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Comment

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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Dear Nicolas Delbart,

Firstly we would like to thank you for taking the time to review this manuscript. The comments you provided on our manuscript have been very insightful and we believe they have contributed substantially to improving the quality of this manuscript. In the following pages we provide responses to the comments made on the manuscript. We hope that our responses have dealt with all the issues raised in the review process, however please do not hesitate to contact us if you require any further clarification. Yours sincerely, Dr. Rowland & Co-authors

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Discussion Paper



Response to major and minor comments from reviewer Nicolas Delbart. (Please note that responses by the authors are shown in red and that our page numbers references are from the revised MS word document)

Major comments: 1. 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.

We agree that many of these models are designed to only be run in steady state equilibrium conditions. The methodology used for the spin up and model simulations was pre-constrained by a method which tested the capacity to simulate the responses of two experimental drought (see Powell et al 2013), where the forest was indeed exposed to an abrupt change in precipitation, in the model and in the field-scale experiments from which the test data were derived. Working within the existing simulation framework of our modelling consortium our study therefore focuses on model responses to short term shifts in temperature and precipitation at a tropical forest site. We agree that it would be interesting to study a slow ramp-up in temperature as well as a more abrupt change as modelled here. However, we note that there is increasing evidence that

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[Interactive Discussion](#)

[Discussion Paper](#)



this region will experience increasingly severe short-term changes in climate (Cox et al., 2008; Reichstein et al., 2013), and that the Amazon region has already experienced two abrupt and severe drought events in the last decade (Marengo et al., 2011). Hence, whilst testing against a slow ramp up is of much interest, we do still believe that there is significant value in analysing these short-term responses. Secondly analysing short-term responses is also important as much of the physiology has a fast response timescale, even if (much less well understood) components such as allocation and acclimation may operate over longer timescales. Many of the rapid physiological response mechanisms are dependent on temperature response functions; examples of these which are a key focus of our paper are photosynthetic responses or shifts in VPD which change stomatal conductance (see Figure 1). If we studied these processes after running the models to equilibrium, the responses of some of the key variables in the study (A_n , g_s) are more likely to be dominated by the effect of long term soil drying rather than any direct temperature responses, particularly at the highest temperature changes in the models which simulate a soil water stress function. We acknowledge and discuss in the paper that even at our shorter time-scale the model responses are bound up into responses of the change in soil moisture stress (Lines 383-395), however within the shorter-term time-scales used we argue there is greater scope to look at the shorter-term responses to changes in temperature. Thirdly because of the responses to variables such as the soil water stress function, if run to equilibrium with the temperature scenarios imposed, many of these models would have shifted from a tropical forest ecosystem to grassland, as CLM3.5 did even over a short time-scale. As discussed below (comment 3) these responses are indeed interesting, but unfortunately not the focus of this paper. Fourthly analysing data from short-term model simulations allowed us to compare model outputs with the Doughty and Goulden data which looked at short-term instantaneous responses of leaves to increased temperature; this validation/test is particularly valuable because there are very few other datasets beyond Doughty and Goulden focusing on this key issue. For these reasons above we would argue that this and other studies (e.g. Luo et al., 2008) which explore the short-term

responses of Amazonian forest to changes in climate are extremely valuable. However, we agree with the reviewer that we have not made these arguments clear enough in the manuscript and have endeavored to insert the sentences below to amend this:

Lines 31-35: "However, significant uncertainty remains regarding the response of tropical forests to warming (Corlett, 2011; Reed et al., 2012; Wood et al., 2012), altered precipitation (Meir et al., 2008; Meir and Woodward, 2010) and short-term abrupt changes in in both precipitation and temperature (Cox et al., 2008)."

Lines 44-48: "However model responses to simultaneous changes in precipitation and temperature complex are difficult to evaluate due to the compound effect of drought and temperature responses (Luo et al., 2008). There are particular challenges when considering short-to-medium term responses (Luo et al., 2008) linked to climatic extremes, such as severe drought (Cox et al., 2008; Marengo et al., 2011)."

