

Interactive comment on “Spatial trends in leaf size of Amazonian rainforest trees” by A. C. M. Malhado et al.

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Overall this study constitutes a lot of work looking at leaf size distributions across Amazonian forests. It is generally well written and appropriately structured.

The results are somewhat negative, especially in terms of environmental correlations, although I do have concerns about whether the appropriate soil fertility metric has actually been applied. I am also concerned about some aspects of the statistics. These and several other relevant points are detailed in what follows:

Paragraph at the end of p2128, start of p2129: This comes across as a little contradictory; first we are told small leaves assist in the avoidance of heat stress and reductions in photosynthesis, then we are told under low nutrient conditions, we have small leaves

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because there is no point in having larger leaves because increased temperatures cannot benefit photosynthesis. In any case, is that really the whole story for the theory of smaller leaves under oligotrophic conditions. For example, is it also related to increased sclerophylly and increased structural defenses for the same C investment?

The statement of "increased water investment costs would therefore be expected to favour smaller leaves" might also be refined. Presumably, what is meant more here is "reduced water availability", unless the argument is higher root investment results in less C available for foliar development. In short, this paragraph needs a bit of improvement and refinement.

Last paragraph p2129: Perhaps here it should already be stated here that the general idea is that variation in leaf area per branch (or unit stem area) in predominately accountable for by variations in leaf size rather than leaf number. Indeed, it would be good to know just how much evidence there is for this assumption.

p2133, Section 2.3: Wouldn't maximum tree height (as in the RAINFOR database) be more appropriate here. Or are we assuming that for any given species leaf size changes systematically as trees grow taller?

p2134, section 2.4: As far as I know, this is the first time the "Pioneer Index" has been used. Perhaps it would be nice to give some indication of its validity. For example, a simple measure of agreement between the three assessors. Kendall's coefficient of concordance would be one possibility. I am also skeptical about applying standard parametric statistics to such an Index as seems to be done in Section 3.3.

p2135, last paragraph: The Quesada (2008) reference should now probably be

Quesada, C. A., Lloyd, J., Schwarz, M., Patiño, S., Baker, T. R., Czimczik, C. I., Fyllas, N. M., Martinelli, L. A., Nardoto, G. B., Schmerler, J., Santos, A. J. B., Hodnett, M. G., Herrera, R., Luizão, F. J., Arneith, A., Lloyd, G., Dezzeo, N., Hilke, I., Kuhlmann, I., Raessler, M., Moraes Filho, J., Paiva, R., Araujo Filho, R., Chaves, E., Cruz Junior, O.,

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Pimentel, T. P. and Paiva, R.: Chemical and physical properties of Amazonian forest soils in relation to their genesis, bgd-2008-0243 (or with Biogeosciences Discussion page numbers when available)

Also, why (on earth) just use sum of bases as the fertility indicator ? Other factors such as "available" P are just as valid, if not more valid fertility indicators. There are also the results from PCA ordinations which have been used with some success as predictors of other leaf properties (as for example in the Fyllas et al. paper in this special issue). A PCA ordination relating primarily to soil fertility is also in the Quesada paper cited above and would probably prove more informative as a predictor variable

p2136; Section 2.6: It is excellent that attempts have been made to account for spatial autocorrelation, but also what we are looking at here is essentially compositional data. Has some sort of transformation been made to account for this? And if not, then why not ?

p2137 and elsewhere: I don't think it excessively pedantic to point out that proportions vary between 0 and 1 and percentages between 0 and 100. Given that one of main advantages of Biogeosciences is that colour figures can be presented at no extra costs, I wonder in the clarity of Figure 2 and other bar charts could be enhanced using colour presentation.

p2141; Section 3.3 See my previous comments w.r.t. pioneer index

p2141; Section 3.4 I would certainly like to see the rationale for "controlling for dbh" better explained. If traits are co-ordinated, then why do this ? Also, see previous comments about appropriate soil fertility indices. Further, why not test for water availability and moisture effects jointly (and I would suggest perhaps using spearman's rho as is available in SAM? Why one earth should any of this be linear! ? Finally, what is the rationale for analysing within different regions as well as across the Basin as a whole Whatever the rationale, then certainly some correction for multiple testing surely has to be made.

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p2143: end top paragraph: It would be good to know how the leaf-size for tropical forests may compare to more temperate types

p2143; section 4.1:

Doesn't one saying that there are smaller leaves in the Guyana shield region just because of more Fabaceae just add up to saying leaf size is itself ecologically irrelevant ?

Is it also worth noting that ter Steege et al. (2006) did not correct for spatial correlation ? Perhaps their results would have been very different if they did?

Although not providing direct support for the work of Webb (1959) and Dolph and Dicter (1980) perhaps it would be good to make clear the magnitude of the precipitation gradients studied here relative to those studies (along with the absolute range) . If for example this study had extended to the dry deciduous Chaco forests of Bolivia (for which Killeen and colleagues have full species compositional data and floras from which leaf sizes could be inferred), then perhaps a gradient might would have indeed been found.

The Discussion would also benefit greatly with some consideration of soil nutrient effects (or lack thereof), again with reference to the range observed in Amazonia as compared with other studies (though in this case they are probably larger).

Interactive comment on Biogeosciences Discuss., 6, 2125, 2009.

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