**Biogeosciences
Discussions**

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Comment

Interactive comment on "Integrating regional and continental scale comparisons of tree composition in Amazonian terra firme forests" *by* E. N. Honorio Coronado et al.

Anonymous Referee #3

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General comments

Based on a substantive dataset, this manuscript aims for a better understanding of tree species distribution patterns along environmental gradients and different spatial scales in the Amazon basin. These issues are both so important for planning and conservation of tropical rainforests, since they would certainly help to identify opportunities and priorities for research and conservation. However, the paper raises concerns that require major revisions to be published. Firstly, it fails in analyze and discuss properly the subject announced, both in the title and in the introduction. Secondly, there are too many caveats in the way the methods employed could answer the research questions.

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Thirdly, the discussion, which was highly descriptive and biased on the floristic composition itself, focused only on the regional scale. Furthermore, in some cases it was either not in line with the results or focusing on subjects that were even not considered in the research questions. Each of these issues will be expanded below.

First, It fails in accomplishing with the expected subject announced in the title: I did not find neither from a methodological point of view nor from a conceptual framework in the discussion, any analytical integration of analyses based on regional and continental scales. Rather than looking for an integrative analysis, the manuscript focused on very descriptive comparisons that somehow could be already obtained from the existing literature. In addition, as it is acknowledge by the authors in the first statement of the discussion, almost nothing is said or done in trying to couple the results obtained at those different spatial scales.

Methods vs research questions: For the research questions (1) Does genus-level data give similar patterns of floristic composition as species-level information? The methods employed to answer this question based on PCoA and correlation analyses seem appropriate. However, PCoA analyses showed a clear "horseshoe effect" in the separate diagrams for species and genus, which proposes the need of other analytical approaches (see specific comments below). Addressing this question in both the Introduction and the Discussion, authors should have considered the work developed by Higgins & Ruokolainen (2004). (2) Is regional-scale variation in floristic composition within north-western Amazonia similar in magnitude to patterns of continental scale variation? The distance-decay method employed is practically not synthesized in the results. Is there any difference in a model with a slope of -0.07 with another one of -0.04? Why did they not mention these models in the results? Perhaps, an ANCOVA, which can test for significant differences between intercepts and slopes of two linear models, could help them to unravel this question. (3) Do soil fertility and other distance-dependent processes have a similar role in explaining floristic similarity at both regional -and continental-scales in Amazonian forests? In this case the Mantel and partial Man-

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tel tests are suitable to answer this research question, but see comments in the next section.

Regarding the Discussion, it largely lacks of any attempt to include the continental scale issues. Furthermore, there is a large amount of wording devoted to the oligarchic model (sensu Pitman et al. 2001), which was not included as a goal within the research questions . Without a clearer test on the relation between distribution patterns of the most abundant species or genus (see Pitman et al. 2008) and soil properties, much of the text included here remains quite speculative. Finally, the discussion seems to be a bit biased to the "niche assembly" model in spite of the results of the Partial Mantel Test, which did not support such a clear and dominant trend. What Partial Mantel tests showed was a very similar amount of floristic variation explained by environmental filtering and distance-based related processes as dispersal limitation, but not a clear dominance of anyone of them as it is assumed in part of the discussion.

Specific comments

Introduction.

It is maybe the clearest section of the manuscript. Nevertheless the work of Higgins & Ruokolainen (2004), and maybe a couple of additional references out of NW Amazonia could help the reader to get a broader view.

Methods and Results

Page 1426 line 25; 1427 line 1. PCoA analysis: as explained above, the two PCoA ordination diagrams presented in this manuscript (Figure 2a,b) showed a clear "horse-shoe effect", which is not a desirable property in this kind of analysis (see Økland, R.H. 1996). The Figure 2a shows a convex upward trend, whilst the Figure 2b shows a concave downward one. This particular situation calls for the search of another analysis, such as either NMDS or Detrended Correspondence Analysis (DCA), in order to correct the arch effect found on the second ordination axis (Ter Braak 1987).

1427, line 7. The authors introduce a new Index of Importance called ORI. Why did the authors consider that this new index gives more or better information than either the classical IVI or the individual variables of frequency and abundance? At least some words may be dedicated to it, mainly when we know the importance of basal area as a measure of dominance.

1427, line 12. Coordinates (or axes) of the PCoA were regressed against soil fertility category (SC) and dry season length (DSL) to assess the role of these environmental factors as determinants of the floristic patterns. Two problems arise with this approach: 1) The scores derived for the PCoA coordinates rather followed a quadratic model than a linear one, which would then violate the assumptions of the linear regression model and the expected normality in the residuals. Thus, testing for any significance based on such assumption is not appropriate at all. Furthermore, I am not sure about the validity of using categorical variables as explanatory ones under the assumption that errors will follow a normal distribution. In this case, a null model approach could be a more suitable way for testing significance, but still they may correct the quadratic trend of the plot scores.

