

Dear Trevor,

Please find enclosed a revised version of my manuscript “Title: Edaphic, structural and physiological contrasts across Amazon Basin forest-savanna ecotones suggest a role for potassium as a key modulator of tropical woody vegetation”. In response to referee’s comments:

Referee 1.

(S)he first raises some statistical issues which we do not necessarily agree with. As example, s(he) seems to hold to the common misconception that $p < 0.05$ = “significant” and $p > 0.05$ = “non-significant” is some sort of fixed rule. But if you read Fisher’s original paper (or any good statistics textbook) then it is immediately clear that $p = 0.05$ is actually just a suggested guideline as regard to specific hypothesis testing and - especially as regards to our context where we were merely describing simple trait *vs.* precipitation patterns rather than specifically testing hypotheses – then “only significant at $p = 0.08$ ” for example is just fine: “Significance” of course really being a linguistic shortcut for “the probability of rejecting the null hypothesis given that it is true”. So especially as we never make any claims about the general nature of the precipitation *vs.* trait relationships (i.e. that those we observed necessarily represent anything other than a transect specific pattern) then we think what we say and the way we say it at the moment is fine, Nevertheless, there were some admitted inconsistencies in style in this respect and **we have gone through the manuscript checking and amending out use of the words ‘significant’ and ‘significantly’ as appropriate. We have also made a specific comment in the text about how the precipitation versus trait patterns should not be regarded as suggesting some sort of general rule (lines 599-603).**

R1 also seems to be concerned that the results are somehow ‘regional specific’ and in the revised version we will be happy to provide more discussion on this issue, especially in relation to the conclusions of Lehmann et al. (2013). As we are currently in the process of preparing a separate critique of Lehmann et al. (2013) we have not added much in that respect; **But we do now mention what we believe to be the overall widespread applicability of our results in a new paragraph (starting line 597 of the revised version) .**

As for shortening the Discussion, what one reader finds to be “irresponsible speculation” another may well find an “interesting insight” and we see no reason to reduce the length of the manuscript at this advanced stage of submission – especially as this seems to be just for the sake of it.

We also do not think it wise to remove figures from the main text as R1 seems to be suggesting just because some panels do not show significant relationships. This is because, especially in a multivariate trait study such as this; the very lack of a clear association with a climate or soil measure can be just as instructive as is a statistically strong one.

Based on R1's comments regarding the complexity of the observed K effect **we have now solved this problem by removing what was actually a slightly illogical argument in the first place.**

Regarding specific comments made by referee 1:

1. Page 7889: We have checked Fig 4b and the regression is correct. In any case, not much is made of this in the Discussion itself (see also comment above in bold regarding generality of precipitation trends)
2. Page 7901: As stated before, we make little of these trends of Figure 7 in any case.
3. Page 7907: The colour pattern and size are perfectly explained in the Figure legend.
4. Page: 7908: This is actually shown in Fig 12a
5. Page 7912: Reference now included.

Regarding technical corrections suggested by Referee 1.

Each of these has been reviewed and modified as appropriate.

As regards Referee II (R2), we address his/her issues

es point by point:

1. We are surprised that R2 actually took that impression from our analysis as we never said that higher photosynthetic rates had anything to do with the potassium effect: Indeed as we show with our statistical analysis the effect of K on photosynthesis per unit area is actually negative and we say in the introduction, there are three primarily identifiable means by which nutrients could have an effect on vegetation structure of which only one involves photosynthetic C acquisition. **No action taken.**
2. In retrospect, we agree our thinking here could have been made more explicit. But in short, other things being equal, trees and/or will always win as they can shade out the grasses. Because of their structural requirements and need to operate at more negative water potentials (plumbing constraints compared with height requirements), trees require more K. **This has now been made clearer, not only through a specific mentioning of this point in the discussion (starting line 848 105 of the revised version), but also with this issue specifically raised in the Introduction as well (starting line 105 of the revised version).**
3. Here we have essentially a reference to Mills and Co "theory of biological energy intensity". We mention this in passing, but admittedly do not attempt to interpret our own data in that respect. This is because we did not want the paper to get bogged down into a critique of a theory that currently has only marginal acceptance in the literature. **Nevertheless we now include a brief discussion whether it could be that trace elements really are the cause of the results in the discussion (starting line 856 of**

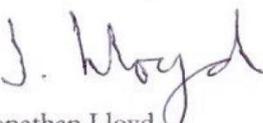
the revised version) with this issue similarly raised no in the Introduction as well (lines 101-119 of the revised version)

4. We discuss fire effects on soil physical and chemical properties briefly, but (noting that K is hardly likely to vaporise away in any sort of low-intensity savanna fire) are also happy to expand our discussion in that respect as would seem to be being requested here. Our own view is that effects of low-intensity savanna fires on soil properties are likely to be much less than for higher temperature whilst crown replacing fires. And, moreover, most savanna fire trials involve burning regimes which are significantly more severe than **We have now included a reference to the Mills and Fey paper (line 889).**
5. See (2)
6. See (3)
7. See (2)

Minor corrections as also pointed out as being required by this referee have also been incorporated.

We trust you will now find this manuscript suitable for publication.

Regards


Jonathan Lloyd