

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 #2

Received and published: 21 March 2009

The contribution by Honorio et al. is well written and provides interesting results concerning the floristic similarity within northwest Amazonian tree plots as well as among tree plots distributed throughout an enormous swath of South America. My main criticism is that the discussion does not deliver on what the introduction promises. I found almost no discussion of the three questions listed at the end of the intro. Instead, the discussion was restricted to putting the results of the northwestern Amazonian plots into the context of other published work concerning edaphic specialization, dispersal limitation and oligarchies. In a revised version of this, I would like to see the discussion expanded to actually address these interesting questions. See below for more specific comments.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



1. 1425 Line 21. 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.
2. 1426 Line 15. 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?
3. 1428 Line 26. 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?
4. 1429 Line 3. It might be interesting to repeat the analyses without these singleton species that occur only in one plot, because singletons cannot be correlated with environmental variables or distances. There might be different patterns for species that occur in many plots vs. those that occur in only a few; if for example one found a similar correlation with soil variables in rare vs. common species, this would be evidence against the dispersal limitation argument brought up on page 1436.
5. 1433. 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. What if *Iriarteia deltoidea* is actually several species? Same with *Eschweilera coriacea*? Some people think that they are actually different species..
6. 1436 Line 6. 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?

7. 1436 Line 13. 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. I say this because the previous paragraphs gave strong evidence for species tracking edaphically similar habitats. The fact that Jenaro Herrera has soils similar to Manaus and also a very similar species composition, while nearby forests like Orosa have similar soils and species composition to Yasuni is a decidedly non-neutral pattern that cannot be explained by dispersal limitation.

8. Finally, I would like to see an expanded discussion about the difference between using species-level data and genus-level data. How different are the results? 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? 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. If they cannot, then at least indicate what additional data they need to ask these questions, and then change the introduction to make clear that the focus of this paper is more narrow.

9. 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?

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)