Geosci. Model Dev. Discuss., 8, C672–C679, 2015
www.geosci-model-dev-discuss.net/8/C672/2015/

Geoscientific Model Development Discussions
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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.

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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.

Anonymous Referee #1

This paper details the improvements made to the ISBACC through the introduction of: 1) a new soil water stress function, which alters the modeled photosynthesis and 2) a new autotrophic respiration scheme. The research tests some interesting adaptations to the model to work towards improving the ability to simulations of respiration and the impacts of seasonal variations in water flux in tropical forests; work which is greatly

needed. However some work is needed on the manuscripts format and writing style.

Major comments:

1) I find this manuscript confusing to read in places. Informal language is used frequently throughout the manuscript and the order or appropriateness of words often seems wrong.

We had the manuscript read by a native english scientist who corrected parts of it.

2) I find the structure of the manuscript odd. Firstly some of your results are introduced in the methods as justification, rather than a clear explanation of what questions and hypothesis you will test.

We are not sure what part of the manuscript the reviewer is referring to. Is it the introduction to section 3 If yes, we indeed wanted to explain why we chose to modify the model in such a way. We believe that bad model results is indeed a motive for improvement. We did add however a sentence reminding the scientific goal already stated in the introduction: "Indeed, large biases in the simulated latent heat and respiration fluxes are not acceptable when modelling a region where precipitation recycling is important and where changes in the carbon fluxes could have profound effects on the global climate." Secondly the text seems disjointed in the results and discussion section; I find myself repeatedly trying to find the explanation for a stated result given and either find no explanation or an explanation given in a separate section. In a combined results and discussion section I would expect results to be stated and followed by an explanation, which is put in the context of relevant literature. Perhaps this section can be restructured and made clearer for the reader. We chose not to change the structure of section 4, but we did add explicit references to the explanations given elsewhere in the text ("as is shown in section...")

3) I do not understand why ORCHIDEE is used in this manuscript; no clear explanation is made of how the comparison with this model adds to the conclusions of the

manuscript.

The idea behind the use of several models was to try to estimate the uncertainty due to the land-surface model choices in the simulations of the Amazon future. For that purpose, we first had to have 2 models that behaved reasonably well, hence the work on improving ISBA. It had already been done with ORCHIDEE. Now, this exercise showed us that in a forced setting, as it is the case here, both models react fairly similarly despite very different hypothesis behind the representation of fundamental processes like photosynthesis, allocation, or phenology.

4) Section 4.1: I would argue that soil texture is not the only issue controlling SWC distribution with depth. SWC with depth will be strongly influenced by your root water uptake too and therefore dependent on: a) differences in root biomass between the model versions, b) differences in the vertical distribution of roots and c) differences in the LAI and gs. I feel that these issues need to be addressed and shown, to test what is truly driving the differences in SWC. This is briefly touched on in the section 4.2, but is not comprehensively dealt with to assess what really drives the changes in SWC distributions between models.

We agree of course that soil texture is not the only factor controlling vertical soil moisture distribution. But in this particular case, we believe it is the most important because a) an b) water uptake in ISBA does not depend on root biomass. Roots have a predefined depth and vertical distribution (based on Jackson et al 1996 work) kept constant. So root depth and profile is identical between the simulations and can't play a role in the differences between simulations. Now looking at the bias common to both simulations (too fast wetting of the soil), it could be explained by too low water uptake by roots during these periods. But increasing water uptake by roots during these periods would increase too much evapotranspiration. But we agree that this section is a bit too short. We rewrote it and included the uncertainty in the root water uptake.

5) The key changes caused by the addition of the respiration and the new water stress

function are changes to both the LE and H fluxes and the carbon stocks/allocation. There is however little effect on the NEE, Reco and GPP caused by PS+R. Presumably there are many trade-offs between the model formulations that allow such drastic shifts in C allocation and storage, and shifts in H and LE whilst maintaining the same C flux balance. These trade-offs are very important for the interpretation of the results, but not presented clearly or well explained in the manuscript.

We added an explanation of the trade-offs in section 4.3: "So, there is a trade-off in the model between f0 and Ammax, that can be expected from the photosynthesis module. A lower maximum assimilation rate (Ammax) tends to reduce the carbon assimilation (see eq A7 in Calvet and Soussana, 2001). On the other hand, with a higher f0, intracellular CO2 increases (see equation 4), which favors carbon assimilation." In section 4.4 we didn't want to repeat why Ra and Rh partly compensate each-other, but we tried to improve the clarity of the paragraph that explains it.

Minor comments:

P1299: "The fractions of newly formed assimilates or reserves allocated to these pools are parameterized as a function of soil water content, temperature, light, and soil nitrogen availability" I am sorry I don't really understand how these parameterisations are made.

We wanted to just give a sketch of ORCHIDEE carbon allocation scheme. But it is indeed too short to be understandable. As it is not essential to our purpose we dropped this sentence.

P1299: L13-15: What is an above ground metabolic and structural pool, is this carbon pool and if so how is this differentiated from an above ground biomass pool?

