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Referee comment on “Carbon, nitrogen and phosphorus stoichiometry of organic matter in Swedish forest soils and its relationship with climate, tree species, and soil texture”

Anonymous Referee #1

Referee comment on "Carbon, nitrogen, and phosphorus stoichiometry of organic matter in Swedish forest soils and its relationship with climate, tree species, and soil texture" by Marie Spohn and Johan Stendahl, Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-346-RC1, 2021

GENERAL COMMENTS

Spohn and Stendahl collected organic matter P data to complement a dataset of the Swedish Forest Soil Inventory. Based on these updated SFSI data, the authors investigated links between organic layer + mineral soil SOM C:N:P stoichiometry, vs climate, tree species and soil texture. Main findings of the study were that SOM C:N ratio differed among the dominant tree species, more so than was the case for the C:OP ratio. Especially in the deeper soil, the relationship between C:OP and species was weaker than for C:N, because of stronger associations of OP than N with finer soil particles (i.e. influence of texture, rather than assumed plant-soil feedbacks). N:P ratio increased with MAT, with potential implications for relative element availabilities. The study can be classified under the field of biogeochemistry, and assumes plant-soil feedbacks, which perfectly match the scope of *Biogeosciences*.

The manuscript is concise, very well structured, and easily readable with its subsections. I really appreciate the attention for P in a region where the nutrient is usually neglected because N limitation is assumed. Studying P, even in Nordic forests, is relevant because P can influence ecosystem function through interactions with other biogeochemical cycles, and P can be (co-)limiting in valleys and some sub-regions. I have the following major comments to be addressed, further explained in the ‘specific comments’ section:

- The manuscript is concise and the authors remain close to the data in the Discussion. This avoids too much speculation, but I do strongly recommend writing a short 1-2 paragraph section 4.6 with implications the authors see for future research. For example: what is the relevance of the findings in the context of nutrient availability/limitation research? See also the specific comment on *Line 356*, ...

- Some choices on data selections, classifications and statistics are not well motivated or explained. For example, were mull-humus soils just absent, or excluded? Why? Why were mixed forests categorized with spruce for the multiple regressions? Why were some positively-skewed variables log-transformed for the analyses and others not? Answers to these questions potentially (co-)explain some of the patterns I can see in the figures. See specific comments for more detail and further examples.

SPECIFIC COMMENTS

Line 13 – This study focused on soil organic matter C:N:P stoichiometry. Therefore not total P (TP), but organic P (OP) was reported on for the mineral soil. However, from the Methods section I understood that actually also TP and inorganic P (iP) were determined.

If TP were used instead of OP, would conclusions remain similar? This can be relevant for comparison to other studies and datasets that determined TP, and sometimes not OP: e.g. Bo et al., 2020 – Forests, Hume et al., 2016 – Forest Ecology & Management, Kranabetter et al., 2020 – Biogeosciences.

Line 20 – "C:N ratios in the litter layer and mineral soil". I assume that "litter layer" should be "organic layer" since litter layers were excluded during sampling, according to *Line 100*.

Line 92 – Does "deciduous" refer to certain dominant species? Betula pendula? Does the dataset include some of the temperate Fagus sylvatica dominated forests in southern Sweden?

Line 110 – Mor and moder humus forms were selected, which excludes peatland. Were forests with a mull-type of humus, with no separate H layer but Ah layer also excluded? If so, was this for practical reasons, e.g. no real separate organic layer, and could that have biased any of the conclusions?

Line 148 – Why were mixed forests pooled specifically with the spruce-dominated forests for the multiple regression analysis?

Line 149 – State that right-skewed variables were (natural?) log-transformed. This seems to be mostly done (as stated in the Results), but in a few graphs I still noticed at first sight + (right) skewed variables so that potentially not all model assumptions were met. See my comments there.

Line 193 – There were only 10 data points for deciduous forests. To what extent are these representative for deciduous (Birch, Beech, ...) forests in the whole country with respect to the variables measured and conclusions we derive from the data?

Line 216 – I became a little confused here about which statements on C, N and P referred to concentrations, and which to stocks. Please explicitly state in this paragraph.

Lines 262 and 270 – I agree that N2 fixation is ultimately responsible for increasing N stocks along the temperature gradient, but it is only part of the explanation for decreasing SOM C:N. What I miss here in this section is a reference to other microbial processes and plant-soil feedbacks. The latter you actually mention in the next two sections.

Where it is warmer, not only N fixation rates are higher, but also N mineralization rates. Consequently more N becomes plant-available per unit of time, and plant tissues and litter will also have reduced C:N. The more N-enriched litter will then result in lower C:N organic matter. Some studies on plant-soil feedbacks and stoichiometry in boreal and global forests (not specifically along temperature gradients) are Hume et al., 2016 – Forest Ecology & Management; Shi et al., 2016 – Plant and Soil; Van Sundert et al., 2021 – European Journal of Forest Research.

Line 282 – “high N inputs can lead to P limitation in south Swedish forests”: yes, and as you state in the first paragraph of the manuscript, P has mostly been neglected in boreal forests because of primarily widespread N limitation. But despite N limitation, considering P can be important for ecosystem function as shown by the first author in earlier studies. And some forests in Sweden, in the southwest and in valleys can be relatively N rich but poor in available P (Giesler et al., 1998 - Ecology). I suggest the authors to add a short section of 1-2 paragraphs at the end of the Discussion where avenues for future research are mentioned. See also my comment there.

