Biogeosciences Discuss., 5, S895–S899, 2008
www.biogeosciences-discuss.net/5/S895/2008/

Biogeosciences Discussions

Interactive comment on "Branch xylem density variations across Amazonia" *by* S. Patiño et al.

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Received and published: 21 June 2008

Review of Patino et al "Branch xylem density variations across Amazonia", Biogeosciences Discussions.

In this manuscript Patino et al present analyses pertaining to the momentous wood density (WD) dataset they have collected across Amazonia as part of the RAINFOR project. More specifically, they present analyses that partition total variance in wood density into components associated with location (region, plot) and taxonomy (Family, genus, species) One quarter to one third of the variance is explained by location and family, with approximately 40% of variance remaining unexplained. The most interesting results compare variance within families, genera or species in relation to plot-mean wood densities; these indicating that much of the time the two properties scale isometrically. Oddly, in other cases the within-taxon wood density variation varied more than

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plot-mean WD (i.e. SMA slopes were significantly steeper than 1), perhaps counter to what one would expect. Disappointingly, there is little discussion of what these slopes mean, or what the implications are of isometric or allometric scaling between these variables.

I found the text in many places rather poorly written, and certainly in need of basic proof-reading: there are spelling and grammatical mistakes in many places, and incomplete (and even incorrect) figure captions. I would have hoped that one or more of the nearly 50 authors could have attended to these issues prior to submission. More seriously, more effort needs to be spent on ensuring that the Introduction effectively sets the scene for the upcoming analyses, i.e. so that it clearly & logically explains the motivations for the research and - most importantly for a manuscript such as this - the expectations (slopes etc)for the results that are presented. Further, in the Discussion more attention is needed on interpreting the results and providing the reader with insight into the issues dealt with in the study. Finally, at times the description given of specific results seems at odds with the results themselves (details below), and conclusions that are drawn in regard to the supposed importance of site climate and soil properties on wood density are unsubstantiated: no analyses concerning climate or soils are presented in this manuscript.

Nonetheless, the data presented as part of this study will make an extremely valuable contribution to knowledge of wood density of Neotropical species. The analyses that are presented have the potential to make a valuable contribution also, but with the current lack of context and interpretation of the results this potential is unlikely to be realized.

It appears that this manuscript is just one of three in which Patino et al seek to describe and analyse this dataset. In its current form I do not see that this first paper stands on its own feet; hence, I suggest that the authors either engage in a serious re-write of the current manuscript, or (even better, perhaps?) combine the current manuscript with one or both of the others, and in doing so present a stronger, more compelling and

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potentially influential piece of work.

Specific comments

Abstract

1. L12-13. No soils or climate data were analyses, so this statement is out of place in the Abstract.

2. L15-17. How is variation predictable according to where it growing? No results are presented that support this statement.

Introduction

P2007

3. L15-27. This section should be re-written to improve readability.

4. L25: "These theories...". Which theories are being referred to, specifically?

P2008

5. L17-26. This is largely conjecture, thus the statement that WD effectively integrates plant ontogeny, edaphic and climate effects over a plant's lifetime is a hypothesis, and not fact, and this should be indicated, and perhaps some evidence presented.

P2009

6. L11-19. Range size is not the same as abundance within sites, and the two may not even be correlated, yet the authors treat the two properties as being one and the same. Also, why should a species "converge" to the site-average WD value?

Methods

P2011

7. L1. The finding, attributed to Lloyd et al 2008b (in prep), that there is no effect of height on WD within a given plant is reasonably crucial for interspecific comparisons

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to be made across plants sampled at a variety of heights, as is done here. Thus, it would have been helpful if this cited work had accompanied the manuscript. In my own experience wood density tends to increase with distance back from the branch tip, in contrast to the findings attributed to Lloyd et al. But perhaps this is not the case with tropical tree species; in support of this possibility (and in agreement with Lloyd et al), the authors might be interested in the recent paper by Swenson & Enquist (Am J Bot, May 2008 issue).

P2012.

8. L16-19. These statements regarding "contaminated data" seem rather at odds with the description in Methods of a standard methodology being used for data collection.

P2016.

9. L2-5. If only one branch was sampled per plant, then the residual variation simply cannot reflect within-tree variation, as claimed.

10. L11-14. 27-33% variation can hardly be described as not "considerable"!

P2016.

11. L20. Could the authors be more precise than using phrases such as "nearly all"? Not oinly is this needlessly imprecise, it is actually untrue in this instance: while the confidence intervals of slopes fitted to 15/24 families and 17/23 genera did indeed include 1.0, these proportions can hardly be said to be "nearly all". Incidentally, all of the slopes that were significantly different than 1.0 were steeper than unity; I wonder if the authors could include some discussion & intgerpretation of this point in the Discussion?

12. L23. Why "somewhat surprisingly"?

13. L28-29. To my reading, Brosimum varied as much or more than other taxa, yet the authors describe this genus as "varying less than all the genera examined".

Discussion

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14. P2017, L11-12. Here the authors argue that knowledge of site conditions is necessary to predict wood density. This may indeed be true, but no data are brought to bear on this issue in this manuscript and so a statement such as this needs to clearly labelled as conjecture, or citations need to be given.

15. Fig5B & 5C captions are at odds with the axis titles.

16. "RAINFOR" is spelt "RAINFOIR" in caption to Fig 7.

17. Table A1 lacks a title. Also, reference is made to "Anderson & Malhi 2008", yet I could not find this citation in the reference list.

18. Caption to Table B1 is incorrect. Is this actually the caption to Fig 2?

19. Table C1 gives a long list of regression parameters, but no mention is made of what regressions these are.

Interactive comment on Biogeosciences Discuss., 5, 2003, 2008.