There was a mistake in the sentence that we corrected and we now specify consistently that we are talking about biomass pools.

P1301: DS is suddenly introduced here, but there is no explanation of this acronym.

Indeed, there was a mistake in the text, D in place Ds (corrected). Note that Ds is also defined in the table 2.

P1306: L 20: I don't really understand what you mean by "model diversity" and what we can illustrate with it.

Ok, model diversity is exaggerated considering that we only have to models. However it's interesting to compare ISBAcc to Orchidee considering that ISABcc has never been evaluated on tropical forest while Orchidee has been largely evaluated for this PFT. We did reword the manuscript.

P1307: L3: What wet bias? You reference no figure or give no quantification! Also the lower panel actually shows that soil moisture contents seem to be relatively similar between the models. If you were able to put error bars on the observations to represent spatial heterogeneity (which can be very large) would you expect either model to be outside of these error bars?

We agree. That is why in the text we wrote "the slight wet bias".

L8: "...allows the model to simulate a relatively wet top-1m horizon as observed." Again you reference no figure or give no quantitative way for the reader to assess this.

Added (Fig. 3 mid panels).

P1308: L6: Acronym SD used and not explained.

Standard deviation (corrected).

L7: "The CTL runs show a systematic overestimation of H that is strongly reduced in both PS and PS+R versions." This sentence is a bit repetitive of previous sentences.

We are describing the bias (bottom) part of the figure. But it was not clear so we rewrote the sentence.

L10-15: No explanation is provided as to why LE and H biases vary between sites

here.

We added a sentence about the low energy closure of the Manaus measurements

L15-24: What is the explanation for the model improvement?

Explained before "The partitioning of the energy budget is better represented with the simulation using Am,max and f0 parameters derived from the in situ observations (PS version, Table 3)."

L19: Again you reference no figure and give no quantification to back up your result.

Indeed, Figure 5, reference added. The quantification is given in the Taylor diagram.

L21-24: I disagree with this statement. I would say that the likelihood of two models being wrong at the same location is not small, particularly in tropical forests where, as your paper is suggests, there are many mechanism and processes we understand very little about and are nowhere near being able to model.

This is only a suggestion that is partially backed up by the weaker energy closure at the Manaus and Jaru sites. We added a sentence about this.

P1309: L1: what is the "CTL experiment"?

Sorry, CTL simulation, corrected.

L17: I don't understand what you mean by "The scores". L18: RSD acronym used without explanation.

Statistical scores, RSD = relative standard deviation. Corrected in the manuscript.

L25: Can you give an explanation as to why you think the biases vary from site to site. You say about eddyflux errors below but do not actually directly say that it is linked to this issue. Also are there any modelling biases which you would expect to vary by site?

We rewrote this part. The NEE biases vary from site to site mainly because NEE is a small flux resulting from the difference of 2 large ones (Reco - GPP). It is important to evaluate modeled NEE to observations since it is the flux actually measured but not much can be said from small differences between sites.

P1310: L3: Data can, and should be filtered for U*. Did you do this or test this affect?

The fluxtower data used in this analysis are indeed filtered depending on u*. The references of the data are given in table 1. As many papers advice to take the u* filtered data we didn't test the effect of the filtration.

L12: You state 330 TC ha-1 but you do not discuss any of the errors on this and similar numbers in Fig 8.

There are no errors on any of your observed values. Observed data should not be used extensively without considering its error, particularly as some of the errors stated in Malhi et al., 2009 are substantial. You are right. We added rough error estimates (from Malhi et al paper) in the text.

P1311: L3-4: Can you reference a figure or quantify the underestimation of Rh and the Rh C stock?

Yes Fig. 9 (reference added in the text)

P1312: L13-16: This has not been discussed in the text directly and it seems odd to introduce this in the conclusion.

Ok, to make the manuscript easier to read we moved this paragraph at the end of the results and reworded the conclusion.

Figure 3: I do not see any blue PS line on here. I am assuming it is under the PS+R line. If so maybe show a single line and note that the responses are identical. Also on this and all other figures can you add a letter in the panels (e.g. a., b.) as in the text it would be much easier to follow. I also do not really understand how you did your re-scaling or what the bottom panel is showing. Is it averaged or max SWC over a 10m-2 area or a 2m-2 area?

Ok, we modified the figure and the legend (where we better explained the rescaling : "We linearly rescaled the soil moisture content of the 10 m pit (Bruno et al., 2006) to the values of the 2 m one (da Rocha et al., 2004) by multiplying the 10m SWC by the ratio of field capacities between the 2m and the 10m pit)"

Figure 4: RN acronym used without explanation in legend, but then R Net used in the Figure.

Indeed, corrected.

Figure 5: Standard deviation should still have units associated with it and if not there should be an indication that the data has been normalised in the Figure legend. Also I would suggest labeling what the lines in the Taylor plots indicate, as there is a lot of information in these plots. Finally you do not mention the period over which the comparison is made in the figure.

Ok, see legend.

Figure 6: Please put the whole figure legend in and not a reference to another figure. Ok.

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