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Biogeosciences Discussions

Interactive comment on "Regional and temporal patterns of litterfall in tropical South America" *by* J. Chave et al.

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Overall, the comments of Referee 2 are very positive. I encourage resubmission with minor corrections, taking those into account and in addition also my own comments which are made both as the Editor and as a reviewer.

p2, line 7: can we specify that the +/- term is, indeed a standard deviation (and not a standard error); It's obvious, so stating it once would be enough.

same line: Can we express things like Mg/ha/yr in the more usual form (with no "/" and with "-1" superscripts) throughout the paper

Equations (1) to (4): why not sum from 1 to 12 (as opposed from 0-11)? Seems more intuitive to me (months 1 to 12) and surely also mathematically equivalent.

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Figure 6. Referee 2 raises some valid concerns here, and I wonder if ratios have just been used to allow the inclusion of the Fyllas live plant material data set. In which case, it is pointed out that C.A Quesada has actual nutrient values for litter at most of the RAINFOR sites. There are some problems with this as well (potentially different rates of mineralisation whilst that litter was on the ground etc.), but maybe incorporating that data might provide an alternative approach, also allowing direct litter N and P relationships to be tested. In any case

1. If one has a dataset with some ecosystems being N limited and some others being P limited (as I believe to be the case here), then is a simple linear approach looking at each variable independently indeed adequate ? I believe not; at best some form of multiple regression should be applied, and I would suggest something like LF = LF_max*f(N)*g(P) might be appropriate where f(N) and g(P) are non-linear functions equal to 1 when either N or P is not limiting. Or maybe multiple quantile regression ? Or LF = min [f(N), g(P)]. Anyway, something better.

In any case, Fig 6a looks to me as if a second order polynomial would fit with a reasonably high level of significance. Also, if the two very high N:P values were removed from Fig 6b, then would any significance remain ?? Checking Table 1 for their ID, I al also left wondering if for some reason or other some points in those tables have inadvertently been omitted from the graphs. I would also encourage the authors to include as many points as possible in a second set of graphs looking at leaf litterfall only.

Seasonality index in general:A nice idea, but whether one gets a correlation or not does not tell us whether it is a negative or a positive precipitation/litterfall relationship within each site. Perhaps in the Figure the seasonailty- patterns for the rainfall patterns could also be included (?). Also, by looking at the relevant equations, it should not be too difficult to develop a correlation index for each site based on the monthly litterfall and rainfall vectors, perhaps even in some sort of time series analysis approach incorporating lags. (though probably for another day).

BGD

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Interactive Comment

The RL ratio is a nice result, but it would hardly seem that the line fitted is appropriate (in terms of both goodness of fit and heteroscedacity. One begins to think that it is more a y = a + 1/x (in which case versus P:N ratio it would really be linear (!). Or alternatively, segmented regression (nice package for this in R : "segmented") might possibly be used to show that the relationship suddenly "explodes" below a N:P or about 10.

Discussion: paragraph 2; nice idea (wish I'd thought of it myself!) but the Patino et al. Reference will have to be to "unpublished data"; as that paper was never submitted.

We need to make sure we don't the Figure numbering mucked up in the final version.

Interactive comment on Biogeosciences Discuss., 6, 7565, 2009.