Yes, also added in the text.

Page 1306 (Section 3.3.3): The SLA is mentioned here but it's never stated where in the model the SLA is used.

Indeed, we did add a paragraph (section 3.3.4) to be more clear Note that we chose not to describe in detail the parametrization of the SLA. We only summarize the concept and give appropriate references.

Page 1307, Line 5: What do you mean by "successfully evaluated", can you briefly state

the results of that evaluation (ie: are the model results similar between K67, Caxiuana, and K83)?

We add a reference to the figure from (Joetzjer et al. 2014) showing the evaluation of the Soil Water Content at K67 and Caxiuana in the text. We showed in this study that monthly Soil Water Content in the top 3m was correctly simulated by the modified version of ISBAcc (with the linear WSF) in terms of quantity (biais < 5%) and seasonal variability (correlations =0,9 at Caxiuana, but only 0,6 at K67 because the meteorological forcing did not cover the whole period (see details in the paper).

Page 1308, First paragraph: Taylor diagrams are now commonly used but it still would be helpful to orientate the reader as you begin to discuss Figure 5 (for example: Lines of constant correlation extend from the origin, and standard deviation relative to the observations is denoted by the blue radial lines, etc). This is especially true because you have several different statistics displayed in the figure, and sometimes it is unclear whether you are referring to the Taylor diagram or to the bias plots.

Ok added (text and legend)

Page 1308, Second Paragraph: The improvements in the model appear to be substantial. Do these occur year-round or are the improvements focused during a particular season?

Improvements are not season dependant. It is said in the 1st paragraph of section 4.2 but we did a reminder in the last paragraph of the section.

Also, why is the bias still so high at Jaru? Here is one place where a link between the model results and characteristics of the sites would be helpful. Also can you be more specific with your final sentence in the paragraph – is there evidence in the literature for which processes might be missing? See for example Baker et al. 2013: Surface ecophysiological behavoir across vegetation and moisture gradients in tropical South America, Agricultural and Forest Meteorology (attached as a supplement), and

da Rocha et al. 2009, (already cited in this study).

Thank you for the papers. As pointed out by Baker et al, 2013, Jaru is wetter than the Santarem sites, with a longer dry season and a rather different radiation seasonal cycle, being located at 10 degree S. But in our case, we also have the Guyaflux site, that is also very different. It is even wetter than Jaru, with an equaly pronounced seasonal cycle and is located at less than 20 km from the ocean. So we could expect a large bias too, which is not the case.

So we agree with the reviewer that site specificities are important and we added a few lines and references in section 4.2 but in our case we believe that the coherence of the dataset itself plays an important role.

Note also that a high bias in H expressed as a percentage is not that important since H is relatively low in the Amazon. So a 100% bias on H at Jaru is actually not that great amount of energy. Page 1309, Line 1: I would not say the GPP is correctly simulated by the CTL experiment, although the annual magnitude appears to be roughly correct.

We added “annual magnitude” in the text.

Page 1309, Line 12: What is meant by “the model behaves as expected”? It means that when the soil moisture is not limiting and the radiation increase, the model simulates an increase of GPP. The seasonal cycle of GPP is actually very similar to what Baker et al simulate at this site with SiB (fig4).

Page 1309, Lines 13-15: This implies that the latent heat flux from the model is mostly due to flux through the stomata, while the observed LE has other important sources. Is the modelled LE mostly from transpiration? What are the other sources of LE in ISBACC ?

In ISBAcc the evapotranspiration term is the sum of the evaporation from the soil, the evapotranspiration from the water intercepted in the canopy and the transpiration from the plants. Over the Amazon, the transpiration represents about 70% of the evapotran-

spiration.

Page 1309, Line 26: What are the “observation uncertainties”? The discussion that follows regarding the measurement uncertainty is useful but also highlights why it would be good to quantitatively include these uncertainties in the analysis.

Page 1311: Is there mortality in the model?

The turnovers rates can be assimilated to a background mortality in ISBAcc.

Table 4: Is 1/SLA constant in both the CTL and PS+R experiment? Also what is the Ts and Tp in the CTL column referring to?

As now explain in the text (see paragraph SLA) in the CTL version the SLA was calculated. As indicated in the table Ts and Tp are respectively the surface and the soil temperature

Figure 2. The figure legend refers to Calvet et al. 1999, but the corresponding text at the bottom of

Page 1300 refers to Calvet et al. 1998. Which is correct?

Indeed, the correct reference is Calvet et al. 1998. We modified the figure. Figures 4 and 6: The display of diurnal cycles for each month is very useful, but for the seasonal cycle it might be easier to judge the model if the 3 years of data are averaged together. We agree, but we also wanted to show inter-annual variability, and having 3 graphs would have been cumbersome. We preferred to show the seasonal change in diurnal cycle (left panels) and the interannual variability of the seasonal cycle (right panel).

You could also indicate the standard deviation of observed fluxes to give some estimate of the uncertainty in the measurements.

As standard deviation is highly dependant of the time step considered we chose to don’t add them. Indeed, if it’s calculated with hourly data the diurnal cycle hide the errors, over hourly data they show the atmospheric conditions (e.g. rain/no rain) while

over monthly data, we see the seasonal cycle and the interannual variability.

Also, can you denote on the figure which months are the dry season?

Ok, added.

Interactive comment on Geosci. Model Dev. Discuss., 8, 1293, 2015.