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Biogeosciences Discussions

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

E. N. Honorio Coronado et al.

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We thank the two reviewers for their comments and consideration of this manuscript. The reviewers agree that the manuscript is valuable for planning conservation of tropical rainforests. However, both raise concerns about the methods employed and the lack of discussion of the research questions. We have prepared a revised manuscript, and have addressed these concerns by (1) using alternative methods for the analysis of floristic patterns using genus- and species-level, and regional- and continental-scale data, (2) integrating regional- and continental- scale analyses, and (3) focusing the manuscript more strongly on the research questions by reorganizing and rewriting the discussion.

Original reviewer comments are in italics.

Responses for reviewer 2 (bgd-6-S521-S523)

Reviewer comment: Shouldn't the sentence read OR distance-related processes rather than AND? I don't think environmental heterogeneity is necessary a distance-related process? And what do the authors exactly mean?; dispersal limitation? Same comment for pg. 1426 line 2, and a few other times in the text. The misspelt words were changed following the reviewer's suggestion and the dispersal limitation phrase has been removed from the revised manuscript because this process has not been assessed in the analysis.

Reviewer comment: What do the authors mean by <priority was given>? Did they include other plot data but weight them differently? Or do the authors mean only that they restricted their dataset to 1 ha plots? In the revised manuscript we clarified that the dataset was restricted to plots that have one hectare of inventoried trees with a diameter higher or equal to 10 cm, with information on the number of individuals and species, and with voucher collections in herbaria.

Reviewer comment: Do the authors think that excluding 26.8 % of the species data changed the results? One way they could check this is rarefaction of their data. If they somehow were able to magically incorporate 100 % of the species data, would the authors speculate that the patterns reported in their results would become stronger or weaker? Why? Even though the percentage of trees with no reliable species-level scientific name varies among plots (7-71 %), we believe that probably, the excluded species are rare, hard to identify, and likely to be specific to certain plots. As a result, overall floristic similarity might decline further with complete inventories. However, as long as the levels of identification were similar across sites, they are unlikely to change the overall pattern of similarity.

Reviewer comment: It might be interesting to repeat the analyses without these singleton species that occur only in one plot, because singletons cannot be correlated

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with environmental variables or distances. In the previous manuscript the analysis was run excluding singleton species and this analysis was also repeated using the DCA analysis in the revised manuscript.

Reviewer comment: Somewhere the authors should bring up the caveat that there may be cryptic species that are morphologically similar but reproductively and genetically distinct. If this is so, perhaps the similarity between geographically-separated plots is over exaggerated. In the last paragraph of 4.1, we speculate that the different floristic relationships could be an artefact of varying levels of taxonomic resolution. However, it will be very difficult to detect cryptic species in the whole dataset because there is no available list that can let us to separate them and that includes a broad knowledge from the whole Amazonia, we expect to get similar patterns because common and abundant species are a key point to related floristically two sites and those species are easy to identify.

Reviewer comment: Perhaps the authors could come up with a more precise example to illustrate this point. After all, why couldn't there be a Myristicaceae species that is specialized for poor soils here? Isn't this a species-level and not a family level analysis we are discussing? In the revised manuscript, we included more examples. For example, in the last paragraph of 3.2, we mention a few examples of families, genera and species that show a pattern in abundance along the first ordination axis. In this section we refer to figure 3 where we put emphasis on the importance of the species level analysis with some examples of species of the seven most important families in the whole dataset.

Reviewer comment: Somehow the authors should stress more here that the <additional> pattern they are referring to that might be explained by dispersal limitation is certainly a more minor pattern. The discussion of dispersal limitation was removed from the revised manuscript because it was not assessed in the analysis.

Reviewer comment: I would like to see an expanded discussion about the difference

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between using species-level data and genus-level data. In the revised manuscript, a floristic analysis based on distance matrices was used to compare patterns of floristic composition obtained using genus- and species-level data and the Pearson's correlation coefficient was used to examine the similarity of the two distance matrices following Higgins and Ruokolainen (2004). The results of this analysis were discussed in the context of the results obtained by Higgins and Ruokolainen (2004) who also used data from western Amazonia.

