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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.

X. Yang et al.

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Thank you very much for your constructive review and helpful suggestions on the manuscript. We have addressed all the comments and modified the manuscript. We hope you will find the revision to be satisfactory.

General comments:

The authors use a series of shrubland sites in northern China and investigate the relationship of leaf N and P concentrations to several environmental factors. In essence, the study attempts to assess the leaf “economics spectrum” (sensu Wright et al. 2004),

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but over a much smaller latitudinal gradient (i.e. within northern China). I think the paper has potential but needs reworking and additional analyses (it is perhaps an editorial decision as to whether that constitutes a minor or major revision, given the amount of additional work finally recommended). The key difference between the Wright et al. (2004) paper and this study is the large range of sites in the former. Wright et al. (2004) attempted to explore worldwide patterns; however, when the scale is considerably smaller (albeit large) other factors may confound the results.

Overall, I think the paper is limited by only looking at leaf N and P and only temperature and precipitation; other (leaf) traits are not touched upon, yet recent syntheses (e.g. Reich 2014, *J Ecol*) highlight the importance of other traits and factors, not just leaf N and P. I think it would be far better to take other traits into account in some way, not only because it brings it up to date with broader hypotheses (see Reich 2014) but also because with such varying habitats (desert through to alpine shrublands) these traits could affect the results. For example, plants at the “slow” end of the economics spectrum may have lower absolute concentrations of leaf N and P even if the ratios (N:P) are roughly similar. At the very least, these varying habitats should be accounted for. I suspect that a linear mixed model, treating the different type of shrubland as a random effect, would likely indicate that within each region there was limited, or no, effect and that the significant regressions observed were due to the changes from one region to the next. All of which is fine, but other factors certainly play a part in that. Some of those factors might be correlated with (co-linear to) the traits assessed (e.g. annual precipitation) but only additional analyses would reveal that. Considering additional factors/traits I think would place the study better in the journal’s stated scope. Furthermore, I would be surprised if the authors did not have additional data available to them to extend the analyses.

Response: Thank you for your comments. Your major concern is that we only considered limited leaf traits and environmental factors. I agree that other plant functional traits are very important. But our study focuses on the stoichiometry of N and P. Due to

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the large area our extensive survey covered and limited labors we had, measurements of functional traits that need to be conducted on site using fresh plant tissue were minimized. Instead, we kept dried plant samples for potential future measurement on other nutrient elements. Our study included temperature, precipitation, and soil nutrients as the environmental factors because of their strong influence on plant growth and leaf chemical traits. In addition to mean annual temperature (MAT) and annual precipitation (AP), we also analyzed the influence of mean growing season temperature (from May to September, GST) and mean growing season precipitation (GSP) as climatic factors during our preparation for the manuscript. However, the relationships of leaf chemical traits with GST and GSP are very similar to those with MAT and AP. Also, MAT and AP explained more variation than GST and GSP and they were used in some previous studies (Reich and Oleksyn 2004; Han et al. 2005; Liu et al. 2012; Chen et al. 2013). Another concern is that whether the observed pattern holds within each type of shrubland. We treat the different type of shrubland as a random effect, because our study region only covers two major types of shrubland (i.e. temperate shrubland and desert shrubland), and we did not randomly select sample types of shrubland from a larger population. To address your suggestion, we analyzed the temperate shrubland and desert shrubland separately using general linear models. The result of temperate shrubland was similar as that of using all data, indicating the same pattern holds within temperate shrubland. For desert shrubland, however, rainfall is the major factor influencing leaf chemical traits. We presented these result in Figure S3 and Table S3.

Specific comments:

Specific comments relating to the following pages and line numbers are as follows:

p. 18977 line 14,15: The Allen paper was concerned about the prevalence of ECM vs AM in different habitats (e.g., mesic vs xeric) and the generalisation “the infection of mycorrhizal fungi mainly depends on environments” is neither quite right or justified by that reference. line 15: “in contrast, N is relatively sufficient”. I don’t know quite what you mean here and I’m not sure how it relates to the Allen reference. p. 18986 line

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24-25: That's not a true statement and not what the Allen paper concludes.

Response: Thank you for the comments. We rephrased these sentences in the revised manuscript to make it clearer. We cited Koske's (1987) paper to illustrate that the colonization of arbuscular mycorrhizal fungi is largely influenced by environments, and cited Allen's (1995) paper to illustrate that AM fungi are not selective of host plant species. These evidences could support our third hypothesis that leaf P concentration might be less phylogenetically conserved than leaf N concentration, because the absorption of P by plants is greatly influenced by environment instead of plant identity.

p. 18978 line 15: It would be good to see the correlations/other data in a supplement for those depth intervals.

Response: We reported the Pearson correlations of soil total nitrogen (STN) and phosphorus (STP) concentrations between different soil intervals in Table S1.

p. 18980 line 22: It would be better to include the AIC values (in the supplement).

Response: We included the AIC values in Table S2.

p. 18984 line 18: These ratios are indicative only but further tests are needed to say for sure.

