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Interactive comment on "Variations of leaf N, P concentrations in shrubland biomes across northern China: phylogeny, climate and soil" *by* X. Yang et al.

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Dear managing editor, The 16th of March 2016 Dear authors,

The manuscript, entitled "Variations of leaf N, P concentrations in shrubland biomes across northern China: phylogeny, climate and soil" by X. Yang, X. Chi, C. Ji, H. Liu, W. Ma, A. Mohhammat, Z. Shi, X. Wang, S. Yu, M. Yue, and Z. Tang (BG 2015-414) proposes in a research paper to study how climate, soil and phylogeny explain the regional pattern in leaf N-P concentrations in China and the pattern components: compositional shift vs adaptive variation. With a stunning dataset where the abundance of species and their traits where measured for each of the 361 shrubland sites of the study, they

highlighted that leaf N and leaf P follows different drivers: while leaf N pattern seems to be more influenced by the compositional shift of the communities than by the adaptive variation of species to climate and soil, leaf P seems to respond to the opposite pattern with a particular adaptation to soil fertility in P. To my opinion, this result is important (unless jeopardized by the issue I explain below) for the scientific community in community and ecosystem ecology and will be relevant and opportune in time for the Biogeosciences journal. It also echoes what has been founded in previous studies in another part of the world in the Amazonian basin (Fyllas et al 2009; Quesada et al 2012 in BGS).

I see a particular major issue and some minor additional ones that I would like that authors pay attention of:

1- As environmental predictors, authors used temperature, precipitation, soil total nitrogen and soil total phosphorus. At first glance, it seems very logical to use these soil variables to explain leaf nitrogen and leaf phosphorus, respectively. However, when you considered recent papers on leaf N and P over biogeographical scales (Fyllas et al 2009 in BGS; Ordoñez et al 2009; Ordonez & Olff 2013; Maire et al 2015; Simpson et al 2016 in minor revision; all in GEB), you note that leaf N is not related with soil nitrogen (available or total), for several reasons (that I won't develop here). It regresses however on soil variables related with cation availability (soil pH, soil total available bases, soil base saturation, soil CEC…). In opposite, soil phosphorus is well correlated with leaf P. Considering major soil variables to explain the variation of leaf P but not considering them for leaf N should strongly impact the results like: leaf N is less well predicted by environmental variable than leaf P, intraspecific variability of leaf N is not dependent on environmental variables… I strongly recommend to consider this point. If data has not been measured yet, a first look can be attempted by getting soil pH from a regional database that has the same resolution than worldclim (Shangguan et al 2013 Journal of advances in modelling earth systems).

2- I find the introduction very interesting as well as the hypotheses that the authors want to test. However, I find hard to follow the link between hypotheses and data analyses in materials and methods – data analyses - and in the result sections. I suggest to improve the structure of MM and result sections so that they better fit the different hypotheses presented in the introduction.

3- Although the manuscript is written with a fully intelligible English, I would encourage a revision by a native English speaker. Some examples: dessert of desert, L10-11 on 18978 page . . .

More minor comments below:

- L10 on page 18975: In Reich & Oleskyn 2004, leaf N and P increase with latitude. Please correct it.

- L4-7 on page 18976: Meng et al 2015 could be quoted as an opposite example.

- L8-11 on page 18977: It is not clear why the plant physiology hypothesis does not consider P along an aridity gradient, while both nutrient are considered along a temperature gradients. Please make a clarification, on page 18975 for instance.

- L15 on page 18977: '. . . in contrast, N is relatively sufficient'. This part of the sentence does not make sense. Do the authors mean in this particular region? In general? I strongly advise the authors to consider Mayor et al (2015 ELE) paper showing that mycorrhiza can significantly contribute to N nutrition from mid to high latitude.

- Paragraph 2.1: It would be important to give an estimation of the shrub contribution to the community biomass (even if community is dominated by shrub).

- L16 on page 18978: Unless I make a big mistake, I think that WorldClim does not include a correction of temperature by altitude. Could the authors check this? It may be particularly important in the context of the study. If true, the authors can use CRU database that does that. Otherwise, the authors can apply the rule of thumb: -0.6°C change by 100m elevation change

- Paragraph 2.2: That is a good thing to have tested for N-fixer species. Could you do the same for succulent species? Could you, please provide some explanation on K interpretation?

- Paragraph 2.3: Symbolism is really confusing: 'i' means two different things in the equation: value of trait for each individual and mean value of trait for each species. I strongly recommend to use different symbols for each case.

- Paragraph 4: I would not recommend to use 'cheap' and 'expensive' rationale here. It is not wrong but one would need to know westoby and wright 2006 or wright et al 2004 studies to fully understand the meaning of it. In the context of the study, it could appear a bit contradictory to have a cheap strategy with lot of nutrients in leaves. You need to understand the link between leaf nutrient and leaf mass ratio / leaf lifespan to properly understand the terminology.

- L21-27 on page 18983: First, it is not clear if litter (leaf or root?) is included in total nitrogen or phosphorus. Second, increase in precipitation leads to increase in litter decomposition and in phosphorus availability, only when precipitation is lower than evapotranspiration, i.e. in the context of the study. When precipitation ≥ evapotranspiration, it is likely the opposite. Please add that the argument concerns the climatic context of the study.

- L27 on page 18983: it does not confirm the hypothesis but is in line with the hypothesis. Please change!

- L5-8 on page 18984: Please test this opposite effect of climate variables with multiple regression, it should give good indication on what's going on.

- L3-6 on 18985: Please correct Liu et al 2012 by Liu et al 2013. Unless I misunderstood, I do not find this result and statement in Liu et al's study. Please modify the text in consequence.

- Figure 1: Please consider using another colour than blue, which spots similar to green.

- Figures 2-3: community figures are not necessary and can be removed for at least two reasons: i) they are similar to figure showing all individuals ii) they are not related with hypotheses. Please consider using colours here to distinguish between the different ecosystem types.

- Figures 4: what is the meaning of bars in the figure? Please give more information in the figure caption

Conclusion I read your paper with great interest and I belief it is very relevant to Biogeosciences readership and useful for the scientific community. I strongly recommend to consider the soil available base content or soil pH in that study as it may greatly modify the results. Hope will the comments be useful.

Vincent

Interactive comment on Biogeosciences Discuss., 12, 18973, 2015.