Lines 87-93: "Our model simulations represent short-term non-equilibrium responses to changes in temperature to make them comparable to the perturbation data collected by Doughty and Goulden (2008). Evaluation of non-equilibrium changes in models is valuable for assessing how models will perform when simulating responses to extreme shifts in temperature and precipitation, which are predicted to increase in frequency and severity across Amazonia (Cox et al., 2008; Marengo et al., 2011). If the models were run to their equilibrium response to a simulated climate shift, the changes in some of the key variables in the study (A_n , g_s) are more likely to be dominated by the effect of long term soil drying rather than direct temperature responses."

Lines 351-354: "Had the models been run to their equilibrium states, it is likely that there would have been greater divergence of model responses at both the canopy- and leaf-scales. Prolonged higher temperatures reduce long-term soil moisture availability and cause more severe changes in β ; in dynamic-PFT models this can result in a substantial shift of PFT away from tropical forest."

2. My second comment concerns the conclusions that the authors should give. If

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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.

Thank you for raising this point; it is very interesting and something which should be discussed in the manuscript. Model development often advances by using different approaches – simpler (statistical or optimized) and complex. Development of more mechanistic processes should ultimately lead to improvements in our ability to simulate and understand these complex natural systems. We acknowledge that mechanistic understanding is always limited, hence it is certainly very interesting to test if statistical models or simplified ‘optimised’ models, which may have greater random, but less systematic error, will give a more informative prediction than detailed complex models. This discussion is relevant to many aspects of gross ecosystem process simulation. We hope that we have adequately addressed this issue by introducing a sentence in line 436–442 of the discussion which states: “The range of model responses in this study is likely to stem from real uncertainty in our understanding of the responses by tropical rain forest ecosystems to changes in precipitation and temperature. Further analysis of the same questions using models that vary in complexity (eg, statistical or optimized models, as well as purely mechanistic) might provide additional insight into mechanistic and simulation bias (systematic or random), as well as advancing understanding about climate risk that we derive from them (Meir, Mencuccini and Dewar, 2015)

3. My third comment is that it seems to me that ecosystem response to such large changes (+6âC) should be treated with the scope of plant functional type changes,

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as a transition from forest to savannah should be expected. Except on page 7834 line 12 this crucial question is not addressed, and must be developed.

We agree that the potential transitions from forest to grassland are very important, especially in the context of making ecosystem scale predictions. However there are other manuscripts currently in prep from the same modelling consortium which will discuss the implications of PFT shifts in model simulations done across the Amazon. The focus of this paper was assessing the effects of climate extremes on physiology in tropical forests, particularly in relation to the data comparison aspect of the manuscript. Therefore we did not address the issue of long-term adaptation and forest transitions, as this involves assessment of the competition matrix within the models, which would introduce a new and complex topic into an already complex and fairly long paper. Finally two of the models used in this study do not have multiple PFT's and comparing the responses of these models, to models which do have competing PFT's becomes impossible if PFT shifts occur.

4. 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.

We agree that the responses of autotrophic and heterotrophic respiration are both very important and that a single graph of total respiration is insufficient to deal with the complexities of this problem. However, the purpose of the respiration figure is not to provide an answer to the responses of respiration fluxes to temperature and drought, but to show the other half of the NEE flux, so the reader can understand what proportion of the NEE response is driven by GPP versus respiration. Although we would be keen

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



to explore the complexities of respiration in detail, this paper is necessarily focused on the gross primary productivity response to climate extremes. Respiration responses are beyond the scope of our paper, which is already relatively complex in addressing leaf to canopy assimilation processes. In terms of how carbon stocks and fluxes differ between models at this site following spin up, when no climate changes have been imposed, and after drought is imposed, this has already been addressed and published by Powell et al. (2013).

Minor Comments: 1. Page 7825 Lines 8-10 and 16-18 comment results on GPP and should be grouped.

Thank you for pointing this out, now grouped.

2. 21 : maybe remove “to”.

Done

3. 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?

Thank you for your comment, we have changed the sentence to make it clear that it is validation at both the leaf and canopy scale which is necessary: “To improve the reliability of simulations of the response of Amazonian rainforest to climate change the mechanistic underpinnings of vegetation models need to be validated at both leaf- and canopy-scales to improve accuracy and consistency in the quantification of processes within and across an ecosystem. .

