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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.

Anonymous Referee #1

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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.

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. 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.

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.

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.

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.

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.

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?

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

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

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?

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.

P1308:

L6: Acronym SD used and not explained.

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.

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

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

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

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

P1309:

L1: what is the “CTL experiment”?

L17: I don’t understand what you mean by “The scores”.

L18: RSD acronym used without explanation.

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?

P1310:

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

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.

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

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

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

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.

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

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