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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 #3

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Overall, this is an important analysis of proxies that can predict SOC content. This manuscript fits well with recent syntheses and reviews like Rasmussen et al. 2018 and Rowley et al. 2018 (both in Biogeochemistry). The authors tested the ability of CEC, clay, LAI, MAT, and MAP to predict the weighted average SOC content in the surface, subsurface, and whole soil profile in >1000 forest soil profiles across Switzerland. They found that effective CEC was the best predictor of SOC content at higher pH's in the whole soil profile and surface 30 cm of the soil profile while MAP was a stronger predictor at lower pH in the whole profile and surface soil. For the subsoil, both climate variables (MAT and MAP) were the strongest predictors likely due to greater weathering and leaching of organic molecules through the soil profile.

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Discussion paper

The statistics are sound and clearly presented. The figures are clear, though figures 2 and 3 are a bit redundant in that they show the same trends. I think Figure 3 is clearer and suggest only using that figure, but given this is an online open access journal, I see no harm in including both if the authors feel strongly about including both.

My largest comment is that I wonder how single cations would be as predictors, such as Ca and Fe or Al as in Rasmussen et al. 2018 as it would be interesting to test those findings with a different dataset. For example, I wonder how much the strong CEC relationship at higher pH is driven by the Ca cations alone and whether Al ions would better explain the MAP relationship at low pH. To test individual cations would likely illuminate mechanisms better and they may even have stronger relationships with SOC than CEC, but individual cations would then not be the integrative proxy that the authors are seeking.

Lastly, I am not sure if it belongs in the Introduction or Discussion, but Rowley's 2018 synthesis, "Ca-mediated stabilization of organic carbon" should be cited in this paper as it also touches upon pH differences in the controls on SOC content. Figure 3 is particularly relevant.

I have some minor comments where the manuscript needs some clarification and where the findings of Rasmussen et al. could be more accurately presented. Specific comments:

Abstract L23: delete "as compared to the mere quantification of clay-sized particles" because as you stated in the intro, that is not a trivial analysis to do. Introduction: L79: Please clarify "exchangeable Ca and Fe, and Al oxyhydroxides". Rasmussen et al. tested the predictive capabilities of oxalate extractable Fe and oxalate extractable Al. These are measures of organo-metal complexes and short range order minerals, not exchangeable Fe or all Al oxyhydroxides. L83: Add that soil pH also determines the relative charges of organic molecules and soil minerals and thus the likelihood that organic molecules will sorb to minerals, so not just organo-metal complexes with ligands. L84: The line about depth should be a separate sentence as it is unclear here what you want to emphasize about soil depth. L114-116: I disagree that Fe and Al variables cannot be measured on large datasets. Doesn't the analysis in Rasmussen et al. of a large soil dataset, which you cite for this sentence, contradict that statement?

Methods: L147: please define "fine earth" does this mean, you corrected for rocks? How did you classify "fine"? L185: What time period during the growing season was used to determine LAI? L218: The line "for each statistical test $P<0.05$ was . . ." directly contradicts the previous statement. Maybe write, "For all other statistical tests . . ."

Results: When reading the results, my first question was what the distribution of samples among the different pH classes were. You might want to move up Figure 4 to the beginning as it strengthens the interpretation of your analyses to know how evenly distributed the samples were among pH classes.

Discussion: Line 299: Here is a good place to cite Rowley et al. 2018. L314-317: This statement here leads to a bit of a 'chicken and egg' conundrum. Is there more Ca because negative charges on OM can bind to it or is there more SOM because there is more Ca to bind to it? Maybe it doesn't matter for predictions. I have the same issue for when pyrophosphate extractable Fe and Al are used to predict SOC as that extraction targets organo-mineral complexes. Maybe the Ca stabilization mechanisms brought up by Rowley could help here. L317: Tone down this statement to "may be instead" in place of "is instead" as to know for sure you would need a mechanistic test as you nicely point out below. L367-360: Please reword this sentence. I found this sentence to be confusing in its structure, particularly the clause in dashes, and had to reread it several times.

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