Answers to reviewer#2

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

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? *We added the information that deciduous forest refers to birch, aspen, beech or oak forest. Pine or spruce forests with a few deciduous trees are classified as mixed forests. However, the group mixed forest in this database is dominated mixed pine-spruce forests.*

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. *We added one sentence in the Discussion (lines 330-332) to address the "hen and egg problem" in the following way. "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."*

The texture does hardly co-vary with tree species. This was (and is) clearly addressed in section 3.5, as follows:"Spruce forests tended to have a higher texture class, i.e., a finer soil texture than all other forests, while there was no substantial difference in texture between deciduous, mixed, and pine forest (Fig. S6c)." This is shown also in the Supplement in Fig. 6c (which was S5c in the reviewed version of the manuscript). The co-variance is (and was in the reviewed version of the manuscript) also discussed in section 4.3, as follows. "In addition, the relatively low C:N ratio in the mineral soil in spruce forests compared to pine forests might also partially result from the fact that the spruce forests tended to have a slightly finer texture (Supplement Fig. S6c), which is associated with lower C:N ratios".

We now added one sentence in the Results section to address the issue of the very low N contents in the following way. "It needs to be taken into account that there is some uncertainty regarding the samples with very low N and P concentrations since the determination of N and P contents in very nutrient poor samples is more strongly affected by sample inhomogeneity than the measurement of nutrient-rich samples."

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. *We compare the tree species and texture categories in boxplots, in which not only the medians but also the arithmetic means are displayed, both by symbols and numbers (Figs. 3 and 4). In addition, the relative difference between texture classes is displayed in Fig. 4. The reviewer seems to request some information about which of these changes are important, meaningful or "real" and which not. However, the p value calculated in an ANOVA or the attribute "statistical significant" cannot provide this information (Amrhein et al., 2019; Wasserstein et al. 2019). Many statisticians caution today against the use of p values since random variation in the sampling of the same population can lead to a large disparities in p values and since p values are very often misused (Amrhein et al., 2019). We follow this advice here.*

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? *Yes, the organic layer stock is largest in the deciduous forest. Yet, the C concentration in the mineral soil is in the same range as in the forests dominated by other tree species. We added a new figure to the Supplement to show this (Fig. S2c). As mentioned in the Material and Method section, we excluded peatlands. There is no stagnosol in this dataset and there are only three gleysols. However, none of them caries a deciduous forest (two are covered by pine forest and one by spruce forest). We now added this information to the Material and Method section. Based on this we see no reason to assume that hydromorphic features are the reason of the higher organic layer stock of the deciduous forests. The reason for the organic layer stocks of the deciduous forests might be related to the fact that the deciduous forests are concentrated at high MAT, where plant productivity is high.*

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). *We did so (see 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. *We thank the reviewer for positive evaluation of the manuscript and the detailed comments which helped us to improve the manuscript (see below).*

Specific comments

67 …adsorb particularly strongly…. *We added ly*

96 wood coring better than tree drilling? *We changed the wording.*

104 – what do the numbers in brackets stand for? *They refer to the code in the database. We now deleted them since they are not informative here.*

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. *Well, it can be debated whether 309 soil profiles is a small or a large number. Many papers in Biogeosciences are based on a much smaller number of soil profiles. We selected forests with a stand age >60 years in order to exclude a (recent) effect of clear-cutting and the associated soil preparation. Further, we only analyzed samples from the the current inventory (which started in 2013) for which the basic chemical data analysis had been completed (which at the time of manuscript preparation was 2018). The P concentration of the parent material was measured in a depth of 50 cm (which corresponds to the B or B/C horizon). We now added this depth to the method description.*

123 Was it concentrated nitric acid? For the mineral soil, was this truly "total" P – this would require HF as well or? *This procedure is widely accepted for the determination of total P, also in the mineral soil.*

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. *This is a misunderstanding. As indicated in this sentence, this referred only to 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.