4. Page 7826, line 23. Meaning of SWC should be given first.

Thank you, now done

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5. Page 7829 lines 14 and 19: same information, should be reorganized.

Now re-organised so similar sentences are concatenated

6. 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?

Thank you for this comment, we agree that the way we have described the fractional changes in the figure legend is maybe confusing and therefore we have changed the figure legend to read: "Figure 3: Modelled effect of short-term changes in temperature and drought. Changes in: a) gross primary productivity (GPP) b) ecosystem respiration (Reco) and c) leaf area index (LAI) in the final year (2006) in the drought run expressed as a fraction of the value in the final year (2006) of the control run, for the Tair -5°C (grey bars) and Tair +6°C (White bars) simulations." However, if we were to express LAI in the same figure as LAI units this would be misleading because of the large variation in LAI across the models. For example, if the absolute reductions in LAI from ED2 and CLM3.5 are equivalent in the figure (e.g. they both lose 2m² m⁻² of LAI), the implications are very different. ED2 has a starting LAI of 4 m² m⁻² and thus predicts a 50% loss. Meanwhile, CLM3.5 starts at 11 m² m⁻² and predicts an 18% loss. We believe that the reader therefore gets more information from seeing LAI as a fraction after having already seen the values of LAI between the models in Figure 2.

7. 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.

We agree that this section and other results sections were unclear; we have completely re-written the results sections in an attempt to simplify them and make them clearer.

8. Page 7837 (lines 1-5) and figure 8: why only two models are shown?

As stated in the Methods the data from Doughty and Goulden is only available for sunlit

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



leaves and SPA and CLM3.5 are the only models which simulate an output for sunlit leaves. We have attempted to make this clearer in the Figure legend and in the text in lines 317-319 stating: “When the effect of soil water stress is removed and sunlit leaf level values are compared to the DG data for the models which could output separate sunlit leaf values of g_s and A_n (only SPA and CLM3.5; Figure 8).”

9. 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?

The + and – signs represent the correlation or possible feedbacks between variables. For example A_n is only positively correlated with GPP, it has a +, (i.e. if A_n goes up GPP cannot go down, with all other things being equal). Some variable can have both positive and negative feedbacks depending on the magnitude of the change and hence they have both a +/- sign. We have now made this clearer in the legend: “Figure 1: Schematic diagram showing how droughts, via the combined effects of increased air temperature (T) and reductions in precipitation (PPT), affect the carbon cycle of a tropical forest, including the effects on: vapour pressure deficit (VPD), evapo-transpiration (E_t), stomatal conductance (g_s), soil water content (SWC), net photosynthesis (A_n), leaf area index (LAI), the maximum rates of RuBP carboxylation and electron transport (V_{cmax} and J_{max} respectively), autotrophic respiration (R_a) heterotrophic respiration (R_h), gross primary productivity (GPP), and net ecosystem exchange (NEE). + signs indicates a positive feedback effect between variables (i.e. an increase in one variable can only cause an increase in another if all else is equal), - signs indicate a negative feedback effect, and +/- indicate the possibility of both a positive and negative effect. Solid arrows represent responses which occur over short timescales of minutes to hours, whereas dashes arrows represent responses which can occur over longer timescales from days to months.”

10. Figure 5: should be expressed in the units of V_{cmax} , not relatively to 25°C .

[Full Screen / Esc](#)
[Printer-friendly Version](#)
[Interactive Discussion](#)
[Discussion Paper](#)


The key point of Figure 5 within our paper is to show the relative responses of V_{cmax} to temperature change. We argue that this is much easier to see if you explore the shapes of the temperature response curves on a normalised scale, where the response curve can be directly compared across models. Within the models V_{cmax} is the only variable explored which has a fixed temperature response independent of other climate variations, for example VPD. If we were to plot the actual values the resulting figure would be very confusing because some of the models output V_{cmax} values for the top canopy only, before canopy integration occurs, and other models output an integration of V_{cmax} across the canopy, or different V_{cmax} values from multiple canopy levels. Consequently the absolute values for V_{cmax} across the models are not comparable whereas the temperature responses on V_{cmax} are. If necessary we can clarify this point in the Figure legend.

