

Interactive comment on “Spatial distribution and functional significance of leaf lamina shape in Amazonian forest trees” by A. C. M. Malhado et al.

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As for the accompanying paper by the same group (which I have just reviewed), this manuscript is generally well written, representing a considerable body of work and the bringing of a new and important data set.

Nevertheless, I suggest many of the issues raised in my review of the "leaf size paper" also be considered, viz: appropriate statistics, more meaningful metrics of soil fertility, the range of soil and climatic variability found here in terms of other studies (etc). In particular, I am wondering if a multivariate approach is possible ?

I also offer some further specific comments

1. Was it also attempted to look at leaf shape in terms of pioneer index/wood density

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(with and without allowing for the covariance with leaf size) ? Even if this result was negative, it might be good to discuss this issue.

2. Given the role of leaf shape in modulating canopy light capture as extensively mentioned in the introduction, it is a pity that incoming radiation was not itself looked at as an influencing variable. Might this be possible?

3. Do you need to repeat the TRMM Figure 3 which is also in Figure 1 of the accompanying paper?

4. line 23, page 1841: Is respiration increasing more than photosynthesis at high temperatures really the reason ? I think the discussion of this issue in the "leaf size" paper was much more informed

5. line 15, page 1846. I think what was meant with I_1 (and in all other similar cases) was an "I" followed by a subscript "1".

6. Section 3.3: If I get this correctly, then dry season length and total precipitation both act in the same direction. Certainly, this may be worthy of emphasis at this results stage.

7. p1856: Although "resolution of this fascinating observation" will indeed require more work, perhaps the collective minds of this long author list could perhaps come up with an hypothesis or two !!??

8. p1857: I think it needs to be appreciated that Cunningham et al. (1999) were not considering what is uniformly a relatively high rainfall system. Are these considerations really so relevant for the Amazon ?

Finally, I realise with spatial autocorrelation accounted for, what one gets is not always what one sees. But it would be nice to see some actual graphs of the relationships represented in Tables 4 to 6; this would certainly make the paper more "accessible" to your average reader.

BGD

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