Answers to reviewer#1

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. *We thank the reviewer for positive evaluation of the manuscript and the detailed comments.*

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, ...*

We added a section called 4.6 Future research questions in the end of the Discussion that reads as follows:

"4.6 Future research questions
Based on the results gained from the analysis of the forest soil inventory, we identified the following questions that should be studied in the future.
First, our finding that the N:P ratio of the organic layer increased strongly with increasing MAT and the atmospheric N deposition rate raises the question if growth of trees in Scandinavia at sites with high atmospheric N deposition is limited by P, and if so, to what extent.
Second, future research should study the temperature-dependence of N2 fixation in forest soils in Scandinavia along MAT gradients to investigate to which extent temperature-dependence of N2 fixation explains the change in N stocks along the MAT gradient observed here
Third, the result that the OP concentration in the mineral soil depends strongly on soil texture, which is likely due rigid adsorption of OP compounds on soil minerals calls for future investigations of (a) the role of OP for the sorptive stabilization of SOM and (b) the turnover of the soil OP pool in relation to the soil organic C pool.
Fourth, we speculated that very high C:N ratios in some pine forests might be related to forest fires. Future research should explore the legacy of forest fires on SOM stoichiometry.
Fifth, the negative relationships found between organic layer stock and organic layer P concentration raises the question if and to which extent P limits organic matter decomposition in Scandinavian forests.
"

- 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.

We added some more explanations throughout the Material and Methods section:

We excluded plots with the humus form mull because at these plots, the organic layer is not sampled separately from the mineral soil in the SFSI. We now added this information in the Material and Method section.

In the reviewed version of the manuscript, we had tried to include the variable dominant tree species into the multiple regression analysis, by assigning numbers to the trees species. However, we now removed this aspect from the manuscript.

We admit that the log-transformation was not consistently done in the reviewed version of the manuscript. We now corrected this and added the following explanation to the Material and Method section. "For the regression analyses, all soil chemical variables (element stocks, element concentrations, and element ratios) as well as the organic layer stock and the P concentration the parent material were transformed by calculating their natural logarithm (log-transformation) since they tended to be not normally distributed but were right-skewed. In addition, also the variable atmospheric N deposition was log-transform prior to regression analysis because it was not normally distributed." Further, we removed the results of the regression analysis from Fig. 1 which shows the linear models for the non-log transformed variables, for the sake of clarity. We like to show the non-transformed data in Fig.1 because the log transformed data is not necessarily helpful for understanding the relationships between the variables.

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. *Yes, for the mineral soil, we determine total P and organic P. However, in line 13 we explain the aim of the study (which is to investigate the ratios of C, N and P in the organic matter). It does not read well if we squeeze"total P" also into this sentence, and it does increase clarity, either.*

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*. *Right, we corrected this.*

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? *We added the information that deciduous forest refers to birch, aspen, beech or oak forest.*

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? *We excluded plots with the humus form mull because at these plots, the organic layer is not sampled separately from the mineral soil for the SFSI. We now added this information in the Material and method section.*

Line 148 – Why were mixed forests pooled specifically with the spruce-dominated forests for the multiple regression analysis? *In the reviewed version of the manuscript we had tried to include the variable dominant tree species into the multiple regression analysis, by assigning numbers to the trees species. However, we now removed this aspect from the manuscript.*

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. *We now log-transformed all soil chemical data and the variable atmospheric N deposition before the regression analysis. Further, we removed the results of the regression analysis from Fig. 1 which shows the linear models for the non-log transformed variables for the sake of clarity.*

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? *We added the information that of the 10 deciduous forests, five were dominated by beech and oak forest.*

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. *We now indicated this clearly throughout the 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. *We added the following lines to the manuscript. "Mineralization of N is also expected to increase with increasing temperature, and hence should counteract the accrual of N with increasing temperature. Our finding that the N stock of the organic layer increased with increasing MAT thus suggests that MAT has a larger positive effect on* N_2 *fixation than on net N mineralization."*

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. *We added another section about future research questions in the end of the Discussion (see above). Further, we included the mentioned reference in the Discussion*

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. *We added the following information at the end of section 2.1: "Of the 309 sites, 119 sites had a stand age >120 years, and are* *thus generally classified as old growth forests (not planted). Further, 25 sites were on formally set-aside land" Furthermore, we added the following sentence in the middle of section 4.2. "Yet, it has to be considered that we cannot clearly attribute the differences in stoichiometry to differences in vegetation since pine forests might have been established preferably on soils that already had nutrient poor SOM."*

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. *This was a mistake. We replaced "litter" by "organic".*

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. *We clarified the sentence and added a reference. It now reads as follows: "In addition, regarding the N concentration it could also be that fine-textured soils are commonly formed from nutrient-rich (potassium, phosphorus, magnesium, etc.) minerals which causes high plant productivity and N_2 fixation, resulting in higher N concentrations in fine-textured soils compared to coarse-textured soils (Clarholm and Skyllberg, 2013). This is likely also the reason for the higher N concentration of the organic layer in the fine-textured soils compared to the coarse-textured soils (Fig. 4f)."*

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. *We added another section about future research questions in the end of the Discussion (see above).*

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? *We added another section about future research questions in the end of the Discussion (see above).Further, we also now included total P in the regression analysis.*

Line 365 – Suggestion to not refer to numbered hypotheses ("in agreement with the third hypothesis") in the Conclusion. *The rationale behind this suggestion is not clear to us. We think that it is good to number the hypothesis because it allows it to refer clearly to one specific hypothesis.*

