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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Influence of tree size, taxonomy, and edaphic conditions on heart rot in mixed-dipterocarp Bornean rainforests: implications for aboveground biomass estimates" *by* K. D. Heineman et al.

Anonymous Referee #2

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This article analyzes three datasets on heart rot in mixed dipterocarp Bornean rain forests to investigate variation in heart rot with respect to tree diameter, edaphic conditions or associations, and taxonomic identity, and discusses potential implications for estimations of tree biomass. Two datasets, from "drilling" of live trees and from felling a subset of these trees, were collected in or before 1974 in a systematic timber inventory of a large area, while one dataset from "coring" live trees was collected in 2009. The analyses find that heart rot frequency increases with tree diameters and on poorer soils, and varies extensively among tree species. Frequency and severity of heart rot

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Interactive Discussion

Discussion Paper

was high overall compared with previous studies, and varied across the landscape with soils. The results imply that spatial variation in heart rot could substantially contribute to spatial variation in tropical forest aboveground biomass and to spatial variation in errors in estimating this biomass. The large sample sizes of this study provide substantial potential insight into patterns of heart rot variation and their implications for biomass estimation, and the findings with respect to soil type are novel and particularly interesting. The manuscript currently falls short of its potential because it fails to report important details of the field methods, fails to weight trees by the differential sampling with respect to diameter in calculating plot scale statistics, and poorly motivates and incompletely describes statistical analyses, as detailed below.

The methods lack key information necessary for interpreting the results. First, interpretation of the reported frequencies of heartrot depend critically on exactly what part of the stem is investigated for heartrot, which is not clear for either the felling or drilling method. The drilling method description does not state the diameter of the drill hole, or the depth to which the log was drilled. And for the felling method, given that heart rot is examined in the ends of felled logs, it is critical to know the length to which logs were cut. Second, a clear description of the sampling designs for choosing trees is also necessary to interpret the results. This is given only for the drilling dataset. No sampling design is given for the coring dataset at all. For the felling datasaet, information on how candidate trees were selected is quite clear, but then it is noted that trees were excluded in the field for safety reason, and no information is given on the proportions of trees excluded. At the least it should be possible to state the total proportion excluded; ideally there would be numbers by reason for exclusion, size class, etc. Third, to interpret the soils results, it is important to know how the soil classification in the coring dataset relates to the soils measurements in the other datasets, and also to know how many missing values were interpolated in the latter dataset. Finally, the year of collection of the drilling and felling data should be reported. There could, after all, also be temporal shifts in heart rot frequency or severity with anthropogenic global change considering changes in climate, nutrient availability, etc.

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The calculation of the plot level proportion of stem biomass lost to heart rot fails to account for the sampling biases inherent in variable radius plot designs. Variable radius plots sample a higher area for large trees than for small trees, and given that large trees also have a higher proportion of heart rot, the current calculations will overstate the biomass lost to heart rot at the plot level. This bias can be entirely removed by using standard expansion factors based on the measured diameters of every tree, and this is what should be done. There also seems to be an inconsistency in how this loss is calculated. As it is currently done, this is really a calculation of the percent loss of merchantable stem biomass, as it only encompasses stem biomass from the base of the tree to the first branch. Given that the paper in general is more concerned with biomass, perhaps the statistic could be calculated for total tree biomass in addition to this calculation for merchantable stem biomass, although the measurements of loss are based exclusively on the merchantable stem biomass. If only stem biomass loss is calculated, then perhaps the statistic should be referred to somewhat differently.

