**Biogeosciences
Discussions**

Interactive
Comment

Interactive comment on "Coordination of physiological and structural traits in Amazon forest trees" *by* S. Patiño et al.

Anonymous Referee #1

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General comments:

The Authors present a complex analysis of an impressive data set and present key dimensions of trait variation across rain forest species and sites. It represents what I am sure is an immense amount of time and effort. The results confirm previously reported dimensions of trait variation relating to wood density, leaf area to sapwood area ratios, carbon isotope discrimination, leaf [N] [P], seed size, and height. The authors have done a very nice job of teasing out key relationships and discuss in detail possible mechanisms underpinning these relationships. The arguments in the manuscript, in general, have been well researched and addressed as fully as the dataset will allow. I found the discussion interesting and insightful.

I would ask the authors to please not take the below criticisms personally. The ideas,

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analysis, and arguments are very good.

The manuscript, as it stands, is very long – In my opinion, far longer than it needs to be. I think the text and figures could easily be reduced by 50%. Much of the text is vague (this is a particular problem) and filled with unnecessary technical jargon and grammatical errors. I give a list of specific suggestions for improving the writing in "specific comments". I sincerely do not think this reflects poorly on the first author. Most writers (myself included) find it very difficult to "see" their own mistakes and it is very helpful if a co-author can give the manuscript a careful and thoughtful review, helping to simplify and clarify the manuscript. In particular, sentences should be shortened. Jargon should be removed unless necessary. Stacked modifiers (e.g.,". . .five integrated genetically controlled trait dimensions") should be avoided. I think it is almost always better to say something as simple as possible in the simplest language possible. This won't "dumb-down" the manuscript. On the contrary, it will make the points clearer and easier to understand.

The introduction is quite long and tedious. I think it would be easier to follow the authors' points if it were broken into sections with subheadings. It would probably be best if the trait correlations relating to their major axes of variation were discussed in the introduction. For example, discuss trait relationships pertaining to "foliar construction costs", and then "leaf economic spectrum", and then "light acquisition". That is just a suggestion, but I think it really needs some structuring to help "prime" the readers for the results and discussion.

The calls to the figures in the text are often incorrect (wrong figure). Please check for consistency.

Specific comments:

Title page: There is an address number "6" (JCU), but no author is associated with that address.

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Page 2, line 3: Was "S" defined somewhere? I can't find it.

Page 3, line 10: What is a modelling interface? Do the authors just mean that plant traits are widely used in ecology and biogeochemistry? The simpler things are stated, the easier they are to understand.

Page 3, line 12: No comma after "adaptations".

Page 3, line 13: What do you mean by "obtaining a mechanistic basis"?

Page 4, line 21: Typo: ", thus reflecting"

Page 4, line 21-23: Longer path-length does not necessarily require ("need") wider vessels. Any of the traits in "Darcy's" Law (as affecting sapwood-specific conductivity) could be responsible. For example LA/SA has been found to increase with plant height. Additionally, taller plants are exposed to direct radiation and, thus, higher VPD. Both, increasing LA/SA and VPD, could be balanced by increasing sapwood-specific conductivity via vessel diameter. I might suggest just saying that increasing hydraulic resistance could possibly be offset by larger conduits.

Page 4, line 25: Typo: another "this"

Page 4, line 23-20 is one big run-on sentence. I don't understand what vessel lumen fraction and wood density have to do with shade tolerance either, but perhaps this is a different sentence/thought?

Page 5, line 4: If you look at studies that carefully measured vessel lumen fraction and sapwood density, there is almost always a negative correlation. In fact, a negative correlation between these two traits is practically a mathematical certainty. What is most interesting, and most pertinent to the authors' point, is that the density of tissue outside vessel lumen matters MORE than how much empty space there is (vessel lumen fraction).

Page 6, line 14: turnover of what? Are you talking about species shifts through suc-

cession?

Page 6, line 18: The abstract says three are new data for 1040 species

Page 8, line 1: A little more information on how wood density was measured would be helpful here? Readers won't have time to go back to the original publication.

Page 9, lines 1-7: Would it be possible to at least give the range of values (e.g., TN, TP, OC, Ca) across the sites in this study? What do the authors consider "low" or "high" fertility? It is not possible to determine from this description how different the sites (or species groupings) are. Ditto for precipitation. Readers simply are not going to have time to go back to these original references.

Page 11, line 6-8: More information on the "non-parametric correlation analysis" would be helpful here. What analysis did the authors use? Did they do the analysis in R? If so, what package? I don't have time to read the Fyllas paper, so I will have to assume that this was done correctly.

Page 12, line 18: Couldn't the same information in Figure 1 be included in a small table? In the interest of shortening the ms, this seems like a good option. Ditto for figures 2, 8, and 10.

Page 12, line 16: "Statistical distribution of measured traits" really doesn't convey any information. I would suggest the author's consider what the section is really about and come up with an informative heading.

Section 3.3: omit

Page 14, line 21-25. This text probably isn't needed.

Page 15, lines 7-14: Text probably not needed. Also, this is discussion material, not results.

Page 15, line 18-22. Not results

Page 16, CPC results: It is very likely that few readers will even know what CPC modelling is, much less be able to interpret these data (text, tables, figures). For example, what is meant by "max [lamdalow lamdahigh]. . . jth characteristic root"? Additionally, readers certainly won't have time to search for and download the "rational" and "considerations" (from supplemental material) need to understand the data. If this is really the best way to analyse these data, then I would suggest the results be reported in a straight-forward manner, using common language. I might suggest the authors try very hard to explain things in the simplest terms possible.

