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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.

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Dear Reviewer 1, 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 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 #1.

We agree that the introduction is too long and repetitive. This was also a concern of

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reviewer #2. Therefore the introduction has been decreased by about 20% and we have made it concise and less repetitive. 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.

The reviewers 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 or 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

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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 were 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 is untested.

Response to section 4.2.2 - we agree that our communication of describing variation in wood density was too narrowly focus on smaller scale, species-specific variation in wood density. This manuscript is more concerned with larger scale variation across the whole Amazon Basin, so this section needed to be re-worded so that a regional scale theme was maintained.

Response to additional 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 section 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.

We have done our best effort to locate and correct all typographical errors and grammar mistakes. We hope that the revised manuscript is clearer because of this.

We appreciate the thoughtful comments and reviews by the Referee #1, and think the

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paper is stronger as a result.

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

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