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Comment

Interactive comment on “Forest response to increased disturbance in the Central Amazon and comparison to Western Amazonian forests” by J. A. Holm et al.

J. A. Holm et al.

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Dear Reviewer 2, We would like to thank you sincerely for taking the time and effort in reviewing our manuscript. You have brought forward several issues that need clarification, most of which require minor changes in the manuscript. We have incorporated your 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 referee #2.

We agree that the introduction is too long and it is hard to follow. Therefore the in-

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roduction 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 more clear. In addition to shortening the introduction, we also moved around some paragraphs and deleted one of my 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 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.

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 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 makes sense, and it might make sense to reformat

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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 Referee #2, and think the paper is stronger as a result.

Interactive comment on Biogeosciences Discuss., 11, 7721, 2014.

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