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Biogeosciences Discussions

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

J. Chave et al.

Received and published: 27 October 2009

Anonymous Referee #2 Received and published: 21 September 2009 General Comments Our understanding of ecosystem processes in tropical forests has lagged far behind our knowledge of temperate forests. Chave et al. present a literature review of a large number of published and unpublished litterfall datasets for South America, and analyze litterfall quantities with respect to rainfall, soil fertility, and litterfall N:P. They also applied a clever index of litterfall seasonality, and then relate litterfall seasonality to precipitation seasonality.

This review brings together an impressive amount of data, and it will serve as a benchmark for both modeling analyses and efforts to understand the geographic variation in carbon cycling across South America. In particular, some of the notable trends that were extracted from this dataset include the negative relationship between investment

in reproductive organs versus photosynthetic tissue and N:P, suggesting that allocation patterns vary as a function of soil fertility, and the positive relationship between litterfall seasonality and precipitation seasonality. Interestingly, litterfall quantities did not depend on annual precipitation. In addition to these conclusions, the seasonality index that these authors use is a useful tool that can be applied to the analysis of other ecosystem parameters. This paper was both well written and concise, and will make a nice contribution to the literature.

RE: We thank the reviewer for this positive comment.

Specific Comments Title. In some sense the title does not capture the essence of the paper because the temporal patterns that are discussed are seasonal patterns, not long-term records of litterfall.

RE: Title has been altered from ‘temporal’ to ‘seasonal’

P. 7567, line 11. Do you mean litterfall N:P ratio? Please clarify. P. 7569, line 16. What are “dry rainforests”? This must mean dry forests.

RE: Changed in the revision

P. 7571, line 27-28. I do not completely agree with the assumption that N and P have similar resorption amounts, and thus litter N:P ratios can be estimated from foliar N:P. See the following reference. Hättenschwiler, S., Aeschlimann, B., Coûteaux, M.-M., Roy, J. & Bonal, D. (2008) High variation in foliage and leaf litter chemistry among 45 tree species of a neotropical rainforest community. New Phytol., 179.

RE: We concur, but given the lack of consistent data, we have used what was available. We now cite Hattenschwiler et al to emphasize this possible caveat in our analysis.

P. 7572. The climatic dataset could use a little bit more description. For example, how does this global climate dataset compare to local measurements? At what scale were the climate data collected?

RE: The paragraph has been updated.

P. 7574. The Introduction discusses annual litterfall quantities and NPP in units of Mg CARBON per ha per year, and the Results section reports data in units of Mg DRY MASS per ha per year. It would make the Introductions and Results section more comparable to standardize units. It is not until the Discussion that the units are clarified.

RE: We have clarified the units throughout.

P. 7586. In Table 1, there are a large number of sites with very high N:P ratios (e.g. Medio Rio Caqueta) but these sites do not seem to appear in Figure 6.

RE: Thank you for pointing this out. The NP ratios have been difficult to retrieve, and the final table NP figures were a late addition. We simply forgot to update Fig 6. Our mistake! Please find the new fig 6, which incidentally shows different results …

P. 7575, line 8. Is there any way to put this number, i.e. the mean litterfall seasonality index of 0.166 into biological terms. For example, can you add “indicating a mild/distinct/etc trend to litterfall across these sites”. Also, it would help to state the range of SL here. P. 7575, how about replacing “designed to be eaten” with “have

RE: Changed on both accounts

J. Lloyd (Editor) j.lloyd@leeds.ac.uk Received and published: 25 September 2009 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.

RE: Thank you.

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

RE: Done

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.

RE: Yes, but more difficult to relate to the polar coordinates (angle is now simply 30*i). If it’s OK, we would like to keep the equations as is.

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.

RE: We are aware of these limitations, and emphasize that not too much confidence should be place”d in the results of Fig 6 (updated from the previous version). We are reluctant to embark in a complex stoichiometric modeling exercise given the limited confidence on the data. We hope, however, to return to this topic (hopefully as part of the Rainfor project) in the future.

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).

RE: We indeed hope to return to this topic in the future.

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.

RE: Again, we have little confidence in the NP and CN ratios for these plots, and the analysis is only exploratory. Beyond the simple analysis reported here, spending much time dissecting these correlations in search for a functional explanation would be vain.

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.

RE: Thanks again for these useful and positive comments.

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