1427, lines 20-28. As same as previous paragraph, except that in this case they are using a continuous variable instead of a categorical one to represent soil properties. However, a question here arises about, how comparable are soil analyses that come from different labs? Based on my own experience, soil analysis from different labs could strongly differ. Authors may at least make a short reference about the lab protocols to know whether they are comparable indeed. Likewise, how comparable the results obtained from a categorical classification of soils fertility to those obtained from chemical analyses are?

1428, line 2. The authors used a Sorensen's Index to assess the distance decay models. Is there any justification in employing Bray Curtis to carry out the PCoA and then switch to Sorensen? Despite of it is known that Bray Curtis only differs from the Sorensen Index in the fact that it uses species abundance instead of presence-

absence, the outcomes could change (Legendre & Legendre 1998). I think it is more appropriate to use the same index throughout the text (or use both!!).

A general comment: A unique model, which will include soils, climatic, and spatial variables seem more appropriate for testing the relative influence of each factor on determining the floristic patterns at both regional and continental scales. Either regression analysis based on distance matrices or based on Canonical Correspondence Analysis, followed by a Variation Partitioning Analysis (Legendre & Legendre 1998), seem to be very useful tools that could help to answer the main questions addressed in this paper (see the Comments section in Ecology Vol 89 No 11).

1430, line 15. The text reports six main floristic groups, but the graph only highlights three. Which are the other three?

1431, line 3. Subheading 3.4, Beta Diversity. Why is the PCoA analysis not considered as an analysis of Beta Diversity?

1431, line 29 to 1432 line 8. I think that it would be useful to know the rMantel correlation between soil variables and the log transformed geographical distances.

Question: Why is there not any similar r Mantel analysis at the continental scale?

Question: Why was DSL ruled out of all these analyses if it showed to be an important factor in the very similar correlations performed with the ordination axes?

Discussion

The next asseveration is questionable:

1433, line 1433. "The reason for these floristic differences may relate to the wide variation in soil fertility in north-western Amazonian forests"

How can the authors conclude this, since the Mantel correlations between the floristic patterns and the geographical space where even higher than those found between the floristic patterns and soils? I think it is quite a biased position that remains in other

parts of this section.

1434, line 24 to 1435 line 13; 1435, line 26 to 1436 line 13. All this text, plus other very short statements are devoted to discuss the oligarchic model sensu Pitman, which was not part of the main research questions. I guess the ORI index was created trying to test this model. If so, explain better why is this new index a good one to do this, please. Otherwise, you might try to test it more directly, specially in those cases assessing the correlation between abundant species and soil properties.

General comment: I would push for a better structure of the discussion according to the research questions made in the introduction and the results obtained from them. It should also have a stronger conceptual framework.

Tables and figures

Table 1. I do not know if electronic Supplementary Information is part of the format employed by the journal, but this table could be rather an appendix or supplementary information.

Table 2. line 4. "indicates that some taxa were omitted to show others that had stronger patterns within the groups". What does that mean? Could the authors explain this better either in the methods section or here in the table, please?

Figure 3. Why is there not neither a line trend nor an adjusted model of the distance decay pattern of the floristic similarity for the original data? Why was the model based on the averages at certain distance not explained, neither here nor in the methods section? What is the advantage to use one instead of the other? On which one was the discussion based?

References

Higgins, M.A & K. Ruokolainen 2008. Rapid forest inventories: a comparison of techniques based in inventory data from western Amazonia. Conservation Biology. 18:799-811 Legendre, P., and L. Legendre. 1998. Numerical Ecology, 2nd edition. Elsevier,

Amsterdam. Økland, R.H. 1996. Are ordination and constrained ordination alternative or complementary strategies in general ecological studies? Journal of Vegetation Sciences. 7: 289-292 Pitman, N. C. A., et al. 2001. Dominance and distribution of tree species in upper Amazonian Terra Firme forests. Ecology 82: 2101-2117. Pitman, N. C. A., et al. 2008. Tree community change across 700 km of lowland Amazonian forest from the Andean foothills to Brazil. Biotropica 40: 525-535. Ter Braak, C.J.F. 1987. Ordination. In: Jongman R.G.H., ter Braak C.J.F. and van Tongeren O.F.R. (eds). Data analysis in community and landscape ecology. Pudoc, Wageningen, pp. 91-173.

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