The particular choice of statistical analyses to explain variation in heart rot among trees and plots was not motivated, and I'm not convinced the approach taken was the best. And regardless of the approach taken, the manuscript or supplemental materials should report the fitted coefficients of the best models: these coefficients embody a considerable amount of the knowledge gained from this study. The analysis at the tree level excludes species with 5 or fewer individuals, which has a big effect on sample size, as the number of analyzed samples is just 661 of 1035 felled trees, and 616 of 1780 drilled trees (comparing Table 1 with the methods text). This could easily bias the results if rare species differ systematically in heartrot frequency from common species, as is certainly plausible given their differential susceptibility to other pathogenic natural enemies. It's also not clear why the species with small sample sizes should be excluded at all, given that no fixed effects are estimated for species. In terms of the statistical approach, why not use multimodel inference to determine the best model, which seems like it will not include two of the soil PCA variables, etc.? It would be nice to see the fitted relationships for probability of heart rot vs. diameter in the three datasets

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at least in supplemental materials. The fitted model was apparently linear in dbh; is this functional form supported by examination of residuals? In terms of the plot-level analysis, why do pairwise analyses here, when the tree-level analyses were all multivariate? Why separately analyze correlations with six soil chemical variables as well as the four PCAs in the plot-level analysis? The whole idea of PCAs is to do these instead of the individual correlated variables; it seems inconsistent to do both PCAs and some of the individual variables. And why these 6 soil variables, anyway, out of the 30 variables that went into the PCA?

Finally, the interpretation of the results go beyond what can be supported. Here biomass "loss" to heartrot is calculated as the volume affected by heartrot, but this is a mix of rotted wood and void space, and the necromass and fungal biomass of rotted wood may approach the biomass of the healthy wood it replaces. As noted in the text, this complicates comparisons with other studies as this study includes all heartrot as lost, while others often look at empty volume only. Thus statements that the study here shows a rate higher than in neotropics based on comparison with neotropical studies that quantify loss as void space is not adequately supported. The discussion also suggests that the differential loss of biomass to heartrot in different sites would affect the quality of biomass estimates. This is certainly true for standard allometric equations. The text implies the problem would be even worse for LiDAR-based estimates (p. 6844). I see no basis for this claim, as lidar-based biomass estimates are typically calibrated against the same tree-based allometric equations used for plot-based estimates. This claim needs to be either justified or dropped.

Overall, there are a considerable number of typographic errors in the text, indicative of inadequate proof-reading. I report only a small subset of these below under minor comments.

Other minor comments

P6823. 16-17. Wouldn't it be more logical to give the smaller number for smaller trees

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first?

P6824L21. Meaning unclear – is the reduction less than 33% or is the resulting timber yield <33%?

6824L27. Wording. Geographic variation in heart rot may influence AGB patterns and the errors associated with AGB estimates from LIDAR etc.; it does not influence the estimates themselves.

6825 – 16. Delete “the”

-25. Should be “fast-growing”

6826-3. change last “with” to “of”

-6 change “slower” to “more slowly”

-9. Change “contents” to “concentrations”

-7-9. Run-on sentence.

6827. Some of site description is in past tense, some in present. Be consistent.

6828 – 4. Delete “located” or add something after it.

Figure1. The Lambir CTFS plot as marked on the map looks like it is >10x20 km, which is clearly wrong. A bit unclear from the methods where exactly coring was done – not on the CTFS plot itself, right? If not, then in how large an area around it?

Figure 3. Caption statements about orientation do not match figure.

6839-14. typo “that” should be “than”.

6841 – 27-29. Text reads “Lower nitrogen concentrations in wood and soil may also cause wood-decay fungi on low fertility soils to excavate greater volume of wood to satisfy nutrient requirements (Boddy, 2001).” The logical complement of this would be to say that higher nitrogen concentrations in wood and soil cause wood-decay fungi

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to excavate less wood, i.e., to grow less – which makes no sense whatsover. In general higher resource levels leads to more proliferation of the organism consuming the resource, not less.

Table 2. I suggest using boldface or such to highlight significant terms.

Table 4 – A key piece of information is missing here. What is the total explained variation? At the least this should be given here for reference for each analysis. To me, it would be even more useful to normalize the listed percentages by this value, so that the table showed percent of total variation explained by each factor, rather than the percent of explained variation.

6837-21. I would think this should be stated as a percent of harvestable stem biomass or such. Graphs are all pairwise, yet analyses are multivariate. Fitted coefficients of models are never presented.

Interactive comment on Biogeosciences Discuss., 12, 6821, 2015.