

SOIL Discuss., referee comment RC1
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Comment on soil-2021-81

Anonymous Referee #1

Referee comment on "Content of soil organic carbon and labile fractions depend on local combinations of mineral-phase characteristics" by Malte Ortner et al., SOIL Discuss., <https://doi.org/10.5194/soil-2021-81-RC1>, 2021

In their manuscript, 'Soil organic matter and labile fractions depend on specific local parameter combinations', Ortner et al. present their work on the analysis of factors controlling the soil organic carbon (SOC) concentration in topsoils of the region around Trier, Germany. The authors collected topsoil samples in arable land and grassland in 4 regions with different parent material, and determined the organic carbon (OC) concentration, hot water extractable carbon (HWEC), microbial biomass carbon (MBC) and multiple soil properties on these samples. They used PCA to cluster the soil samples based on parent material and soil texture into different clusters. The aim of their study was to assess the main factors controlling topsoil organic carbon concentration, HWEC and MBC using two modelling approaches: a bivariate model and mixed effects models. The main findings are that (i) mixed effect models outperformed bivariate (linear) models in predicting OC%, HWEC and MBC, (ii) at the local scale, site-specific parameters explained OC variability better than landscape-related variables and (iii) using the 'local' model resulted in better results when predicting the OC% of a specific cluster compared to the 'global' model.

The results of the present study help to improve our understanding of the factors controlling topsoil organic carbon concentrations at the landscape scale, which is needed e.g. in order to improve soil organic carbon models. The authors have constructed a valuable dataset which may benefit other researchers. I would therefore encourage the authors to make this data available through an online repository, instead of making it only available upon request.

Overall, the manuscript is well-written. However, at multiple locations very long sentences are used, which does not benefit a smooth reading. Splitting those sentences and using more commas would improve the readability of the manuscript considerably. In addition, I would encourage the authors to use subsections in the Results and Discussion sections, which will provide a better overview to the reader of what is being presented and discussed.

One of my main concerns about the present manuscript is related to the quantification of the goodness-of-fit of the different models, which is now done using R-square. This is a measure to quantify the proportion of variation in a dependent variable that is explained by an independent variable, but is not a measure for the goodness-of-fit of a model. For example, a very poor model can have a high R-square value, while a good model can have a relatively low R-square value. Therefore, the authors should use a different measure to quantify the goodness-of-fit of their model when comparing measured with modelled data, such as the (root) mean square error or similar.

In addition, I missed a discussion about the broader implications of the results and the implications for future research. For example, do the authors suggest that researchers should use 'local' model whenever possible? And how about regions where local information is not present? It would also be very informative if the authors would quantify the difference in predicted SOC% when using a global versus local model. To how much of an over or underestimation would this lead? Is that difference significant enough to invest more resources in the collection of local data?

Another concern is related to the title, which I find not very informative. For example, it will not be clear to someone who has not read the manuscript what 'specific local parameter combinations' are. Also, it would be good to be more specific about what they mean with 'soil organic matter and labile fractions'. From the title it is not clear if the authors mean SOC concentrations, stock, spatial distribution etc. In addition, the manuscript discusses soil organic carbon, and not all soil organic matter, so would be good to change this in the title.

Lastly, it would good if the authors specify in the beginning of the manuscript that they discuss SOC concentrations, and not stocks. Throughout the manuscript, the authors talk about 'SOC' without specifying that it concerns concentrations, not stocks. This is an important difference, which should be clear to the reader from the abstract onwards, and repeated throughout the manuscript. For example, the authors could change 'SOC' to 'SOC%' to make this clear.

Specific comments

L18: Would be good to explain here what you mean with 'global' and 'local' clusters (and models).

L19: define that you assess SOC concentrations, and thus not stocks

L20: would be good to explain here which 'labile fractions' you study

L21-22: would be good to make it clear that bivariate models were used to determine these correlations

L24-25: you cannot use R-square values to compare modelled versus measured data (see above)

L27: here you use the term 'organic matter', while until here you used 'SOC'. Please be consistent with these terms, and only use one

L29-30: 'showing that labile fractions depend less on soil properties than on organic matter input and turnover in soil'. The latter were not studied, so you cannot say this with certainty. Would be better to end the abstract with a statement about the broader implications of your results.

L41: another important labile fraction of SOC is particulate organic carbon. Would be good to justify why you did not study this fraction

L45: 'MBC is expedient to explain SOC dynamics': this is rather vague, please be more specific

L45-46: 'much less research and attempts for quantitative modelling of these labile fractions [...]': recently, multiple mechanistic models have been used to simulate labile carbon fraction such as MBC and POC, e.g. Ahrens et al. (2015), Wieder et al. (2015) and Zhang et al. (2021)

L58: 'In addition or even instead of': choose one

L63: please clarify what you mean with parameters versus factors, as you use these terms throughout the manuscript

L87: please define what you mean with 'global models'

L99: the term 'sufficient quantification' is rather vague, please clarify this

L107: 'similar numbers of samples': how many per region?

L108-109: the use of the abbreviations throughout the manuscript is not intuitive and confusing for the reader, please use different names to identify the different regions, e.g. the parent material

L119: why were some samples stored at -20 °C and others air-dried?

