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Interactive comment on "Soil carbon, nitrogen and phosphorus stoichiometry (C : N : P) in relation to conifer species productivity and nutrition across British Columbia perhumid rainforests" *by* John Marty Kranabetter et al.

Anonymous Referee #1

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GENERAL COMMENTS

Kranabetter et al. performed an interesting study in which they investigated the link between soil CNP stoichiometry, foliar stoichiometry and productivity in planted coniferous, perhumid temperate rainforests of western Vancouver Island. Their main findings were that (i) soil C:N ratio was the primary explanatory soil variable explaining variation in basal area and foliar stoichiometry, (ii) this link between basal area and soil C:N was species dependent, and (iii) besides N, P may also play an important role in limiting productivity of these forests. The study falls well within the scope of Biogeosciences, since it for example encompasses biogeochemistry and plant-soil interactions.

In addition to further comments below, I suggest three main points for revision of the manuscript:

- A lot of attention is given to the potentially important role of P. Although the authors have attempted to provide support for their statements on P (co-)limitation based on multiple lines of evidence (relatively high foliar N:P, high soil C:Po vs low soil C:N, low soil Pi, low leaf P, literature + comparison with data from other parts of the island), I am not yet fully convinced. For example, there was only mediocre evidence for a role of soil C:Po in explaining variation in basal area (and the apparent + effect that occurred may have been caused by a mere confounding effect of soil C:N, as acknowledged by the authors), and no link between leaf P and soil C:Po, in sharp contrast to its analogue with N. I suggest to reinforce the evidence by performing some additional analyses you can do based on the data you already have (e.g. test for effects of Pi, test soil C:N*C:Po if no collinearity, plot foliar N:P vs forest floor and mineral soil C:N), and by referring better to any existing literature discussing P limitation on this region of the island, or other comparable systems. I provide some more details in the specific comments below!

- The manuscript has a strong focus on these particular perhumid forest ecosystems of western Vancouver Island. The eventual paper might attract a broader audience if more reference is made to other possible P (co-)limited coniferous and temperate ecosystems worldwide. One short section in the Discussion, plus perhaps mentioning it in the Abstract and/or Conclusion may suffice. In the Introduction, the authors explain why the forest ecosystems of Vancouver Island are unique, but still I think some parallels can be made with other forests globally.

- Language is generally OK, but at some places, it can be improved. In combination with some of my suggestions, I am confident that if all authors carefully read through the manuscript in the end, vocabulary and grammar will be good.

SPECIFIC COMMENTS

Line 5 – Consider changing the title to reflect the main conclusion of this work, e.g. the different responses among tree species to variation in soil stoichiometry, or on the possible importance of P (the latter only if evidence is strong enough, see my other comments).

Line 6 – I assume that "nutrition" refers to foliar stoichiometry?

Line 26 – "We described the nature of soil organic matter (...)": this is a bit confusing, could also refer to SOM properties not measured in this study. I suggest you immediately indicate that you determined the forest floor thickness, and forest floor + soil nutrient concentrations and stoichiometry.

Line 31 – At this point, it is not clear whether you refer to forest floor or mineral soil C:N. In practice, both explained well spatial variation in basal area. I suggest to specify that.

Line 36 – "(...) no evidence via foliar nutrition for increased P availability with declining element ratios (...)": this refers to the lack of a relationship between foliar P and soil C:Po ratio. Looking at Table 4 and Fig. 5b, this seems to be correct definitely when combining species. But maybe there is a significant correlation between foliar P and soil C:Po within species? Foliar stoichiometry is typically strongly taxonomy-dependent (Sardans et al., 2015).

Line 36 – Throughout the manuscript, C:Po ratio is used instead of C:P ratio. The rationale behind this is mentioned in the text, and the text also explains that results for C:Po and C:P are very similar. I suggest to at least once also mention C:P in the Abstract, and making clear that in this case results are very analogous anyway.

