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Discussions

# *Interactive comment on* "Parallel functional and stoichiometric trait shifts in South-American and African forest communities with elevation" *by* Marijn Bauters et al.

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General Comments:

I found the paper well written and of general importance to the scientific community given the rarity of elevational transects in tropical forests. My concerns with the current version are generally technical, based on a few somewhat easily modified statistical and linguistic approaches that would greatly improve the manuscript.

(1) Consider that altitude itself is not biologically meaningful, only factors that change with altitude (temperature, pressure, UV). So, in essence you are statistically comparing a proxy for climatic variables that influence plant and soil processes. This is cer-



tainly acceptable when climatic variables are not representative, as in the case where climate stations are too remote (or at the wrong elevation) from study plots. However, if you believe your MAT/MAP data reported in Table 1 are real then those metrics would be more sensible to use than altitude.This is because 2000 m elevation means nothing to a plant, and does not represent the same temperatures at different latitudes, aspects, distances from oceans, etc.

(2) Improving the statistical approach is unlikely to change the authors overall conclusions but currently does not represent the best practices in the field. I outline several specific areas for improvement below.

(3) The authors use functional characteristics and ecosystem function interchangeably. I believe this to be invalid but acknowledge that there are many whom would disagree with this semantic distinction. I agree that sometimes SLA or LNC is related to leaf function (e.g., photosynthetic rates) but it does not necessarily represent ecosystem function. Similarly, leaf and soil ðİŻ£15N values can integrate ecosystem N cycles but the authors to not demonstrate firm understanding of the caveats and alternatives. Specifics below.

Specific Comments:

L140 Consider that lme4 is the preferred package as nlme is no longer supported and therefore uses older estimators.

L141 Including the interaction and then dropping it from final models based on P values is in essence manual stepwise selection, which is generally not recommended when selecting from nested models. Consider using AIC or just leaving the interaction in. Also, there is no description of how these P values were estimated from an nlme model and this is a topic of great debate in the literature as determining the denominator degrees of freedom for random error variance is not a straightforward exercise. For instance, when using mixed effect models and P values the authors need to report something to the effect of: "hypothesis testing was evaluated using likelihood ratio tests with type III ANOVA Satterthwaite approximation of degrees of freedom in the lmerTest package".

L142 The authors mention they assessed linearity (normality?) but do not discuss what other model fitting metrics they evaluated. Heteroscedascitity? LPC certainly looks non-normally distributed, thus in violation of the pearson correlation statistics that are reported on Fig. S2.

Fig. S2 While on the subject, correlation among variables that contain the same variables will always be correlated. Therefore, reporting correlation statistics (with P values!) between LNC, C:N, and N:P is nonsensical.

L152 "Climatic conditions were similar" is simply not true. Rwanda gets wetter and the Ecuador gets drier with increasing elevation. This is a major problem with elevational transects in the tropics as sites that get wetter at high elevations also receive less solar inputs and will typically contain less foliar N as a result of reduced photosynthetic variability (not to mention either greater hydrological or gaseous outputs of soil N). It is true that both transects get colder but they do so within very different temperature ranges and magnitudes (12.8° vs. 4.7° changes that barely overlap) owing to the vastly (~300%) different altitudinal ranges among transects (2811 vs. 1084 m). How can these authors claim they are similar? Where did the temperature data come from? What are the latitude and longitudes of the sites?

Instead, the author should refer to the similar adiabatic declines (e.g., 219 m per -1° in E, 230 m per -1° in R), then graphically depict temperatures of the plots; same goes for Fig. S3. This is one of the most important comments I provide on this manuscript.

L163 Consider explicitly examining the difference between foliar and soil ðİŻ£15N as this may better represent aspects of the N cycle (shameless plug: see Mayor et al. 2016, Eco. Lett. 18 for an in depth discussion of the patterns among tropical ðİŻ£15N values).



L181 The authors should refer to the similar adiabatic declines (e.g., 219 m per -1° in E, 230 m per -1° in R.

L203 One could also find elevation transects where the parent material changes from low to high along the same mountain as well. Also, you imply that your transects have consistent geology, aspect, slope etc. when they clearly do not based on Table 1 and Fig. S1. So the position you invoke, based on Ed Tanner's defense of low replication, is invalidated by your own data. In addition, there was a recent large scale global elevation gradient study published in Nature (another shameless plug) that suggests within-regional variability is negligible when comparing cross-continents. Given this, your selection of datasets based used in Fig. S3 to those with only single mountain transects may limit your conclusions and does not appear well justified.

L206-207 So Van de Weg data is from both SA and SE Asia? Why don't the lines reflect that on Fig. S3? Why don't the lines simply state the location of the transects rather than the authors?

L222 As mentioned, what may be more informative is the enrichment of plants relative to soil ðİŻ£15N values (△15Nplant-soil) is increasing at both sites. This may indicate either greater fractionating pathways during N uptake/translocation or a shift from one N form to another, rather than simply a more closed N cycle. Also, the Rwanda soil line does not appear to fit those data. Were any nonlinear lines compared?

Refs to consider here:

D. N. L. Menge, W. Troy Baisden, S. J. Richardson, D. A. Peltzer, M. M. Barbour, New Phytol , no- (2011). E. A. Davidson, C. J. R. de Carvalho, A. M. Figueira, F. Y. Ishida, et al., Nature 447, 995-8 (2007). J. Mayor, M. Bahram, T. Henkel, F. Buegger, et al., Ecol Lett 18, 96-107 (2015). C. Averill, A. Finzi, Ecology 92, 883-91 (2011). E. Bai, B. Z. Houlton, Global Biogeochem. Cycles 23, GB2011 (2009).

L234 You mean to say "different degrees of dependence upon ectomycorrhizal fungi" in particular. (Although there is no discussion about the mycorrhizal type of your tree communities, I would assume they are AM which do not appreciably fractionate). Also, there is no talk about why higher soil 15N suggests and open N cycle when there is a large body of literature that clearly discusses that it may be due to greater gaseous N losses to denitrifiers.

L240 “Confirmed”? No, not confirmed, as you say above, only suggestive of declining bioavailable N.

L242 Here is a statement that would be much stronger if you were actually looking at the same temperature ranges across all transects in Fig. S3.

L245 What is “this evolutionary conditions for N” mean?

Technical Corrections:

L53 Extra comma.

L156 “ajd” subscript incorrect.

L160 Spacing between R2 is off.

L227 What does “C:N increase dominates over the C:P increase along the transects” âĂÍmean? Also, Fig. 1 lists N:C, not C:N, and there is no C:P. . .

L228 Consider replacing “build in” with “incorporate” or “use”.

L239 Delete “both”.

L508 Fix 15N superscript.

L509 Fix R2adj subscript.

L517 It would be useful to those conducting meta-analyses if these raw data were made available.

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