Author's response - "*Drivers of soil organic carbon from temperate to alpine forests: a model-based analysis of the Swiss forest soil inventory with Yasso20*"

Editor's comments

Dear authors,

Both reviewers agree that your manuscript has been greatly improved. However, one of the reviewers still has some minor comments. If you address them, the manuscript will be accepted.

Best regards

Bertrand Guenet

Author's response

We thank the Editor and the Referees for their positive evaluation of the revised manuscript, and we appreciate the additional comments that you provided. We have addressed each of the remaining comments. The manuscript has been revised accordingly (see *Manuscript with track changes*).

You can find below our responses to the reviewer's comments, referring to the line number of the revised manuscript with track changes.

Anonymous referee #1: accepted subject to minor revisions

The authors have done a tremendous amount of work revising the manuscript. They have answered each of the points I raised in detail. I greatly appreciate the effort they have made. While I am satisfied with many of the responses, I still have some concerns.

The result in Fig. S2 (i.e. the 1:1 plot of simulated versus observed carbon stocks) is striking. Such an important result should be presented as a figure in the main text, rather than as a supplementary one (that most readers will not read), to truly demonstrate the model's performance. It appears that the predicted and measured data are not even correlated. This is fairly embarrassing for the model, suggesting that the comparable mean values achieved (LN22–23) may be coincidental.

Author's response

Thank you very much for the positive feedback on the revised manuscript, and for the further comments that you provided. Although the average simulated SOC stocks match well the average SOC stocks across Swiss forest soils (see Fig. 3), we are aware that the Yasso model and its application in this study does not capture the variability of SOC stocks at the site level (Fig. S2). This was expected, since the model does not consider: (1) mineral soil properties driving SOC stabilization (see lines 72-73, 79-83 etc. in the manuscript), with minerals (e.g. Fe and Al) being highly variable at the site scale, and (2) anaerobic conditions that may retard SOC decomposition and lead to locally high SOC stocks (see lines 386-390 in the manuscript). Additionally, uncertainties in litter inputs (i.e. satellite-derived NPP) at the soil profile scale also potentially contributed to the observed discrepancies between simulated and measured SOC stocks at the site level (lines 430-431).

Here, we would prefer to keep the 1:1 plot of simulated vs observed C stocks in the supplemental material (Fig. S2) given that the main goal of the manuscript is to identify the main factors controlling SOC stocks in Swiss forests - and thus which processes should be included in soil C models - rather than to evaluate and demonstrate the model performance at the site level. We have added reference to Fig. S2 at lines 431 and 449 in the main text.

Reviewer's comment

My suggestions are as follows:

The conclusion (LN489–491) should be toned down considerably by emphasising that the model fails to capture variations at the site level. This is because the simulated and observed data are not only far from the 1:1 line, but are also poorly (or not at all) correlated.

Author's response

Thank you for your remark. We have now added the following sentence at line 481 in the conclusion: "*although the model does not capture the variability in SOC stocks at the site level*".

Reviewer's comment

I am also wondering whether a positive correlation between simulated and observed C stocks could be found at the regional level. While I suggest making Fig. S2 as the new Fig. 4, could it be possible to create a 1:1 scatter plot for each region as a supplementary figure to complement Fig. 3a too?

Author's response

Thank you for your suggestion. The correlation between simulated and observed SOC stocks is weak but significant across all sites (where r is the Person correlation coefficient, $r = +0.08$, p-value = 0.034, $n = 468$ sites). Similarly, at the regional level, only weak or no correlation between simulated and measured SOC stocks was observed with the exception of the Southern Alps, likely due to the larger SOC stocks gradient found in this region ($r = + 0.44$, p-value = 0.006). We have not added this plot to the main manuscript or supplemental material, given that it is not adding more information compared to the Fig. S2 already presented. Moreover, we want to stress again that the main goal of the manuscript is to identify the main factors controlling SOC stocks and thus which processes should be accounted in soil C models rather than to evaluate the model performance at the site level.

Reviewer's comment

LN195: Ok, but if variables are only centred, but not standardised (with z-score normalisation), they may not be homoscedastic and therefore may violate the rule for certain analyses (such as Pearson's correlation, see Fig. 4).

Author's response

Thank you for the remark. We note that while centering and standardization serve specific purposes - such as reducing multicollinearity or making coefficients comparable - they do not directly ensure homoscedasticity. This assumption was instead verified visually through residual and scatter plots (line 192-193).

The centering of explanatory variables was performed only in linear mixed-effect models, not in Pearson's correlation analysis. Scaling the variables to a certain standard deviation (as done with standardization), would mean that the estimated coefficient changes by the corresponding factor. We choose to not scale the variables to a certain standard deviation since we wanted to provide in Table 2 and 3 (column Est.) meaningful model estimates, which can be interpreted in the same measurement units of the predictor and response variables and can be more easily taken into account in further studies.

Reviewer's comment

LN280 and LN192-4: Why do you sometimes transform the x-label data but not at other times? How did you decide on the transformation method?

Author's response

Thank you for your comment. Transformations such as log or square root were applied when the relationship between the predictor and response was non-linear or when residuals showed signs of heteroscedasticity or non-normality. The decision to transform variables was guided by visual inspection of scatterplots and residual plots (line 192-193).

To clarify how we decided on the transformation method, we have rephrased the text at line 189-192: "*The numerical explanatory variables were log- or square-root transformed when the relationship between the explanatory and the response variable was non-linear or when the residuals showed signs of heteroscedasticity or non-normality*."

In Fig. 4 and Fig. S5, we show only for visualization purposes the exchangeable Fe and Al on a square-root scale x-axis, and clay on a natural-logarithm scale. Transformations applied in the linear mixed-effect models are reported in the Tables where results of statistical analysis are shown (see Table 2, Table 3, Table S2-S6).

Anonymous referee #2: accepted as is

Authors sufficiently addressed my main concern's about the uncertainty of derived litter input from NPP in expanded discussion and the mismatch between the trends in NPP (as proxy of the litter) and simulated SOC by adding Fig.S1. I have no further suggestions for the revision

Author's response

Thank you very much for the positive feedback on the revised manuscript!