L134: was the chloroform fumigation extraction performed on samples freshly collected from the field?

L137-140: for how long were the samples incubated? How often was the CO₂ measured?

L143: were all parameters log-transformed? Please clarify this

L146: please provide some examples of the 'mineral phase parameters'

L146-147: please provide more information about the linear regressions that were performed

L156-157: Please provide information about which parameters were removed from the models

L161: were all parameters checked for collinearity? Please clarify

L163: why a square root transformation? Please justify this

L163: Please clarify how the performance of the models was examined

L170: Please clarify the difference between 'soil' and 'topsoil' properties

L177: are those differences statistically significant? What are the averages for the different parent materials?

L186: please provide examples for the 'parameters describing the composition of SOM'

L191-194: this is not clear

L205-206: which 10 parameters?

L213: 'that largely matched with those found for the complete dataset': this is not clear

L224: what do you mean with 'sufficient extent'? Similar wording is used throughout the manuscript, but this is very subjective and should be clarified.

L240: please clarify what 'equal weight of samples' means

L237-242: please make clear that you are discussing the results of the bivariate models

L241: what are the 'complex interactions of several different parameters'?

L243: please clarify what you mean with 'insufficient'. Which measure do you use to determine if a model performance is sufficient or not?

L249-251: R-square values are no measure for model performance, please provide the root mean square error (or a similar measure). Please show these results in a graph, perhaps in the supplement?

L273: do you mean the bivariate models with 'linear regressions' Please be consistent with this terminology

L284: please replace R-square with a measure of model performance

L287: please provide the goodness-of-fit values before concluding that a certain model has an 'inferior performance'

L309-310: by saying 'Al- and Fe-oxides were shown to have a relevant influence on sequestration and stabilization of SOC', it seems like you explicitly studied this, while you only used a statistical model to assess this. Also, since you model SOC concentrations and not stocks, you cannot say anything about C sequestration, as this also depends on bulk density.

L342: please explain what you mean by 'multidimensional'

L348: 'to explain SOC': please clarify which aspect of SOC

L366: how do you conclude that sample size biased the results? Did you test for this?

L369: 'satisfying extent': how is it quantified that a model performs satisfying? Please be objective in deciding if a model is good or not

L370: what do you mean with 'partially practicable'?

L374-375: 'sufficient quality level': same remark as L369

L380-381: by saying 'It became clear that [...] with different annual dynamics', it seems like you tested annual dynamics. Please rephrase

L281-382: You did not take SOC turnover into account, so how can you say that this is a reason for the lower explained variability by the different models?

L403-404: 'sufficient estimation': same remark as L369

L405: would be good to end the Conclusions section with a statement about the broader implications of your results

Figures and tables

Fig. 2: the colours in B are difficult to distinguish

Table 3: please make clear in the caption that these are the result for the bivariate regressions

Fig. 3: 'Predicted vs. measured': please clarify in the caption which model was used to make these predictions. Please provide a measure for the goodness of fit and remove the R-square values, as this is not measure for model performance

Table 5: please provide more information about the table in the caption, the table should be clear to the reader without having read the entire manuscript. It would be more informative to provide a table with e.g. root mean square errors instead of R-square

Figure 6: it would be informative to show the same graphs for other clusters in the supplement. Please remove the R-square values and replace them by a measure for the goodness-of-fit of the models

Technical comments

L36: driver => drivers

L57: expedient => suitable

L73: space between 'asCa2+'

L119: it's not clear what 'respectively' refers to

L170: it's not clear what 'respectively' refers to

L172: it's not clear what 'thereby' and 'essentially' refer to

L252: it's not clear what 'respectively' refers to

L275: what do you mean with 'not last'?

L304: 'the in total very sandy soils': please rephrase

L320: what is 'circumneutral'?

L324: please remove 'respectively'

L343: please remove 'respectively'

L344: confirmed => is in line with

References

These references were chosen based on their scientific content. I leave it up to the authors to decide if they wish to include them in their manuscript.

Ahrens, B., Braakhekke, M.C., Guggenberger, G., Schrumpf, M., Reichstein, M., 2015.

Contribution of sorption, DOC transport and microbial interactions to the ¹⁴C age of a soil organic carbon profile: Insights from a calibrated process model. *Soil Biology and Biochemistry* 88, 390–402. <https://doi.org/10.1016/j.soilbio.2015.06.008>

Wieder, W.R., Grandy, A.S., Kallenbach, C.M., Taylor, P.G., Bonan, G.B., 2015. Representing life in the Earth system with soil microbial functional traits in the MIMICS model. *Geoscientific Model Development* 8, 1789–1808. <https://doi.org/10.5194/gmd-8-1789-2015>

Zhang, Y., Lavallee, J.M., Robertson, A.D., Even, R., Ogle, S.M., Paustian, K., Cotrufo, M.F., 2021. Simulating measurable ecosystem carbon and nitrogen dynamics with the mechanistically defined MEMS 2.0 model. *Biogeosciences* 18, 3147–3171. <https://doi.org/10.5194/bg-18-3147-2021>