Final Decision of bg-2015-137

Title Influence of tree size, taxonomy, and edaphic conditions on heart rot in mixed-dipterocarp Bornean rainforests: implications for aboveground biomass estimates

Authors K. D. Heineman, S. E. Russo, I.C. Baillie, J. D. Mamit, P. P-K. Chai, L. Chai, E. W. Hindley, L. Buong Ting, P. S. Ashton, and S. Tan

Dear Dr. Heineman,

Both reviewers provided a detailed and substantial evaluation of bg-2015-137. The reviewers highlight the scientific importance of the presented dataset and the novel character of this contribution, however, indicate that there is potential to improve the scientific quality especially in the material and method section, analysis and interpretation of the obtained results.

After reading all comments, critics and suggestions of both reviewers and evaluating your comments and how you will address the reviewer's suggestion in the revised version of the manuscript, I am pleased to accept your manuscript for publication in *Biogeosciences* after a minor revision considering the following remarks:

Please provide the requested information pointed out by both reviewers on the drilling methodology (point 1 of your interactive comment) as far as this information is available.

Points 2 & 8 of interactive comment: I agree with reviewer 1 that a table providing data on species-specific wood rot frequency and severity would be an important information and I support that this will be added in form of a table in the supplementary material together with Table R1 which provides information on the level of taxonomic identification of the tree species in the different data sets which addresses the comment of reviewer 1.

Further I support your suggestion to emphasize more on the species-specific effects as suggested by reviewer 1 at point 3 and to include further analysis of variance considering the relative importance of different taxonomic levels on wood rot frequency and severity. Further I agree with your suggested modification indicated in your response to point 3 on page C3944 of your interactive comment.

Points 4, 9 and 31: The information included in the revised table 1 addresses sufficiently the comments raised by both reviewers showing the number of selected tree species and individuals from each dataset for analyses. You should explicitly mention in the corresponding material and method section of the revised version that trees without species-level covariates (wood density or soil habitat association) and species-level identification were not included in the GLMMs.

Point 5: I agree to provide more information in the revised manuscript on the comment raised by reviewer 1 on species selection for logging as this may influence the results. This was also a major concern of reviewer 2 which is now well addressed in your response (page C3946 of your interactive comment).

Points 6 & 29: Both reviewers claim that your data only evaluate stem biomass. Therefore it is important that you clearly state that the estimates refer to stem biomass and not to the whole tree or aboveground biomass since data on wood rot of roots and branches are not available and extrapolation to the aboveground wood biomass are complicated due to varying crown architectures.

Points 7 & 24: I think that your modification of the sampling description in section 2.2 now sufficiently responds the critics raised by both reviewers.

Point 9: Please clearly state in this section that in the three datasets only trees from identified species were included in the GLMMs.

Points 10 & 26: Maybe it would be appropriate to cite some relevant studies of P. Ashton (listed in the interactive comment) and Baillie et al. (1987) (page 10, lines 5-6) when saying “for species not included in these studies, natural history data and personal observations (by P. S. Ashton) were used.

Point 11: Adding the information on soils in both study regions (Table R2) in the supplements will improve the understanding of soil classification (see also comments of reviewer 2). You should discuss that due to under-representation of large trees in the upper diameter classes probably no significant difference in stem rot frequency was observed between soil types in the Lambir region.

Point 12: The selected sequence to present information on analysis of individuals (section 2.6) before analysis on the stand-level (section 2.7) makes sense for me.

Point 13: ok.

Point 14: You should indicate more clearly in section 2.2 describing the methodology of coring that sampling was done only at one point, while drilling was performed at two points on the stem (the reader might miss this information). Further you should include in the discussion that the discrepancy in estimating the frequency stem rot, despite the methodological problems, might be also related to the diameter distribution of the trees from the two datasets. Therefore I suggest to include Figure R2 as supplementary information highlighting the diameter distribution of trees from the three sample sets.

Point 15: Also this point should be clearly addressed in the discussion referring to figure R2.

Points 16 & 21: As pointed out earlier, the selection criteria are now much better described and documented. The diameter distribution of the trees used for felling and drilling is quite similar and includes even more trees in the upper diameter classes selected for drilling compared to those for felling (Figure R2). The methodological problems of detecting small amounts of rot between felling and drilling should be mentioned in the discussion to explain part of the differences on wood density's impact on the presence of stem rot. This might be also due to methodological differences to observe stem rot which was done on both ends of the stem of felled trees, but only at height of DBH on drilled trees (see point 21 raised by reviewer 2).

Points 17-20: ok.

Point 22 & 23: sufficient information is now provided.

Point 25: I agree with your comment.

Point 27: Please report in the revised version the year of collection of the drilling and felling data.

Point 28: I think you answered sufficiently the critics of reviewer 2, but I feel it is necessary to discuss in a revised version biases of overestimates which can be caused by plot designs or over-sampling large trees (see figure R2)

Point 30: This information should be presented in the supplement of the revised version.

Point 32: These analyses should be included in the revised version and in the corresponding figures and tables.

Point 33: The fitted relationships for probability of stem rot vs. diameter in the three datasets should be presented as supplementary information.

Points 34-36: are sufficiently responded.

Point 37: Comparisons between your datasets and those from other tropical regions, especially the Neotropics, should be made with caution. So far only very few data sets are available and it is important to emphasize that more studies in the different tropical forests are needed.

Point 38: suggest to drop the sentence focusing on LIDAR-based estimates in the revised version.

Point 39: your comment sufficiently address the critic of reviewer 2.

Point 40: ok.

Please consider also the minor concerns and comment of reviewer 2 in the revised version.

In your revision, please make sure that you take full account of the comments of the referees and editor.

Thank you for submitting to *Biogeosciences*. We look forward to receiving the revision.

Best regards,

Jochen Schöngart