Biogeosciences Discussions

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Comment on bg-2021-346

Anonymous Referee #2

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-RC2, 2022

This study reports variation in soil C, N and OP stocks and stoichiometric ratios in organic layers and C, N and OP concentrations and ratios in mineral soil in a smaller subset of the large national Swedish forest soil inventory. It is a very comprehensive and spatially extensive data set, which is highly interesting by its inclusion of P as well as top- and subsoil layers of the mineral soil. I therefore recommend the paper for publication after attention to a number of issues mentioned below.

The mixed forest stands need to be better described. Based on a figure heading it appears it was only pine-spruce mixtures, so how was coniferous- deciduous mixtures handled? Are they merged with some other category?

The finding that subsoil stoichiometry was affected by tree species even in subsoil are interesting as we tend to believe and also often see that topsoil is mainly influenced by vegetation. As with all observational studies it is be good to discuss “hen and egg” questions like whether tree species affected subsoils or subsoils determined the natural regeneration or where specific species were selected for planting. This is also done in section 4.3. It is likely that texture differences are covarying with tree species, and therefore the subsoil C/N ratios are also lower in spruce than pine. Another possible problem with subsoils is that detection limits can give some bias. Was this an issue for e.g. N in 55-65 cm for which concentrations of particularly N but also C tends to get very low? It would be good to address this somewhere.

The effects of tree species and texture categories need to be more clearly communicated. None of the box plot figures enable separation of significant vs. non-significant mean values. The relative differences are mentioned but not whether such differences are significant. This needs to be improved.

The deciduous tree species group is a bit strange. There is a very high C/P ratio in min soil and highest OM stock in org layer? OM stocks in the organic layer are also extremely high in some cases. I cannot help speculate if a larger share of these sites are hydromorphic/peaty even though such sites were sought to be excluded?

Lastly, there are a number of typos and reverted comparisons (higher/lower) to correct, and the finish in figures could be improved (please find details below).

In conclusion, this is a solid manuscript based on a nicely planned study. The manuscript needs some attention to communicate results more clearly based on statistics, to clarify inconsistencies and consider some further methodological issues and aspects for the Discussion.

Specific comments

67 …adsorb particularly strongly….

96 wood coring better than tree drilling?

104 – what do the numbers in brackets stand for?

109 OK, I was initially surprised that the number of selected sites was so low, but based on these criteria it makes more sense. However, why was 60 years chosen rather than e.g. 30-40 years? Why only soils sampled 2013-2015? Could you have got substantially more sites and data from less rigid citeria, e.g. was the most limiting criterion the P analysis in parent material (which depth was this considered to be? It is definitely a nice dataset, but also quite small based on the huge forest area and high number of sampled plots.

123 Was it concentrated nitric acid? For the mineral soil, was this truly "total" P – this would require HF as well or?

148 Why was spruce and mixed stands combined? And were mixtures conifers+deciduous species or mixed spruce-pine? This is further in contrast to info in Fig. 3 where mixtures

are shown separately, but are said in heading l. 573 to be only pine-spruce mixtures.

144+ I miss information on the correlation analysis shown in Table 1 – is it simple correlations or linear regression as indicated by Fig. 1)? I also miss information on how the mineral soil layers were handled in analyses of C, N and P

157 Please indicate direction of correlations (pos/neg) in Table 1 as well.

157 Why not show the correlation of N stock with log transformed N in Fig. 2? All other graphs match with Table 1.

169 No need to repeat "molar" after M&M section.

172 – this should be the P stock increasing by a factor 2.3, right?

175 It sounds strange that N:P ratio was negatively correlated with N deposition (given pattern in Fig. 2f). I would expect a positive correlation, also as N dep and MAT is closely intercorrelated.

178 It is unclear to me which layers of the mineral soil were analysed in Table 1?

178 We need information on the direction (pos/neg) of these relationships for mineral soil even though they were weak.

181+ In this section please indicate that the results are based on an ANOVA (?) and give the model already in the M&M section 2.3.

