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Interactive comment on "Is the content and potential preservation of soil organic carbon reflected by cation exchange capacity? A case study in Swiss forest soils" *by* Emily F. Solly et al.

Anonymous Referee #1

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The current study presents a thoughtful consideration of factors affecting SOM abundance across a dataset of considerable size and quality. The examination of variance in these relationships with depth is especially interesting, and stands to lend useful and insightful information for soil C cycle modeling efforts. This dataset is extremely valuable, and I believe the authors will be able to extract some very meaningful conclusions from this work. The manuscript is well written. The introduction could benefit from a reread by the authors and some slight revision for clarity, but the main concepts being discussed are timely and well-articulated for the most part.

The language surrounding the concept of proxy variables is used inconsistently

throughout the manuscript. In the introduction, the authors hypothesize that CECeff can be used as an integrative proxy, representing the sorptive capacity associated with reactive soil surfaces (including organic surfaces, which presents some logic problems). In other places, the authors suggest that CECeff could be used as an integrative proxy of SOC content and its potential preservation. If CECeff is an integrative proxy of stabilization mechanisms, then it would be a predictor of SOC content. If CECeff is an integrative proxy of SOC content and stability, then it would potentially be used instead of SOC in models. By definition, a proxy is, "…a measurement of one physical quantity that is used in the place of a different quantity that would be too difficult or expensive to measure directly" (Bailey et al., 2017).

I believe the biggest issue the authors must effectively address during revision is the choice of CECeff as their explanatory variable of choice. SOM often accounts for a very large portion of the overall CECeff of a particular soil sample. Therefore, CECeff is dependent on SOM content, not the other way around as the model in the paper suggests (using CECeff as an explanatory variable in a model for SOC). That isn't to say that CECeff couldn't be couldn't be used as a proxy for SOC content, but it would seem more effective to just measure SOC content since CECeff only exhibits a moderate correlation with SOC and is just as laborious to measure. The dependence of CEC on SOM content would explain why correlations among CECeff and SOC are stronger in surface soils where SOC is more abundant, as is stated in the discussion. To some extent, the same argument could be made against the findings of Rasmussen et al., 2018, since exchangeable Ca comes from organic exchange sites not associated with mineral surfaces as well as from organo-mineral cation bridging. I believe the use of exchangeable Ca is somewhat more defensible since its role in SOM stabilization is understood on a mechanistic level. It forms cation bridges between organic and inorganic surfaces through ligand exchange. Monovalent cations do not form cation bridges, and therefore cannot contribute to the stabilization of SOM. Exchangeable Mg does not lend itself to stable cation bridges due to a smaller ionic radius. The authors will have to justify from a mechanistic perspective, how CECeff functions to promote

SOM accumulation and/or stability.

The introductory material suggests that CECeff might act as an effective integrative proxy for properties such as surface area, short-range-order mineral content, clay content and soil organic matter. Here again, is a circular argument. The authors are stating that variance in reactive mineral surface area and SOM exchange sites can be predicted by changes in CEC. They then claim that changes in SOC can be predicted by changes in CEC. We all know that SOM and SOC are inherently linked, and CEC is highly dependent on SOM, so why bother with the proxy? Just measure SOM, which will basically give you a SOC value. Also, explanatory variables included in soil C models must have predictive capacity in order to be useful. We need explanatory variables that will be able to predict how SOC stocks will change in abundance or stability. Because CEC is so heavily influenced by SOM concentration, it changes as a result of changes in SOM concentration, not the other way around. Yes, they are correlated to some degree, but I believe CEC is the dependent variable and SOM (and therefore also SOC) is the explanatory variable.

Also, if the desire is to prove that CEC can be used as an integrative proxy for stabilization mechanisms, then the wrong model has been constructed. In order to prove that CEC is accounting for variation in oxalate-extractable metals, clay content, and surface area, a model would have to be constructed with CEC as the dependent variable, and oxalate-extractable metals, clay, surface area, etc. as the explanatory variables. It seems like the first hypothesis of this paper should be, "CECeff serves as an effective integrative proxy for variables such as metals, clays, and surface area". The authors would then prove the correctness of that hypothesis by using a statistical model to link variation in CEC with variation in metals, clays, and surface area. Then the argument would follow that CEC is much easier to measure than these other properties, as stated in the introduction, and the second hypothesis would then follow, "Because CEC is an effective integrative proxy of SOM stabilization mechanisms, CEC can be used to predict changes in the stability and abundance of SOC". Then a model similar to the one

currently presented would be appropriate.

I believe the modeling work in the manuscript could be improved by a slightly different statistical approach. It doesn't seem appropriate to test for the significance of the explanatory variables for the 0-120 cm models without taking depth into account. Depth is a confounding variable due to the fact that most soil physicochemical characteristics vary predictably with depth. The chosen approach then was to split surface soils from subsurface soils (0-30 cm and 30-120 cm) to examine how the relative influence of difference explanatory variables varied with depth. I believe a more appropriate approach would be to apply a linear mixed model using all the explanatory variables as fixed effects. Depth and all its interaction terms would also be included as fixed effects, with SOC as the dependent variable. The resulting model would indicate which of the climatic or physicochemical variables varied in their influence with depth (which fixed effects were significant). The two-way interaction terms that are significant should be fairly easy to interpret given how the data has been transformed. I'm also confused about why pH and its possible interaction terms were not tested for significance. One of the main findings of the paper is that the relative importance of explanatory variables depend on pH. Perhaps pH could be a more useful proxy than one or more of the other variables currently used in the model? Was pH ever included in a model? Are there other soil physicochemical properties that the authors could explore in lieu of CECeff?

I would also ask the authors to explain their choice of environmental parameters. Why use LAI instead of NPP? Many soil scientists would argue that NPP would be a better predictor of OM inputs to the soil. Why use MAT and MAP instead of PET or a soil moisture regime index? The authors indicate that differences in moisture are important regulators of the downward propagation of C in these soils, because of differences in leaching depth. PET and/or a soil moisture index would do a better job of representing leaching potential because the seasonality and form of precipitation matters, not just the total amount of precipitation.

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