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. *We added this information to the Material and Method section.*

157 Please indicate direction of correlations (pos/neg) in Table 1 as well. *We added signs in Table 1 to indicate positive and negative correlations.*

157 Why not show the correlation of N stock with log transformed N in Fig. 2? All other graphs match with Table 1. *We improved this aspect of the manuscript to separate more clearly the regression analysis (in Table 1 and 2) for which all soil variables have been log-transfomed now and the linear models shown in Fig. 1 (from which we now removed the results of the regression analysis).*

169 No need to repeat "molar" after M&M section. *Yes, in theory this is right, but since both mass and mol based ratios are used in this field of research it is better to keep this information in order to avoid confusion.*

172 – this should be the P stock increasing by a factor 2.3, right? *Yes. Thank you. We corrected this.*

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. *We corrected this.*

178 It is unclear to me which layers of the mineral soil were analysed in Table 1? *We added in the caption that the data refer to the mineral soil in a depth of 0-10 cm.*

178 We need information on the direction (pos/neg) of these relationships for mineral soil even though they were weak. *We now added + and – in Table 1 to indicate this. In addition, we clarified this throughout the Results part.*

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. *The reviewer seems to request some information about which of these changes are meaningful and which not. However, the p value calculated in an ANOVA or the attribute "statistical significant" cannot provide this information (Amrhein et al., 2019; Wasserstein et al. 2019). Many statisticians caution today against the use of p values since random variation in the sampling of the same population can lead to a large disparities in p values (Amrhein et al., 2019). We follow this advice here.*

182 I miss to see mineral soil C concentrations somewhere for the rtee species groups – can be added here or in the supplementary materials. *We added a new figure to the Supplement to show this (Fig. S2c).*

182 Please indicate significantly different species groups in Fig. 3. What does "inf" as the mean value indicate? *"inf" resulted from a mistake made when exporting the figure from R, which we now corrected by replacing inf by the correct numbers.*

185 I think you mean pine forests and not spruce forests here? *Yes, we corrected this.*

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. *The number are above the quantification limit. We added the following lines in the Results. "It needs to be taken into account that there is some uncertainty regarding the samples with very low N and P concentrations*

since the determination of N and P contents in very nutrient poor samples is more strongly affected by sample in homogeneities than the measurement of nutrient-rich samples."

190 What happened to C:P ratios in 10-20 and 55-65 cm layers? *We only measured P in the organic layer and organic P and total P in the mineral soil in 0-20 cm depth.*

191 Here is an example why we need more specific evidence of effects: what does "…did not substantially differ…" mean exactly? *The reviewer seems to request some information about which of these changes are important or "real" and which not. We understand this request. However, the p value calculated in an ANOVA or the attribute "statistical significant" cannot provide this information (Amrhein et al., 2019; Wasserstein et al. 2019). Many statisticians caution today against the use of p values since random variation in the sampling of the same population can lead to a large disparities in p values (Amrhein et al., 2019). We follow this advice here.*

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? *None of the deciduous plots has a hydromorphic soil. As mentioned in the Material and Method section, we excluded peatlands. There is no stagnosol in this dataset and there are only three gleysols. However, none of them caries a deciduous forest (two are covered by pine forest and one by spruce forest). We now added this information to the Material and Method section. Based on this we see no reason to assume that hydromorphic features are the reason of the higher organic layer stock of the deciduous forests.*

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? *In general, we showed the most interesting findings in the figures. Further, when exploring relationship between SOM and texture (that might potentially stabilize SOM through sorption) it makes more sense to analyze the concentrations rather than the stocks.*

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

200 – you mean Fig. 4a-c? *Yes, we correct this.*

202 Sentence is wrong – should be opposite. Highest ratios in coarse-textured soils. *We corrected this sentence.*

205-207 This is not N stock in Fig. 4f, but N concentration…. What is right here? *We corrected this by replacing the word stock by concentration.*

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? *The sites with very high mass in the organic layer is actually associated with more fertile humus forms (moder type) with a significant mineral content, which contributes to the high mass. Looking at the carbon stock there are only 5 sites with >100 ton C / ha. One single site had a humus depth >30 cm (34 cm), but still the humus form was mor (which is acceptable under non hydro-morphic conditions).*

226 In this section, I would also mention the strong covariance between N deposition and latitude. *We added the following sentences here "Latitude and MAT were strongly negatively correlated (R^2=0.88). Further, the log-transformed N deposition rate was strongly negatively correlated with latitude (R^2=0.78), and strongly positively with MAT (R^2=0.82)."*