Reviewer comment: If previous studies have shown that congeners span a large edaphic range, how can genus-level data be correlated with edaphic variables? Or do the authors think that most genera exhibit niche conservatism with respect to edaphic variables? What do the data tell us about these issues? How different are the patterns within northwestern Amazonia to the regional patterns? Genera are pretty conservative with respect to edaphic variables, and this explains why the abundance of different genera correlates with edaphic conditions. However, it does not exclude the possibility that many genera have species that occur in different habitats, as well. Just that the dominant pattern is the first one, and not the second. The results shown that regional-scale variation in floristic composition can rival continental-scale differences, and that variation in floristic composition at both scales was influenced by geographical distance and environmental factors, such as climate and soil fertility.

Reviewer comment: The authors should attempt to answer the three questions they posed in the introduction about the relative importance of edaphic variables vs. distance in regional and continental scale analyses. The revised manuscript was restructured according to the research questions made in the introduction to give more clarity to the text.

Reviewer comment: I did not think that Table 2 was a particularly effective way to demonstrate these results. Would there be a way to graphically depict these results? Also, could the authors include a table detailing the mantel tests and partial mantel tests? I found almost no discussion of the three questions listed at the end of the

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intro. The table 2 in the revised manuscript was replaced by a graph (see Figure 3) that better demonstrates the abundance patterns of the seven most important families, genera and species. A table including the partial Mantel test is also provided at the different levels of analysis (see Table 1). As previously mentioned, the discussion was restructured to follow the research questions cited in the introduction.

Responses for reviewer 3 (bgd-6-S724-S730)

1. General comments

1.1 Analytical methods to address research questions

Reviewer comment: 8230; (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 and Ruokolainen (2004)

In the revised manuscript, we used different techniques to analyse the floristic patterns: both distance matrices and floristic ordination, based on DCA to compare patterns of floristic composition obtained using genus- and species-level data. The analysis based on distance matrices follows the work developed by Higgins and Ruokolainen (2004) and the floristic ordination was run using DCA analysis to correct the "horseshoe effect" obtained in the previous manuscript using PCoA analysis.

(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

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between intercepts and slopes of two linear models, could help them to unravel this question.

In the revised manuscript, the decay in floristic similarity with distance is described in the results and the significant difference was assessed using the 95 % confidence limits in the overlap of the intercepts and slopes at different scales. The results were discussed in the manuscript comparing the results to other similar studies developed in western Amazonia.

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

In the revised manuscript, Partial Mantel tests were used to test the relative influence of geographical distance and environmental factors, such as climate (dry season length - DSL) and soil fertility, on floristic dissimilarity at continental and regional scales (Tuomisto et al., 2003; Ruokolainen et al., 2007). Specific comments related to this point are explained below.

1.2 Discussion

Reviewer comment: 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

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dominance of anyone of them as it is assumed in part of the discussion.

In the revised manuscript, we included the continental-scale issues in the results and discussions. The section devoted to the oligarchic model was excluded because it was not a main question of the research. During the reorganization of the discussion, we avoided to bias the results to any model, so we focused on the different results obtained in the partial Mantel test at different scales.

2. Specific comments

2.1 Introduction

Reviewer comment: Include the work of Higgins and Ruokolainen (2004) in the introduction. The suggested paper was included in the revised manuscript in the introduction and discussion sections. This example introduces the reader to the importance of taxonomic resolution in the data analysis.

2.2 Methods and Results

Reviewer comment: PCoA ordination diagrams show a clear "horseshoe effect". To correct this effect along the first axis of the ordination, we replaced the PCoA with a Detrended Correspondance Analysis. This analysis shows a strong floristic gradient through the plots along the first axis.

Reviewer comment: The authors introduce a new Index of Importance called ORI. We replaced the ORI value with the abundance and Kendall's tau values for each species of the correlation between abundance and the axis 1 scores from the DCA analysis. The direction of the correlation was compared among species to show that different species show different association with this gradient.