182 I miss to see mineral soil C concentrations somewhere for the rtee species groups – can be added here or in the supplementary materials.

182 Please indicate significantly different species groups in Fig. 3. What does "inf" as the mean value indicate?

185 I think you mean pine forests and not spruce forests here?

187 Please provide information whether C:N ratios were potentially biased because of low N concentrations (or C concentrations) that would be lower than analytical detection limits.

190 What happened to C:P ratios in 10-20 and 55-65 cm layers?

191 Here is an example why we need more specific evidence of effects: what does "…did not substantially differ…" mean exactly?

193 This is surprising – is the stock of OM in the organic layer really highest in deciduous stands? If so, I would expect a very unbalanced design for species along the climatic gradient or some interaction with drainage regime? Based on the high and much higher mean values cp to other species groups for deciduous species (149 t/ha) I wonder if there is still a substantial amount of hydromorphic (wet) soils with peaty topsoil included in the deciduous group?

197-207. This section is about texture effects. Why not go through soil chemistry variables as for tree species for consistence? Why not show the same CNP variables in Fig. 3 and 4 for better comparison? Of course fine to deviate if there is a point and you want to show what is significant for species and textre, respectively. But the strategy it is not clear. Why show both C:N ratio and N concentration in Fig. 4? Is it because you should have shown N stocks (kg/ha) instead as the text seems to indicate?

198-201 These sentences can be condensed – or the last one can be deleted.

200 – you mean Fig. 4a-c?

202 Sentence is wrong – should be opposite. Highest ratios in coarse-textured soils.

205-207 This is not N stock in Fig. 4f, but N concentration…. What is right here?

210+ Nice to see the real data too for organic layers, and I think it would be fine to show just the non-linear version and the statistics based on logarithmic transformation. I believe, however, that the authors need to consider the organicdata here – it is said that

peat soils were excluded, but these very influential sites with high organic layer mass have to be peat – organic layers well above 200 t/ha are somehow hydromorphic and must be >30 cm deep?

226 In this section, I would also mention the strong covariance between N deposition and latitude.

235 An increase from R2=0.16 to 0.20 is perhaps not so substantial?

244 I would start here with the correlation with latitude and then discuss if it is MAT or deposition which would be the main driver for organic layer N stock.

253-256 I do not understand this argument. Stem growth rate was clearly (as expected) positively related to MAT. I think gradients are somehow mixed up here. Please revise/reword.

260+ But in southern Sweden, N deposition is generally larger than 4 kgN/ha as seen in Fig. 2c. But the low N dep/high MAT sites could see a contribution of N fixation to increase N stocks. Howeerv, I thin it would be valid to acknowledge that the spatial resolution of the Ndep map may not enable it to reflect local deposition conditions well enough. This could also be the reason for your weak correlation with N dep?

271 What was the suggested mechanism in Högberg et al. – was N dep also not good as explanatory parameter?

307 mineral soil

337 Adsorption and protection against mineralization is a good point, but I think you need to discuss that fine-textured soils also have a much higher weathering capacity to release P?

341 This effect of productivity on N stock in organic layers could also be in play to explain mineral soil N?

350 Another reason could be that higher organic layer stocks are found at more (K and P) poor parent materials where decomposition rates are slow?

Table 1 Please indicate direction of correlations (pos/neg).

Table 2 What is NTree?

Fig. 2 Heading l 565: N stock

Fig. 3 Please indicate significant differences. What does "Inf" in red mean? Move x axis heading ("Tree Dom" – please write out) to x axes of Figs 3e and 3f. Heading l. 573: It is important to communicate the info about mixed stands earlier – and how mixed deciduous –coniferous stands were handled?

Fig. 4 Again, we need indication of significant differences among texture classes. 4f: this is not N stock (l. 577) but N concentration….