Response: We tested the N:P ratio using one sample t-test. The N:P ratio was significantly greater than 16 ($p < 0.001$), which indicates a significant P limitation. We included this result in the revised manuscript.

p. 18985 line 2: Available nutrients are not necessarily the best, as "available" measures are typically based on agricultural plants that often lack the root specialisations or root symbionts (mycorrhizal fungi, for example) that can allow access to other forms of N and P. That is, there is other literature to support the use of total elements.

Response: Thank you very much for the insightful suggestion. This is a very strong support of using total element. We included this point in Discussion in the revised

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manuscript, citing Aerts and Chapin (1999).

pp. 18975, 18976: The three paragraphs starting at line 14 could be condensed a little; structurally, it might all be better as one paragraph. p. 18976 line 14: “proved to prior to others” needs correction; there’s a wrong word there. line 15: insert “been” after “have” p. 18977 line 4: insert “the” after “have” line 6: “, plants” does not flow. I’m guessing you meant “and plants” line 7: Did you mean “soil P availability”? If not, you need to explain why “nutrient availability” in general would affect leaf [P]. line 11: “remarkably”, I presume you meant “markedly”. line 21: “expanding”, I suppose you meant “encompassing”. p. 18978 line 2: insert “then” after “were” line 4: “sites” should be singular. line 10: “leaves under 950C for combustion” needs rewording, e.g., “were combusted at 950...” p. 18980 line 10: “and explain them” needs rewording. I’d suggest something like: “with climatic and soil nutrient factors as explanatory variables”. line 13 and in other parts of the paragraph: tense should be the past tense, e.g. “We can then extract the SS” should be in the past tense. p. 18981 line 3, 4 and 5: This sentence is not really necessary. line 7: You don’t need to mention the base package because it’s loaded by default. line 10 and the Results section in general: Appropriate numbers of significant figures should be used – there are too many in some places. line 22, 23 : insert “a” before “significant” p. 18982 line 13: “Come to the” doesn’t make sense here. line 20: “focused” should be “focus” presumably. p. 18983 line 7: shrub should be “shrubs”. line 24: P does not diffuse well at all in most soils, but rather is usually quickly and tightly bound to soil particles. p. 18984 line 17: “is” should be “are” p. 18985 line 20: Better to change the tense here, e.g. “Climate influenced...” line 24: insert “a” after “exhibited” p. 18986 line 1, 2: That’s a big statement and as other factors weren’t included I don’t think it’s justified. line 14, 15: A somewhat obvious statement. line 18: nutrient should be “nutrients”. Additionally, given the results it would be better to suggest/surmise, e.g. “we surmise that the influence...”. Paragraph starting with line 15: The sentences in this paragraph to line 22 seem to be a bit of a rehash of the paragraph starting at line 7. p. 18987 line 10: “along climate” should be “along the climatic” p. 18993: Table 1. Appropriate numbers of sig. figures should be used

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(i.e. less in some places). It would be better to have only one header column, giving a taller table. “non-significance” should be “not significant”. p. 18994: Table 2. Again, appropriate numbers of sig. figures should be used. “non- significance” should be “not significant”. p. 18995: Figure 1. “Dessert” should be “Desert”. p. 18996: Figure 2. “Dessert” should be “Desert”. A dash between “community level” would be better. “logarithm transferred” should be “log-transformed”. p. 18997: Figure 3. “Dessert” should be “Desert”. A dash between “community level” would be better. In one of these figures (e.g. Fig 1), or all, the acronyms (AP etc) should be spelt out in the caption.

Response: Thank you for your corrections. We have corrected accordingly.

Reference: Aerts, R. and Chapin, F. S.: The mineral nutrition of wild plants revisited: A re-evaluation of processes and patterns, *Adv. Ecol. Res.*, 30, 1–67, 1999. Allen, E. B., Allen, M. F., Helm, D. J., Trappe, J. M., Molina, R. and Rincon, E.: Patterns and regulation of mycorrhizal plant and fungal diversity, *Plant Soil*, 170, 47–62, doi:10.1007/BF02183054, 1995. Chen, Y., Han, W., Tang, L., Tang, Z. and Fang, J.: Leaf nitrogen and phosphorus concentrations of woody plants differ in responses to climate, soil and plant growth form, *Ecography*, 36, 178–184, doi:10.1111/j.1600-0587.2011.06833.x, 2013. Han, W., Fang, J., Guo, D. and Zhang, Y.: Leaf nitrogen and phosphorus stoichiometry across 753 terrestrial plant species in China, *New Phytologist*, 168, 377–385, doi:10.1111/j.1469-8137.2005.01530.x, 2005. Liu, C., Wang, X., Wu, X., Dai, S., He, J.-S. and Yin, W.: Relative effects of phylogeny, biological characters and environments on leaf traits in shrub biomes across central Inner Mongolia, China, *J. Plant Ecol.*, 6, 220–231, doi:10.1093/jpe/rts028, 2012. Koske, R. E.: Distribution of VA mycorrhizal fungi along a latitudinal temperature gradient, *Mycologia*, 79, 55–68, 1987. Reich, P. B. and Oleksyn, J.: Global patterns of plant leaf N and P in relation to temperature and latitude, *Proc. Natl. Acad. Sci. U. S. A.*, 101, 11001–11006, doi:10.1073/pnas.0403588101, 2004.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/12/C9401/2016/bgd-12-C9401-2016-supplement.zip>

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

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