Line 40 – A lot of attention is given to the potentially important role of P, but it was not possible to detect a clear effect of soil C:Po ratio on foliar P, nor productivity. My feeling is that this is to a great extent because N is still the primary nutrient limiting

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productivity across most of the gradient, yet I agree that P may become more important as a limiting factor at plots with low C:N. Since soil P availability is strongly influenced by soil pH, and pH seems to have been measured at all plots, you may consider testing relationships between productivity and pH, foliar P and pH, soil C:Po and pH, soil Pi and pH, ... Since curves of P availability vs pH typically show an optimum, first try fitting a quadratic function (although pH is generally low in this dataset and may eventually be below the optimum anyway).

Line 80 – "Baseline relationships in soil resource stoichiometry and ecosystem productivity should also consider the interaction of tree species.": I agree, and this is also the case for foliar stoichiometry. While different species are shown in Fig. 5, the analyses in Table 4 do not test for the interaction between soil stoichiometry and species. Why?

Line 110 - A lot of attention is paid to the different responses among species, and a distinction between ECM and ARB trees is made. Moreover, in the discussion, a link with CSR strategies is made. I agree with the authors that these differential responses should be discussed, and I support the idea of explicitly stating a hypothesis or objective on this aspect. However, I would like to see a bit more of an explanation (in Intro and/or Discussion) of why you expect responses to differ between ECM and ARB. Some elements are in the text, but for example, in general, ectomycorrhizae are rather associated with enhanced N uptake, whereas arbuscular mycorrhizae rather for the uptake of P, which may influence (hypothesized) slopes like those in Fig. 4b. Having said that, these ECM-N vs AM-P links may be an overgeneralization; you for instance already mention the potential role of arbuscular mycorrhizae in N uptake in the manuscript.

Lines 118-124 – Add this information to Table 1 (or make a Table S1 in SI and refer to it). Then it is clear which site has what conditions. While this is not relevant to the main messages of the manuscript, it may be practical in case researchers want to use the data of the paper in the future, e.g. for reviews, meta-analyses, ...

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Line 140 – Add forest floor depths to Table 1 or S1.

Line 156 – ! molar ratios ! In terrestrial ecology, some studies use mass-based ratios, others use molar. Please clarify that molar ratios were used at least in the description of every table and figure.

Line 232-233 – Foliar N:P ratios are used in the manuscript as one line of evidence suggesting P (co-)limitation. However, caution is needed when using such critical N:P ratios, since they depend on species. Also, I did not immediately find the proposed threshold of 16 in the given reference Güsewell et al., 2004. As explained under "general comments", please try to find some stronger evidence for (co-)limitation of P. Then for me, mentioning a critical foliar N:P ratio can remain in the manuscript if justified (but you note the taxonomy-dependence), but it should be one piece of the evidence, together with other arguments.

Line 236 – For clarity, consider subdividing Discussion into sections with titles, like in Results or referring to the three objectives.

Line 238 – Like in the Introduction, the authors refer here to "high C and N regimes". In contrast to what was written in the Introduction, however, the sentence here discusses TOTAL N, whereas in the Introduction, reference is rather made to the AVAILABILITY of N. I suggest to (i) rephrase the vague mentioning of "high regimes" (e.g. total C and N concentrations were high), and (ii) not use total N as an argument to suggest that P may be as or even more limiting than N. Only a small proportion of the total N is plant-available, in the form of small organic molecules, ammonia and/or nitrate.

Line 256-258 – The cited synthesis paper (Booth et al., 2005) indeed focuses on the link between C:N and N availability, among other things. It however not explicitly refers to the influence on soil C:N ratio on stand productivity and/or foliar N. I suggest adding a few references of gradients/large-scale studies exploring Productivity/foliar N $\sim$ soil C:N ratio, e.g. Alberti et al., 2014; Van Sundert et al., 2018, ... –> "The clear relationship between mineral soil and forest floor C:N with stand productivity and foliar % was

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consistent with many other biomes (NEW REFERENCES) and (...) with declining soil C:N (Booth et al., 2005)."

Line 276 – "(...) we found it more effective to gauge P availability through soil Pi concentrations (as the only significant correlate with foliar P %)": overall, the evidence for P (co-)limitation based on soil C:Po is limited (except from the facts that C:Po was comparatively high, and it had a significant + influence on basal area in Table 3, but a confounding effect with soil C:N cannot be excluded). Table 4 confirms the potential of Pi to explain variation in foliar P instead. So, why didn't you further check whether Pi was perhaps a better indicator of the soil P status than soil Po or C:Po? Would spatial variation in Pi, even within plots, be too high, and also seasonal variation, as can be argued for available N (depending on the application)? I suggest you to either perform additional analyses using Pi, or explain in the manuscript why it is not a suitable indicator.

