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Biogeosciences Discussions



# *Interactive comment on* "Above- and below-ground net primary productivity across ten Amazonian forests on contrasting soils" *by* L. E. O. C. Aragão et al.

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This is a very fine paper which dramatically expands our knowledge of NPP for tropical forest ecosystems, also providing some new and important insights, for example apparent effects of soil fertility on root turnover rates.

My main some concern relates to some questionable extrapolations, and also some statistical considerations.

In terms of the first point there are two issues:

1. On page 2545 (paragraph starting line 14) we find out that fine root production



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at Manaus was not actually measured and that a value is then simply fudged on the basis of values from similar sites. Although such an approach may be consistent with the "Malhi School of Heroic Extrapolation" I really do not think it is permissible here; especially as this invented number is then used in subsequent regressions (eg Fig 8). I am sorry, but I think this site simply has to be dropped from the analysis.

2. Although phosphorus is hypothesised to be as a key driving variable, it also turns out that the "available" P has not even been measured for two of the Caxiuanã sites. With values again "fudged" from nearby sites. Especially as CAX-06 is a ferralsol (with the other CAX non-terrapretas being acrisols) it is worth noting that co-author Quesada should have these soils in Manaus and thus be able to do the analyses easy enough to get the real values. I thus cannot really regard this as acceptable; especially as CAX-01 and CAX-02 actually varied in available-P quite a bit (15.7 and 8.9 mg/kg respectively). Basically, it is not a valid extrapolation and hopefully getting the true numbers would improve the relationships presented.

In terms of statistical issues; this paper differs from most in the special issue in that problems with spatial correlation are not taken implicitly into account. To a large extent this is not a problem as the Caxiuanã and Tambopata sites are quite different. But for Agua Pudre this is a different kettle of fish as the two sites are effectively the same. I would therefore suggest pooling the data of these two sites together this being the simplest way to deal with such problems. Although this reduces the number of sites again, it might even improve the significance of some regressions because the AGP observations would not then be given an unnecessarily high weight (twice the other sites) as they are now.

Other Issues;

P2455: The M&M for the leaves is correct only for the Brazilian samples; check the Lloyd et al. Biogeosciences Discussion paper in the special issue once it is finally published.

Section 4.1: Tapajos; Yes, this is interesting; perhaps one alternative to trying the two separate regressions (with and without TAP-04) would be to apply some sort of robust regression (eg. http://www.jstatsoft.org/v14/i07) and see if TAP-04 emerged as an outlier.

Section 4.4; Although there is some support for the "P hypothesis", shouldn't one also look at other nutrients, for example Ca, K or Mg in order to make sure that they are not even better predictors. After all, co-author Quesada has the numbers.

Fine root turnover; Yes, an interesting result. But

1. It does not look linear to me; why not try a 1/x fit or similar. 2. Any discussion of this issue, should also include a consideration of the hypotheses presented in Silver and Miya; Oecologia (2001) 129:407–419 3. Likewise, the Preiss et al paper hypothesises Al toxicity as a key determinant influencing fine root lifetimes. Perhaps it would therefore be interesting to look at any exchangeable Al relationship with fine root turnover as well, even if just to show this was not important.

Table 2: references should be Quesada et al. (2009b) and Fyllas et al. (2009)

Table 3; something wrong in the second row.

Table 4; well, I guess this shows the theory; unless data are linear and normally distributed the non-parametric is better; when you have ties (Rainfall, Temperature, DSL) the Kendall's tau should win; otherwise Spearman's rho. I guess it is OK, but why not also try some multiple regressions (perhaps robust as suggested above with all soil variables perhaps logged ?). After all, there is no reason to believe that only one factor should be involved. Also, do you need to correct for multiple testing ? If not, then why not ?

Fig 4; check the y axis for part b

Fig 7. Might be worthwhile trying to look at the leaf nutrients on an area basis (using SLA values which are also available); likewise, do things change when soil P and N



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are expressed per unit area ? This is which is what agronomists concerned with plant production generally prefer (i.e. kg P /ha) . Co-author Quesada should have the bulk densities allowing this transformation to be made and who knows; it might even help improve the relationship. At least it is worth a check.

Fig 9. I am not that happy about having soil P sometimes on a log scale and sometimes on a linear one. But in any case, could you put the actual numbers on the x axis (i.e. have it going from 10 to 100

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