

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

**A. C. M. Malhado et al.**

Received and published: 21 May 2009

Here, we immensely thank the referee J. Lloyd and the anonymous referee for all their constructive comments on our work, and hope we have satisfactorily addressed all comments, as detailed below.

1. J. Lloyd (Referee) 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?

Malhado et al. (response) See comments for leaf size paper.

2. J. Lloyd (Referee) Was it also attempted to look at leaf shape in terms of pioneer index/wood density (with and without allowing for the covariance with leaf size) ? Even if this result was negative, it might be good to discuss this issue.

Malhado et al. (response) We didnot attempt this because of the lack of strong hypotheses relating life history traits to leaf shape. We agree it would be interesting as an exploratory analysis but perhaps not appropriate for this paper.

3. J. Lloyd (Referee) 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?

Malhado et al. (response) This would be a fascinating study. Unfortunately, we did not have access to a long term averaged dataset on spatial variation in incoming radiation. If this data ever becomes available we would certainly be interested in doing this analysis.

4. J. Lloyd (Referee) Do you need to repeat the TRMM Figure 3 which is also in Figure 1 of the accompanying paper?

Malhado et al. (response) No, we donot need to repeat. This has now been removed.

5. J. Lloyd (Referee) 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

Malhado et al. (response) Fair comment, we have combined the text to produce a more in depth discussion of this issue.

6. J. Lloyd (Referee) 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".

Malhado et al. (response) This has been changed throughout the document

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

7. J. Lloyd (Referee) 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.

Malhado et al. (response) We have now emphasized this in the results and added a short sentence to the discussion.

8. J. Lloyd (Referee) 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 !!??

Malhado et al. (response) Fair point, ve elaborated on this although clear hypotheses are problematic with the current level of information.

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

Malhado et al. (response) We feel Cunninghams generic statements are relevant to the Amazon and other high rainfall systems although we agree that these effects may be far more subtle and difficult to detect this may explain the lack of strong patterns and, more generally, is a problem of doing macrogeographic analysis within ecophysiologicaly similar environment.

10. J. Lloyd (Referee) 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.

Malhado et al. (response) We have all of the graphics but because of the nature of the statistics for analysing spatial autocorrelation (as you have rightly mentioned you do not always get what you see), we feel that in the absence of a sophisticated understanding of this technique the simple x-y figures may mislead the general reader and certainly do

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

not make the results easier to comprehend having presented these in several seminars. More generally, we hope the analysis in this paper and the leaf size paper provokes a discussion among our research community on spatial autocorrelation issues in plot based analysis.

---

Interactive comment on Biogeosciences Discuss., 6, 1837, 2009.

**BGD**

6, S1064–S1067, 2009

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

S1067