Reviewer comment: Coordinates 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. We replaced these simple regressions with a partial mantel test run using multiple factors. The significance of each factor was

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assessed using a Monte Carlo randomization procedure.

Reviewer comment: How comparable are soil analyses that come from different labs? We analyzed separately, plots with soil data from different labs because each lab uses different protocols and they are unlikely to be comparable. We used the simple soil fertility categories (SC) for the data analyses using all the plots.

Reviewer comment: I think it is more appropriate to use the same index throughout the text. In the revised manuscript, we decided to use a unique index (the Bray and Curtis index) throughout the whole text.

Reviewer 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. In the revised manuscript, we used a different method, the partial Mantel test, for testing this question. This method allows the relative effect of each variable to be examined.

Reviewer comment: The text reports six main floristic groups, but the graph only highlights three. In the revised manuscript, this graph has been redone using DCA analysis. All sites that are reported in the text are marked on this new graph.

Reviewer comment: Subheading 3.4, Beta Diversity. Why is the PCoA analysis not considered as an analysis of Beta Diversity? In the revised manuscript, the subheadings are related to the main questions proposed in the introduction. In this way, the ordination analysis was used to compare floristic patterns using genus- and species-level data.

Reviewer comment: I think that it would be useful to know the rMantel correlation between soil variables and the log transformed geographical distances. This is not applicable to the revised manuscript because the Mantel test was replaced with a partial Mantel test using multiple variables. The Mantel correlations, that represent the variation explained by one factor whilst controlling for the other two factors, are shown in

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Table 1 of the revised manuscript.

Reviewer comment: Why is there not any similar r Mantel analysis at the continental scale? In the revised manuscript the Mantel analysis is shown at both continental and regional scales.

Reviewer comment: 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? In the revised manuscript, DSL is considered in the results and discussions because climate is an important factor determining floristic patterns at continental scales.

2.3 Discussion

Reviewer questions the following declaration: The reason for these floristic differences may relate to the wide variation in soil fertility in north-western Amazonian forests The analysis prepared in the revised manuscript using partial Mantel test showed that soil fertility was a very strong character that defines floristic composition, as well as, climate and geographical distance.

Reviewer questions the following declaration: 1434, line 24 to 1435 line 13; 1435, line 26 to 1436 line 13 of the previous manuscript. 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. The revised manuscript has avoided discussing the oligarchic model sensu Pitman as it was not a main research question. However, a few examples are given in the results to demonstrate the dominance of some species over large distances.

Reviewer 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. The revised manuscript was structured according to the research questions throughout the introduction, methods, results and discussion sections.

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2.4 Tables and figures

Reviewer comment: Table 1 should be considered as supplementary information. This table was included as an appendix following the reviewer's suggestion.

Reviewer comment: 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? This table was replaced with Figure 3 in the revised manuscript where the abundance of the seven most abundant families and genera are shown along the main axis of the species-level DCA. Within each genus, two species were selected (the genus/species with the highest abundance and the highest correlation (Kendall's tau value) between the axis 1 scores and species relative abundance).

Reviewer comment: 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? This figure was excluded in the revised manuscript Instead, overlapping 95 % confidence limits of the intercepts and slopes of the relationship between floristic distance and ln(distance) was used to test for significant differences in the decay of floristic similarity between regions and at a continental scale.

References:

Higgins, M. and Ruokolainen, K.: Rapid tropical forest inventory: A comparison of techniques based on inventory data from Western Amazonia, Conservation Biology, 18, 799-811, 2004.

Tuomisto, H., Ruokolainen, K. and Yli-Halla, M.: Dispersal, environment, and floristic variation of western Amazonian forets, Science, 299, 241-244, 2003.

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Ruokolainen, K., Tuomisto, H., Macia, M., Higgins, M. and Yli-Halla, M.: Are floristic and edaphic patterns in Amazonian rain forests congruent for trees, pteridophytes and Melastomataceae?, J. Trop. Ecol., 23, 13-25, 2007.

Interactive comment on Biogeosciences Discuss., 6, 1421, 2009.