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

Line 525 – Maybe showing correlation coefficients would be more useful than R^2 so that the reader sees the sign of change. *We added information on whether the variables were positively or negatively correlated in Table 1.*

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. *This was a mistake. We replaced "litter" by "organic".*

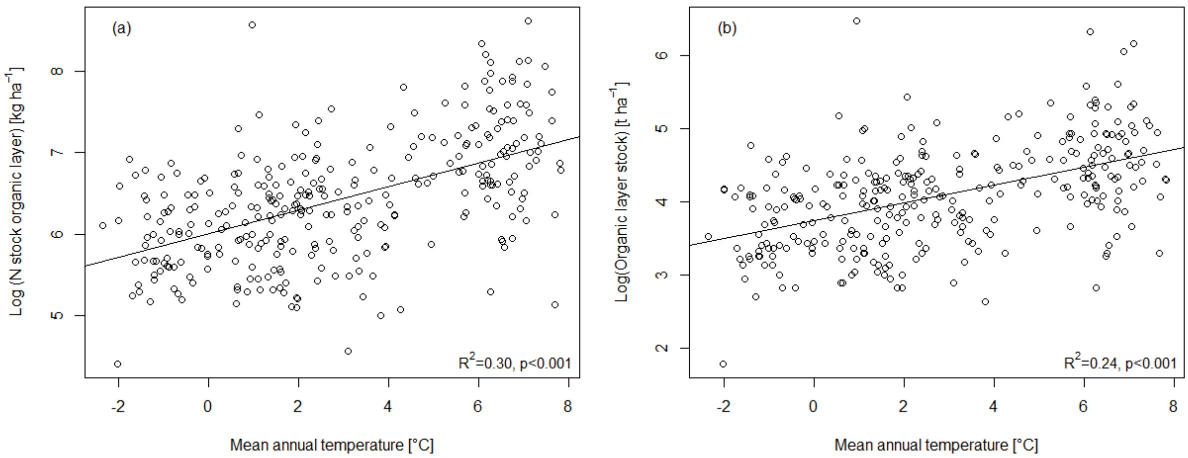
Table 1 – Very good that "molar" was explicitly mentioned, as in terrestrial ecology/biogeochemistry mass-based ratios are also common (e.g. Hume et al., 2016 – Forest Ecology and Management; Manzoni et al., 2010 – Ecological Monographs; Vejre et al., 2003 – Soil Science Society of America). Was there

a specific reason to opt for molar ratios? *The reason to opt for molar ratios was that most papers about organic matter stoichiometry report element ratios on a molar basis. Hence, this facilitates comparison.*

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? *We now added the results of additional multiple regression analyses that consider interactions between the independent variables in Table 2.*

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. *We corrected this. For the regression analyses, all soil chemical variables (element stocks, element concentrations, and element ratios) as well as the organic layer stock and the P concentration the parent material were now transformed by calculating their natural logarithm (log-transformation) since they tended to be not normally distributed but were right-skewed.*

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. *The relations holds under log transformation:*



We added the following sentence to the Discussion "Similarly, the increase in the organic layer stock with increasing MAT (Fig. 2b) suggests that the MAT has a larger positive effect on plant productivity (Fig. 2d) than on decomposition. This in accordance with other studies showing stronger C accumulation in the organic layer in the South than in the North of Sweden (Akselsson et al., 2005)."

Figure S3 – [although correlations]: heteroscedasticity --> log-transformation of variables? *Now, in the revised manuscript, all soil chemical variables (element stocks, element concentrations, and element ratios) as well as the organic layer stock and the P concentration the parent material are transformed by calculating their natural logarithm (log-transformation) since they tended to be not normally distributed but were right-skewed. This applies also to Fig. S3.*

TECHNICAL CORRECTIONS

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

Line 25 – "Further, we found that (...)" *We deleted the comma, as suggested.*

Line 55 – "in Swedish forest soils" *We deleted one s.*

Line 92 – "the following classes based on basal area:" *We replaced ; by :*

Line 132 – "P in ignited and non-ignited samples" *That's what we wrote. This comment is not clear to us.*

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. *It is from 2021. We corrected this.*

Line 185 – "the C:N ratio of the mineral soil in PINE forests was on average 1.8 times higher" *Thanks! We corrected this.*

Line 189 – "The C:P ratio of the organic layer in PINE forests was on average 1.3 times higher" *Thanks! We corrected this.*

Line 127 – "Williams and Saunders (1956)". *Yes, we added a s.*

Line 227 – "We analyzed the relationship between" (or relationships among) *We corrected this.*

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). *Done*

Line 274 – "N:P ratio INCREASES with increasing MAT" *Thanks! We corrected this.*

Line 279 – "foliage N:P ratio INCREASES with increasing N inputs" *Thanks! We corrected this.*

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

Line 305 – "some of the pine forests had" *We added a s.*

Line 337 – "charged surfaces" *Done*

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" *Thanks! We corrected this.*

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

Line 565 – "temperature together with the N stock" *We added a s.*

Table 1 – remove space: "0.13, p<0.001" *Done.*

Figure 1 – "Map depicting mean annual temperature (MAT), ..." *Thanks! We corrected this.*

Reference

Akselsson, C., Berg, B., Meentemeyer, V., and Westling, O.: Carbon sequestration rates in organic layers of boreal and temperate forest soils—Sweden as a case study. Global Ecology and Biogeography, 14(1), 77-84, 2005.