Geosci. Model Dev. Discuss., 7, C3000–C3003, 2015
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Geoscientific Model Development Discussions
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Interactive comment on "Modelling climate change responses in tropical forests: similar productivity estimates across five models, but different mechanisms and responses" *by* L. Rowland et al.

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Received and published: 20 January 2015

Review of : Modelling climate change responses in tropical forests: similar productivity estimates across five models, but different mechanisms and responses. By : Rowland et al. Published in GMDD, 7, 7823–7859, 2014 www.geosci-model-dev-discuss.net/7/7823/2014/

Dear editor, dear authors,

First I prefer not to remain anonymous. I am Nicolas Delbart.

This manuscript presents an inter-comparison study of the sensitivity of several outputs from five land surface models to temperature and precipitation changes in Amazonian broadleaved forests. Canopy scale outputs are gross primary productivity (GPP), net ecosystem productivity (NEP), ecosystem respiration (Reco), leaf area index (LAI). Leaf scale outputs are carbon assimilation (An) and stomatal resistance (gs). Thus the objective of the manuscript is to compare the model sensitivity to temperature and precipitation changes at these two scales of analysis. Main finding can be summarized as models disagree on the sensitivity of the leaf scale processes, but that at the canopy scale the inter model agreement increases mostly because models also disagree on LAI which compensates other disagreements. Even at the canopy scale model, the models disagree on the relative sensitivity to drought and to warming.

The amount of modelling work is very impressive. The figures are generally clear. The text is well written although I find it very often difficult to follow, but I think this is linked to the high richness of the results that are difficult to comment within a reasonable length. Being not a native speaker I am not able to judge the quality of English.

I will develop later some minor comments on the clarity of the text, but I would like first to make some more general comments.

My main comment is about the experimental setup. To my understanding all these models, maybe except ED2, are steady state equilibrium models. However, the experiments carried out in this manuscript consist in rising temperature dramatically and abruptly, or dropping the precipitation, after the model spin-up. Then the model is run for eight years. I am not a model expert but it seems to me these models are not designed to be able to respond adequately in a short term to such disturbances. Thus I am afraid that the model outputs that are presented only reflect how a model adjusts itself during a transition period to a new and totally different climatic situation. I think the models are here used outside of what they are built to do. I think it would be more adequate to impose a temperature increase ramp after the spin-up, or at least run the models much longer than eight years. Moreover that may be more informative on real ecosystem response as it would be closer to realistic climatic changes that do not consist in such abrupt changes. I am not saying the authors should redo the experiments but they must explain very precisely why steady-state equilibrium models can be used in such a way.

My second comment concerns the conclusions that the authors should give. If it is found that the models only really agree on NEP, less on GPP, and disagree on all processes, it is probably necessary to conclude that despite their complexity these models do not present a clear advantage over simpler models such as light use models or statistical models adjusted on existing ecosystem exchange measurements. Complex models are useful if they allow understanding the mechanisms behind canopy scale measurements, but here we see the models do not bring this knowledge. Thus, the authors should bring a general conclusion on the utility of complex models at their current stage of development to address the question of changes in Amazonian forests in response to climatic variability.

My third comment is that it seems to me that ecosystem response to such large changes (+6°C) should be treated with the scope of plant functional type changes, as a transition from forest to savanna should be expected. Except on page 7834 line 12 this crucial question is not addressed, and must be developed.

My final general comment is about the simulation of respiration. As the inter-model agreement is higher on NEP than on GPP, it is necessary to develop the changes on respiration, and maybe to separate heterotrophic and autotrophic respiration responses. It is commented but not shown, and maybe this is a good option to keep the manuscript in a reasonable length but still these results should be a bit more developed. We also need to know how the models differ in term of both respiration fluxes right after spin-up, and thus the biomass and the soil carbon should also be given to understand the initial differences between the different model simulations.

Minor comments

Page 7825 Lines 8-10 and 16-18 comment results on GPP and should be grouped.

21 : maybe remove “to” 23-26 : this a key issue. As said page 7837, lines 23-25, uncertainties on LAI are compensated by uncertainties on leaf scale processes. May it be possible that this is explained by the fact that the main source of validation data is canopy scale exchanges measurements? Moreover you point the lack of data later (page 7827), thus which validation strategy are you suggesting here? Page 7826, line 23. Meaning of SWC should be given first. Page 7829 lines 14 and 19 : same information, should be reorganized. Page 7834, figure 3. It would be cleared to me to see the LAI changes expressed in LAI units rather than in initial LAI fraction. Moreover, in figure 3, what is shown is not the fraction of change but the fraction that does not change, or am I wrong? Page 7835 : the text here is very complicated, whereas the figure 4 that it described is very clear. I think the manuscript would gain in clarity if the results were described less intensively. Same comment applies elsewhere in the manuscript. Page 7837 (lines 1-5) and figure 8 : why only two models are shown? Figure 1 : unclear. What mean signs + and - ? Is it the response of models? Why temperature increase induces an increase in GPP whereas in figure 2 we see the contrary? Figure 5 : should be expressed in the units of Vcmax, not relatively to 25°C. Table 3, caption : unclear. I apologize but interpretation of figures 6 and 7 are unclear to me.

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