Lines 302 and 310 – “spruce tends to grow in more fertile soils than pine” and “first study to show that this difference in C:N (...) is also visible in the mineral subsoil, in a depth of 55-65 cm”. This is indeed one of the first studies to show this result, +/- in contrast to for example Cools et al., 2014 at a Europe-wide scale. Differences in deeper soil C:N may occur because of belowground litter inputs and root activity, but isn’t an alternative explanation at least as likely here: the large majority of these forests are plantations, and spruce is planted on already more fertile soil than pine. So perhaps deeper soil C:N was already lower for spruce than pine before planting, even if this occurred >= 60 years ago. Also, mention in the Methods section what percentage of the forests was natural vs planted.

Line 340 – “litter layer” should be “organic layer” in this paragraph? If litter C:N:P stoichiometry was determined – which does not seem to be the case – some results could be added to the supplement to support discussions on plant-soil feedbacks elsewhere.

Line 340 – References are missing in this short paragraph, and the sentences are a bit unclear. Please rephrase. For example, the second sentence appears to suggest that some bedrock can provide nitrogen (such bedrock does exist, e.g. Holloway & Dahlgren, 2002 – Global Biogeochemical Cycles), but this could be a grammatical issue.

Line 356 – I warmly recommend to add a 1-2 paragraph section 4.6 with "avenues for future research" or alike. Here, relevance and implications of the research can be further emphasized and discussed. For instance, can such newly available soil P and CNP stoichiometry data help in better quantifying regional and global-scale nutrient availability and limitation, defined and determined as in e.g. Van Sundert et al., 2019 – Global Change Biology? How can understanding of soil CNP gradients help in advancing global change research? ... Putting the research in such contexts can be of interest for the broader readership.

Line 360 – Here, explicit mention is made to N:P AVAILABILITY. I agree with the statement, and some explanation of NP availabilities was written under section 4.2, but please explain a bit more in the newly suggested section 4.6. For example, while not the focus of this stoichiometry study, do you think that the iP and TP data could be useful for large-scale studies with more focus on N vs P availability?

Line 365 – Suggestion to not refer to numbered hypotheses ("in agreement with the third hypothesis") in the Conclusion.

Line 384 – I agree, the value of newly collected soil P data can not be overstated!

Line 525 – Maybe showing correlation coefficients would be more useful than R^2 so that the reader sees the sign of change.

Table 1 – In the Abstract and text, the litter layer is mentioned about twice. Probably "organic layer" was meant there but if not, please add litter nutrient concentrations and ratios to the tables.

Table 1 – Very good that "molar" was explicitly mentioned, as in terrestrial ecology/biogeochemistry mass-based ratios are also common (e.g. Hume et al., 2016 – Forest Ecology and Management; Manzoni et al., 2010 – Ecological Monographs; Vejre et al., 2003 – Soil Science Society of America). Was there a specific reason to opt for molar ratios?

Table 2 – No interactions were tested among the explanatory variables. Why? Were there à priori reasons based on theory to exclude interactions from the analyses? Statistical

reasons w.r.t. the dataset?

Figure 2 – Some variables seem positively skewed, which can lead to violations of the homoscedasticity assumption of linear regression. Have you considered log-transforming, as you did elsewhere? Log-transforming may resolve heteroscedasticity for the soil variables (effectively + skewed?) but not growth (panel 2d). For at least the latter I consider this acceptable because the regression line still passes through the middle of the point cloud, and tree stem growth was not the primary focus of the study.

Figure 2b – assuming the relationship holds under log-transformation - Would you actually expect soil organic layer stocks to increase with MAT? Litter production (input) increases north-south, but also decomposition (output). Do the southern, warmer region data points represent more wet-soil (but not peaty) areas? Maybe this region has some drier sites without organic layer (e.g. mull humus) which were excluded from the dataset? ~bias? If so, this would not invalidate the main conclusions of the study but it may be important for interpretation of some results.

Figure S3 – [although correlations]: heteroscedasticity --> log-transformation of variables?

TECHNICAL CORRECTIONS

Line 18 – "(...) MAT, almost twice as much as the organic layer stock increase along the MAT gradient."

Line 25 – "Further, we found that (...)"

Line 55 – "in Swedish forest soils"

Line 92 – "the following classes based on basal area:"

Line 132 – "P in ignited and non-ignited samples"

Line 147 – R version 4.1.1 is not from 2003. To get the most up to date citation for R you can use the citation() function of the statistical program.

Line 185 – "the C:N ratio of the mineral soil in PINE forests was on average 1.8 times higher"

Line 189 – "The C:P ratio of the organic layer in PINE forests was on average 1.3 times higher"

Line 127 – "Williams and Saunders (1956)"

Line 227 – "We analyzed the relationship between" (or relationships among)

Line 270 – "the C:N ratio decreased": the relationship was significant, so I would remove the "tended to", also in light of earlier studies with more data points by the last author of this manuscript (Van Sundert et al., 2018 - Biogeosciences).

Line 274 – "N:P ratio INCREASES with increasing MAT"

Line 279 – "foliage N:P ratio INCREASES with increasing N inputs"

Line 281 – "it has was suggested" Please correct.

Line 305 – "some of the pine forests had"

Line 337 – "charged surfaces"

Line 360 – "N:P ratio of the organic layer INCREASED substantially with increasing MAT, likely due to an INCREASE in the ratio of N:P availability with increasing MAT"

Line 362 – Please check grammar/structure of this sentence: "(...), as hypothesized, however, not (...)"

Line 565 – "temperature together with the N stock"

Table 1 – remove space: “0.13, p<0.001”

Figure 1 – “Map depicting mean annual temperature (MAT), ...”