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# ***Interactive comment on “Improving the ISBA<sub>CC</sub> land surface model simulation of water and carbon fluxes and stocks over the Amazon forest” by E. Joetzjer et al.***

## **Anonymous Referee #2**

Received and published: 20 March 2015

This paper presents results based on improvements made to various aspects of the ISBA<sub>CC</sub> 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.

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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.

Lastly, I recommend some proofreading and editing. There are several instances where the wording is not precise or sentences are unclear.

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?

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)?

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.

Page 1300 (Section 3): The text in this section explains the different model versions

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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.

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.

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).

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

Page 1305 (Section 3.3.2): Typo in heading name (change 'trunc' to 'trunk'). Also what are the values for  $\beta_{\text{wood}}$ , E0 (mention that the values are given in Table 4)? Is the  $\beta$  the same in Equations 11 and 13?

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

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)?

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

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whether you are referring to the Taylor diagram or to the bias plots.

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? 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 behavior 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).

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.

Page 1309, Line 12: What is meant by “the model behaves as expected”?

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?

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?

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

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?

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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. You could also indicate the standard deviation of observed fluxes to give some estimate of the uncertainty in the measurements. Also, can you denote on the figure which months are the dry season?

Please also note the supplement to this comment:

<http://www.geosci-model-dev-discuss.net/8/C193/2015/gmdd-8-C193-2015-supplement.pdf>

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

**GMDD**

8, C193–C197, 2015

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