Biogeosciences
Open Access
Discussions

**Interactive
Comment**

Interactive comment on "Contrasting photosynthetic characteristics of forest vs. savanna species (far North Queensland, Australia)" *by* K. J. Bloomfield et al.

Anonymous Referee #2

Received and published: 11 July 2014

General comments

This manuscript presents an impressive dataset of functional traits measured on forest and savanna trees across far North Queensland, Australia, specifically, photosynthetic capacity, foliar Nitrogen and Phosphorus, leaf dry matter content, leaf density and leaf mass per unit area, as well as soil and climate data for the corresponding sampling plots. Bloomfield et al. found area-based photosynthetic capacity to be higher in savanna species, explained well by vegetation type and foliar Nitrogen and Phosphorus, and vary more between species than between plots. They conclude that savanna trees have the capacity to out-perform their forest counterparts in their study sites.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

The manuscript is well written but lacks some clarity and focus, especially a well-defined and discussed research question. I would also welcome a more detailed analysis of the environmental conditions at the sampling sites (soil and climate data seems to be available but has not been used explicitly to look at trait-environment relationships). Considering the wealth of data available, I think that the main aim (testing for differences between photosynthetic traits for trees growing in adjacent forest and savanna formations), could be better achieved.

Finally, some measurements, as well as an additional greenhouse experiment, are described in the results section but not discussed and, in the case of the experiment, not even described in the methods section.

Amending the manuscript to be more concise and structured, as well as including this lacking information and re-considering the conclusions will make this manuscript a valuable contribution to BG.

Specific comments

The introduction is informative and offers a good overview of the subject. Although it provides the reader with the necessary background, it lacks an engaging discussion of research gaps and does not lead up to the posed research questions. For example, several examples in the literature which show differences in the measured traits between forest and savanna trees are cited, as well as that "differences between the two vegetation types. . ... might reasonably be expected. . ..". One thus questions the value of the first posed question. I also feel that the first question is slightly redundant considering the third question (differences in the slope or intercept of the Amax – leaf nutrient relationships implying differences in photosynthetic capacity/nutrient use efficiency).

I got the impression that the third question posed in the introduction has not been answered. As far as I could see, Amax – [N] and [P] relationships are only presented at the plot level, not at the species level. The only exception is presented in the appendix (Table S4) but not discussed in the main text. Maybe make this clearer in the

description of your mixed effects model and the discussion and conclusions.

Question 4 is approached in the data analysis with some results presented in the Figures but I felt discussion of these results, especially in the light of your main conclusion (savanna trees outperforming forest trees in leaf area-based photosynthetic terms) was insufficient. In addition, Question 4 was not mentioned as one of the main aims of your study in the Discussion or Conclusions section.

I also found there to be a lack of explicitly including soil/climate data in your analyses. You explain in detail how important these factors are for your study subject in the introduction and even the second sentence of your abstract, as well as the discussion ("P8989 L24-26). According to table 1, there is wealth of soil data available for all sites. It thus seems straightforward to also take advantage of this data in more detail and look at the degree to which soils rather than "just" vegetation formation and PFT influence the measured traits. Looking e.g. at the strikingly different soil [P]ex in both, forest and savanna formations, plots could be separated into low- and high P soils rather than, or in addition to, vegetation formations.

The results section is very extensive and seems to include some maybe unnecessary detail. I found that I got lost about half-way through. Adding subheadings would help a lot to guide the reader. I also found that, whilst they are mentioned in table 2, Vcmax and Jmax results are not discussed in the results section (other than that "linear relationships were strong for both nutrients") or mentioned in the conclusions. Either the information provided by Vcmax and Jmax is superfluous and should thus be removed or otherwise, the results should explicitly be discussed. The same is true for the greenhouse experiment, which came as a large surprise, not having been mentioned in the abstract, introduction, method or, as far as I could see, any other part of the manuscript.

I also miss a more in-depth discussion of how much of the variability in photosynthetic capacity is due to differences between forest and savanna trees per se and how much

is likely to be an effect of species belonging to a different PFT as defined in your study, considering savanna trees as predominately sun exposed. For example, figure 5 suggests that savanna and tall pioneer forest trees are not significantly different in their photosynthetic N and P use efficiency. Only answering question 4 without considering the implications for questions 1 and 3 is in my opinion not a valid approach.

I would also consider adding sub-headings as well as re-structuring the Discussion section. For example, I found it confusing that you went back to discussing the influence of [P]a vs. [P]m at the very end of this section, after you have already discussed this subject in the second paragraph. You also spend a lot of space discussing the PFT results. This is certainly interesting but seems very secondary according to your abstract and conclusions. I recommend amending both sections accordingly.

The conclusions seem rather short and "simple" regarding the wealth of information presented. For example, there is no reference at all to the research question nr. 4 posted in the introduction.

Technical corrections

Please increase the font size in figures 1 and 2, as well as Table 1.

The caption in figure 1 is not clear. For example, you state that "in the left panels, the two V classes are distinguished by colou". However, this is true for all panels, right? Also, I can't see how "the box width in the right panels is proportional to the number of observations per V" – to me the widths are identical.

This manuscript is generally very well written, but please check the spelling again – I found some mistakes (e.g. P8972, L3: missing a word after "also" (being?); P8975, L7: in instead of "at Table S1"; P8976, L2: missing a word after "as" (e.g. it is)).

Please double-check references to figure labels, in some cases they were wrong (e.g., P8983 L22).

Interactive comment on Biogeosciences Discuss., 11, 8969, 2014.