Page 17, line 6: probably don't need figures 8a-c. The information is adequately described here.

Page 18, line 23-27: Not results

Page 19, lines 15-18: These are not results. I also don' t think it's that "impressive" if you consider that much of the variance in any dataset will be explained by the first principal component – even when there is no relationship at all among traits (i.e., a perfect multi-dimensional "sphere" of un-related points). This is just the nature of PCA – you'll always get a large percentage of variance explained by the 1st PC – it's not like bivariate regression where the r2 tells you how well one trait predicts another.

Page 19, lines 20-24: Simpler way to write this? I had to read this sentence about 4 times to understand what the authors were interested in. . . and I'm still not really sure. Please, keep it simple.

Page 20, lines 1-10: Most of this is discussion, rather than results.

Page 21, line 7: I am not sure it is fair to say Wright et al. 2006 did not consider environment or soil in this publication. Looking over it, it seems they address both in their analysis. I am not sure about Baraloto et al. 2010.

Page 21, line 10: Of course, we don't really know how "widely varying" these sites were without a better description in the methods.

Page 21, line 10: Should this be trees or species?

Page 25, line 15: Awkward sentence. Perhaps try simplifying/rephrasing?

Page 22, line 5: Should this be "Meinzer"?

Page 22, lines 13-16: I wonder if it wouldn't be good to point out here that in angiosperms, vessels and wood (outside lumen) serve different and independent functions: hydraulics and mechanics (i.e., the represent different axes of variation). Whereas in conifers, tracheids serve both a hydraulic and mechanical function (tracheids, in addition to conducting sap, are also load-bearing). Gymnosperms and angiosperms both vary freely in the "S" dimension (vessel area/vessel number, viz Zanne et al. 2010). I actually think it's a better argument to say that angiosperms can achieve much wider conduits, and since conductivity scales to the fourth power of conduit diameter, this is why they achieve higher conductivity – not because increases in vessel lumen fraction (i.e. decreasing wood density) somehow confer greater conductivity benefits.

Page 22, lines 21-29: This sentence is way, way too long. . . and convoluted. I might suggest just saying something like, "Decreasing wood density, associated with increasing [P] and LMA, is also likely associated with decreasing investment in wood defence [citation].

Page 23, line 1-3. What do the authors really mean here? What does the CPC analysis, table 3 [table 4?], and the lack of contribution of Px for the second component identified mean? If they mean to say, "wood density did not correlate with traits conferring high growth, i.e. the leaf-economic spectrum", then just write that.

Page 23, line 10-12: awkward

Page 23, line 12-14: awkward

Page 23, line 23: By "xylem traits" the authors really mean "wood density", right?

Page 23, line 25: Yes, indeed – bananas and wombats. LOL. I hope this phrase survives the peer-review.

Page 24, line 1: What study - the Baraloto paper? Also, typo (. . .all three of foliar. . .)

Page 24, line 4: I understand the author's desire to address this question, but neither Ks nor vessel diameter were measured in this study. I don't see how it is really possible and I don't think the previous discussion sheds any real light on the issue. I think the authors should be allowed to speculate, but it should be pointed out clearly that the plant traits necessary to address this issue were not measured.

Page 24, line 15-21: very long and awkward sentence. Is this test described in the methods? Stand-level turnover of what (same phrase used in Fig 10 legend)? The following sentence uses term "tree turnover". If you mean species shifts taking place through succession, make this clear. Personally, I think "turnover" is a confusing term that never should have been applied to species in the first place.

Page 24: Line needs a citation.

Page 28: In general, the discussion is insightful, but the language is very vague, which makes it difficult to read. For example, the following sentence doesn't convey much information: "The decline in Ma with LA/SA is understandable in terms of the trait dimension Fw". It is much better to write: "Increasing Ma and LA/SA may reflect thicker leaves and denser canopies, which would confer greater light interception for taller plants". I would encourage the authors to say exactly what they mean and avoid obtuse language.

Page 28, line 20: what does ". . .a smaller range of genetic variability" mean? Fewer species?

Page 29, 6: What does "a rather considered entity" mean?

Page 30, lines 26-27: Because the authors presumable know which species are drought-deciduous and which are not, they should be able to test this hypothesis di-

rectly. This is important, as it is not only counterintuitive that LA/SA should decline with precipitation, but defies logic and suggests something went wrong in the analysis.

Page 33, line 4-9: Long, convoluted sentence. Much of this paragraph appears to be methods/results and not discussion. The last few sentences are all that are needed to make the point.

Page 35, lines 25-29: I can't understand much of this argument being made here because the writing is convoluted, but what I do understand is probably wrong. Wood density, per se, does not influence Ks. Ks is influenced mainly by vessel lumen fraction (directly influences wood density) and/or S (the vessel diameter-number trade-off). See the Zanne et al 2010 reference cited by the authors. Changes in S will influence Ks without changing wood density. In fact, changes in S are responsible for the majority of the variance in Ks across and within species. If Ks (not measured) is changing without changes vessel lumen fraction (also not measured), then it is being accomplished via S (also not measured). The idea that Ks is increasing across the many species in this dataset because vessel lumen fraction increases at the expense of fibre/parenchyma lumen is unlikely. In general, changes in S (no changes in WD) should account for between 2 and 3 times as much variance in Ks as changes in vessel lumen fraction. Without actually having these data (vessel diameter, number, and Ks) this part of the discussion is just arm-waving, and probably wrong.

Page 36, lines 17-19: incomplete sentence. I also don't understand the point of this sentence. Ditto for the rest of this paragraph.

Interactive comment on Biogeosciences Discuss., 8, 5083, 2011.