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# *Interactive comment on* "Global patterns of leaf nutrient resorption in herbaceous plants" *by* Zhiqiang Wang et al.

**Anonymous Referee #1**

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My major concern for this paper is its novelty. So far there are many, many publications addressing how nutrient resorption changes with latitude, temperature and precipitation etc. The question is really whether the results in this present ms. lead to a more full understanding of the global patterns and the mechanisms that create them. The methods for analysis, the results and the conclusions in this submission in effect do not significantly differ from existing publications. The statement by the authors ".....at a global scale is still inadequate" is not true.

In particular, the authors claimed that they found "similar patterns of NRE, PRE and NRE:PRE with respect to latitude, MAT and MAP " (page 2, line 43 and Figures 2-3).I am getting a little a bit confused with the latitudinal PRE trend because it is generally accepted that the tropics at low latitudes are P-limited. PRE, reflecting the soil nutrient



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availability, is expected to be negatively correlated with soil P and thus has high a PRE value at a low latitude, i.e., PRE does not increase but decrease with increasing latitudes.

Another concern is that the authors should show awareness of the shortcomings of the methodology due to that they focused on this study on " herbaceous species". Their calculations of nutrient resorption were not corrected by this leaf area, because senescent leaves shrink in herbs and therefore their concentrations can be higher. Furthermore, the authors did not correct their NRE/PRE by a mass loss correction factor (MLCF), especially important for herbaceous species. See how to do it by Vergutz, Ecological Monographs, 2012, 82(2):205-220.

There are lots of factors affecting nutrient resorption process. The authors only chose latitude (LAT), mean annual temperature (MAT) and precipitation (MAP). There is the possibility that temperature and precipitation are not the only factors varying at a broad scale. Soil characteristics are in fact more important because they are closely related to plant growth. AET might also be a more important climatic index than MAT/MAP to drive RE. Apparently, the authors have neglected these variables.

In Table 1, the authors conducted a multiple regression analysis. LAT and MAT (including MAP) are tricky metrics, as often MAT is auto-correlated to LAT and this auto-correlation needs to statistically identify. It is not LAT itself but LAT-related environment variables (e.g, temperature. . .) that exert effects on plants and ecosystems. Therefore it not suitable to conduct a 3-way ANOVA such as Lat*MAT*MAP. Moreover, using mean annual climatic indices does not consider monthly variations, especially in growing season. So the statistical analysis appears to be incorrect, or at least poorly justified. Figs 2-3 show quite clearly that NRE/PRE is not normal, with data points concentrated towards middle LAT/MAP.

More importantly, the predictive power, i.e., the coefficient of determination, is extremely low. Most r2 is less than 0.05, the lowest r2 with a significant P value is 0.01!!





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It is impossible to estimate with certainty the resorption efficiency of a certain LAT or MAT/MAP. It therefore appears unwarranted to claim that climate factors could affect nutrient resorption under future climate changes at a global scale. The authors should rather try to explain the large variability of resorption. The great variation and very low coefficient of determination in resorption efficiency make it impossible to predict the response of resorption in forests to global change. I doubt that a potential change in nutrient resorption efficiency with increasing MAT/MAP will have any implications for the response of resorption to global change.

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