Line 336 – I strongly support your reference to additional nearby fertilizer application studies. However, to what extent are soils on N Vancouver Island comparable to W Vancouver Island, where the current study was performed? Earlier in the Discussion, you note based on your own data that the East of the island at least has soils differing from those in the West, as reflected in different foliar P.

Line 352 – See also my earlier comment. Inorganic P was generally in low supply, and contributed a relatively minor proportion of total P. Please perform additional analyses on basal area vs Pi etc., or explain why this would not be a good idea.

Table 2 – Please include Pi or argue why not.

Table 3 – Please include Pi or argue why not.

Table 3 – Why exactly was the 0-20 cm interval used for sampling mineral soil? Is this roughly corresponding to the main rooting zone? Please specify in the M&M section.

Table 4 – It would be interesting to see how foliar and soil stoichiometry relate within

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species. Separate species were visualized in Fig. 5, but separate analyses (or analogous: soil*species interaction) were not performed. You could show that not only the link basal area $\sim$ soil stoichiometry depends on species, but also foliar stoichiometry $\sim$ soil stoichiometry. If you make a new table for this, perhaps place it in SI, and refer briefly to it.

Figure 4b – I do not understand why mineral soil C:N was preferred here as a predictor over forest floor C:N. Table 3 suggests both are good explanatory variables. Please add and discuss panels using forest floor C:N, or explain why mineral soil C:N is the better alternative.

Figure 4b – Add test statistics (P, R^2, ...)

Figure 5 - Why was foliar N and P chosen, and not foliar C:N and C:P (here and throughout the whole manuscript)?

Figure 5 – Figs. 4 and 5 confirm to me the role of N as a primary determinant of forest structure and function. In order to find stronger evidence for the role of P, you may consider (i) testing the interaction soil C:N*soil C:Po on basal area, and perhaps other response variables, and (ii) plotting foliar N:P (within and among species) vs mineral/forest floor C:N. My feeling is that (i) may fail, because soil C:N and C:Po may induce collinearity in the statistical model. If so, try checking the single influence of soil C:Po for data points only where C:N is low ($\sim$ high N availability). For (ii), we may expect an increase in foliar N:P, and thus P (co-)limitation, with decreasing soil C:N.

Figure 5b – I would be curious to see this graph with Pi as an explanatory variable (cf. Table 4).

TECHNICAL CORRECTIONS

Line 5 – Perhaps the abbreviation C:N:P is not necessary in the title, or only provide the abbreviation and not the full words at this place.

Line 35 – "(...) no evidence via foliar nutrition (...)": please rephrase.

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Line 36 - “(...) no evidence via foliar nutrition for increased P availability with declining element ratios as we did for N.”: “declining element ratios” is vague; this refers to soil C:Po ratio in the first place. Please rephrase.

Line 52 – “the N regime in certain soils can be extremely rich”: somewhat weird way of expressing that some soils can be rich in available N = exhibit high N availability. Please rephrase.

Line 63 – Remove the word “global”.

Line 80 – “Baseline relationships in soil resource stoichiometry and ecosystem productivity should also consider the interaction of tree species.”: you mean the statistical interaction between soil stoichiometry with species. As written now, with the word “of”, it may seem as if the paragraph would discuss biological interactions between species. I suggest to replace “of” by “with”.

Line 188 – Sometimes P-values are given along with the correlations in brackets, sometimes not. Please add P-values everywhere.

Line 277 – Remove the “%”, from the data it is clear that foliar P is expressed in % (also apply this to analogous cases elsewhere).

Line 317 – “The small difference in forest floor N concentrations under Douglas-fir”: you mean it was higher than for the other species. Please rephrase.

Line 601 – At some places in the manuscript, Latin names were used, yet at others, tree species were named in English. Please use one of the two throughout the manuscript, including Tables and Figures.

Figure 1 – Please add “mineral” to the titles of both vertical axes.

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