235 An increase from R2=0.16 to 0.20 is perhaps not so substantial? *We deleted the word substantially.*

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. *Yes.*

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. *Yes, it should have been "increased" not "decreased". We corrected this now.*

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? *Yes. We added the following lines: "even if we take into account that the N deposition data is relatively uncertain and might not be fully representative of the accumulated differences in deposited N."*

271 What was the suggested mechanism in Högberg et al. – was N dep also not good as explanatory parameter? *The study also looks at a sequence at which N deposition and temperature are strongly correlated.*

307 mineral soil *Corrected*

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? *We added a new figure to the supplement (now Fig. S3, see below).*

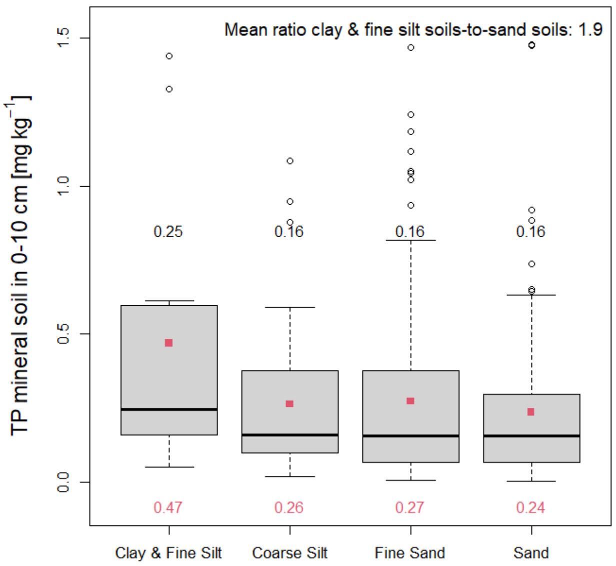

Figure S3: The total phosphorus concentration in the mineral soil in a depth of 0-10 cm depending on the soil texture (clay and fine silt n=11, coarse silt n=52, fine sand n=136, and sand n=110) in 309

Swedish forest soils with a stand age >60 years. The texture class called sand here encompasses sand and coarse sand. Black numbers give the median, red dots and red numbers depict the arithmetic mean.

We added the following lines to section 3.3 "In contrast to OP, the total P concentration did not differ between soils with the texture class sand, find sand and coarse sand. Only soils with the texture clay showed elevated P concentrations compared to soils of the other three texture classes (Supplement Fig. S3). TP concentrations were higher in soils with the texture clay or fine silt than in soils with the texture sand or coarse sand by a factor of 1.9 (Supplement Fig. S3)."
Further, we added the following lines to section 4.4. "The fact that the total P concentration (Fig. S3) differed much less than the OP concentration (Fig. 4c) between the soils of different texture class supports our interpretation that the strong enrichment of OP in the fine-textured soils is mostly caused by rigid adsorption of OP compounds (that protects OP against decomposition) and much less by a higher P concentration or P availability in these soils."

341 This effect of productivity on N stock in organic layers could also be in play to explain mineral soil N? *Yes. We added the following sentence. "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)."*

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? *At least for P, we know that this is not true. We added the following sentence in the Results "The organic layer stock was not correlated with the P concentration of the parent material and only very weekly with the P concentration of the uppermost 10 cm of the mineral soil (R^2=0.01, p<0.05)."*

Table 1 Please indicate direction of correlations (pos/neg). *We now added this.*

Table 2 What is NTree? *We explained this now better in the Material and Method section (see above).*

Fig. 2 Heading l 565: N stock *Corrected.*

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? *This was a mistake. We now replaced Inf by the correct number.*

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

References

Amrhein, V., Greenland, S., McShane, B. et al., (2019). Scientists rise up against statistical significance. *Nature 567*, 305-307.

Clarholm, M., & Skyllberg, U. (2013). Translocation of metals by trees and fungi regulates pH, soil organic matter turnover and nitrogen availability in acidic forest soils. *Soil Biology and Biochemistry*, *63*, 142-153.

Wasserstein, R. L., Schirm, A. L., & Lazar, N. A. (2019). Moving to a world beyond "p< 0.05". *The American Statistician*, *73*(sup1), 1-19.