Dear Biogeosciences Editors,

I am writing in regards to our submission of the revised manuscript "Forest response to increased disturbance in the Central Amazon and comparison to Western Amazonian forests". We have addressed the reviewer's comments and have made all necessary revisions to the manuscript based on the reviewer's suggestions. We would like to sincerely thank the reviewers for taking the time and effort in reviewing our manuscript. They brought forward several issues that need clarification, most of which require minor changes in the manuscript. We have incorporated these comments and hope that the changes we are suggesting are sufficient. The changes listed below have been incorporated into a final version of the manuscript, which we hope will be accepted for publication.

Foremost, we agree with the comments by the anonymous referees.

We agree that the introduction is too long and it is hard to follow. Therefore the introduction has been decreased by about 20% and we have made it concise and less redundant. We have restructured the introduction so that the motivation for this study is clearer. In addition to shortening the introduction, we also moved around some paragraphs and deleted one of the main research questions at the end of the introduction because it was only indirectly addressed in the discussion and not a major question.

Prior to the discussion section of the manuscript we have now tried to make it clear why this study is using 2 different models. The reason for including two models in this study is because one is a benchmark model (ZELIG-TROP) for the other (CLM). CLM-CN is a widely used, more general model while ZELIG-TROP is a more specific, detailed, individually based model that looks at fine scale forest dynamics. We wanted to use ZELIG-TROP as a comparison model to see how well CLM-CN captures disturbance-recovery processes, and changes in carbon sources and sinks. In the end, both models essentially fail at capturing the response to elevated disturbance, or that they get the right answer for the wrong reason. Also CLM does at poor job at capturing inter-annual variability in carbon stocks and fluxes because this version does not have dynamic vegetation.

The reviewer's comments on wood density gradients across the Amazon Basin were very informative and addressed. After reviewing the literature we have included two references finding that wood density has also been found to be high in northern Peru. We welcome suggestions on any new emerging literature that finds high wood density in the western Amazon, and will read this literature and consider including it. We have edited the manuscript to say that trends typically find higher wood density in the central amazon, and lower wood density in the western, southern amazon, but there is not a clear gradient (as there are outliers in multiple locations across the Basin).

In the second paragraph of the discussion, upon suggestion of the reviewer, we bring our findings that neither basal area nor LAI are drivers of patterns in biomass more to the forefront. We still assert that wood density is a contributor to differences in biomass, but the models are failing because they attribute the reduced biomass to basal area and LAI, which is not supported in the literature. We believe our findings are also significant because the model predicted no significant difference in wood density with treatment (in fact it increased slightly with elevated disturbance), yet there was a significant reduction in biomass - therefore the model failed again.

Response to section 4.1.1 - we agree with the reviewer's comments and this section has been edited accordingly. First, we have changed the title of the section so that the main focus is not CO2 fertilization, but instead drawing more attention to disturbance and biomass accumulation. We have removed language that describes CO2 fertilization as a fact or known, but rather it is a possibility to be considered. For example, we have removed the sentence that stated that there is "causal evidence" that increase in biomass is caused by CO2 fertilization, as it is overly speculative. Instead, we simply state that biomass has been increasing in the Amazon, which has been found in studies (Phillips et al. 1998, 2008), but we do not infer the cause. In the initial submission to BG (before this manuscript went into publication in BGD and went into the interactive discussion), the assigned editor requested that more modeling studies on CO2 fertilization be discussed in section 4.1.1. Specifically modeling studies looking at the role of atmospheric CO2 and biomass change, so this literature has not been removed. While editing and fine tuning this section, based on the reviewers comments, we also noticed that a few references where in the wrong place and did not back-up the claims made in those sentences. For example Canadell et al. 2007 and Lewis et al. 2009. These have either been deleted or moved to the correct location in this section. Lastly, we provide additional references that manipulation experiments of enhanced CO2 in the tropics are untested.

Additional response to discussion comment - we agree that the manuscript would benefit from a final paragraph that communicates the key findings. We have added in a final paragraph summarizing the key results. But in order not to be redundant in the discussion we moved sentences from the original third paragraph in the discussion to go into this final, concluding statement. These key findings were originally misplaced in the discussion and would be stronger at the end.

Specific responses to specific comments -

- Reference included for Line 25-27, page 7729, and more information has been provided. (Laurance et al. 2004)

- We agree the subtitle “Calibration methods” is misleading and has been changed to Verification Methods.

- Line 27, page 7733. A reference has been included to confirm that the canopy layer (vs. subcanopy or emergent layer) is a dense area of biodiversity.

- Units have been provided in Table 1.

- The suggestions for Figure 2 make sense, and it might make sense to reformat and group the species belonging to the same growth form. This suggestion was taken into consideration and was tested. We did reformat the figure so that the same growth forms were grouped together in a trial figure, but we do not think this made a substantial difference. However we would very much like to hear from others if they agree with this reformatting.
Originally the species were listed in order of contributing the most to basal area (at bottom) then stacked as contributing the least to basal area, in that order. Even though the growth forms are discontinuous, we believe this still shows a strong representation of the effect of disturbance. Figure 2b in its original version is able to show that the dominant emergent species is not longer dominant, and in its place (as a result of disturbance) canopy level species have filled in. Species that contribute the least to basal area are found at the top of the stacked figure. The transparency of the "red" canopy species at the top of the figure was used so that the other growth forms could still be seen.

We appreciate the thoughtful comments and reviews by the two referees, and think the paper is stronger as a result.

Thank you for your consideration,
Jennifer Holm