11. Table 3, caption : unclear.

This caption has now been re-written, to what we hope is a much clearer format: “Table 3: Values show the normalised intrinsic water use efficiency (IWUE) calculated as the linear slope of normalised A_n plotted against normalised g_s (Figure 6). The normalised IWUE is calculated separately for each models’ control and drought temperature simulations (ambient air temperature (T_{air}) -5°C, +0°C, +2°C, +4°C, and +6°C).”

12. I apologize but interpretation of figures 6 and 7 are unclear to me. Thank you for pointing out that the interpretation of Figures 6 and 7 are not 100% clear.

The purpose of Figures 6 and 7 is: firstly to demonstrate that there are clear relationships between A_n and g_s in the models, but not between A_n and V_{cmax} . We hope that this is now clearer in lines 299-301: “Consequently for each model there are apparent, but variable, relationships between g_s and A_n (Figure 6), but no obvious relationships between A_n and V_{cmax} (Figure 7).” Figure 6 is also used to show the normalised intrinsic water use efficiency: the normalised increase in A_n per unit increase in nor-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

malised gs. The purpose of this plot is to show that the rate at which normalised An increases per unit increase in normalised gs (i.e. the linear slope) increases from the lowest temperature simulation (ambient air temperature $-5\text{ }^{\circ}\text{C}$) to the highest temperature simulation (ambient air temperature $+6\text{ }^{\circ}\text{C}$) and this increase is greater in the drought than control simulations. The normalised values of intrinsic water use efficiency per simulation (the linear slope values) are shown in Table 3 The results and discussion sections in this manuscript which include the explanation of the results from these figures have now all been re-written (see comment above), as have the figure captions. We hope now that the figures and results concerning these figures are now made much clearer throughout the manuscript.

New references added to text and manuscript: 1.Cox, P. M., Harris, P. P., Huntingford, C., Betts, R. A., Collins, M., Jones, C. D., Jupp, T. E., Marengo, J. A., and Nobre, C. A.: Increasing risk of Amazonian drought due to decreasing aerosol pollution, *Nature*, 453, 212-215, 2008. doi:10.1038/nature06960 2.da Costa, A. C. L., Galbraith, D., Almeida, S., Portela, B. T. T., da Costa, M., Silva, J. D., Braga, A. P., de Goncalves, P. H. L., de Oliveira, A. A. R., Fisher, R., Phillips, O. L., Metcalfe, D. B., Levy, P., and Meir, P.: Effect of 7 yr of experimental drought on vegetation dynamics and biomass storage of an eastern Amazonian rainforest, *New Phytol*, 187, 579-591, 2010. doi: 10.1111/j.1469-8137.2010.03309 3.Gatti, L. V., Gloor, M., Miller, J. B., Doughty, C. E., Malhi, Y., Domingues, L. G., Basso, L. S., Martinewski, A., Correia, C. S. C., Borges, V. F., Freitas, S., Braz, R., Anderson, L. O., Rocha, H., Grace, J., Phillips, O. L., and Lloyd, J.: Drought sensitivity of Amazonian carbon balance revealed by atmospheric measurements, *Nature*, 506, 76, 2014. doi:10.1038/Nature12957 4.Marengo, J. A., J. Tomasella, L. M. Alves, W. R. Soares, and D. A. Rodriguez.: The drought of 2010 in the context of historical droughts in the Amazon region, *Geophys. Res. Lett.*, 38, L12703, doi:10.1029/2011GL047436. doi:10.1029/2011GL047436. 5.Meir, P., Mencuccini. M., and Dewar. R. C. Tree mortality during drought: narrowing the gaps in understanding and prediction, *New Phytologist*, in press. 6.Reichstein, M., Bahn, M., Ciais, P., Frank, D., Mahecha, M.D., Seneviratne, S.I., Zscheischler, J., Beer, C., Buchmann, N., Frank,

C3360

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Discussion Paper

C3361

