Interactive comment on "Continental-scale controls on soil organic carbon across sub-Saharan Africa" by Sophie F. von Fromm et al.

Note: The line numbers in our answers are referring to the updated manuscript.

Anonymous Referee #2

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General comments:

REVIEWER_02: The manuscript "Continental-scale controls on soil organic carbon across sub-Saharan Africa" describes a continental-scale analysis of associations between soil organic carbon and soil physico-chemical properties across Africa. The manuscript outlines a novel soil dataset collected at the Afsis "sentinel sites", and then steps through several statistical analyses that tease apart associations between carbon, extractable metals, and soil exchange pools across different domains of climate, soil pH, and soil weathering status. The authors conclude that short-range order (oxalate extractable Al) and to an extent Fe explain much of the variation in carbon stocks in wet/acid soils, whereas exchangeable calcium explains much of the variation in dry/alkaline soils. soil texture and land use appear largely irrelevant at this scale.

I think this manuscript is excellent and will be a very useful contribution to the study of soil geography. While the primary result has been identified in earlier studies (particularly Rasmussen et al.'s 2018 study), this manuscript applies to a different geographic domain (tropical and subtropical Africa) and with a more systematic data collection effort. It also considers soil weathering status using total elemental inventories and chemical weathering indices, which adds novelty. The results provide clear confirmation of the patterns hinted at in the Rasmussen study, and also point to some new complexities (particularly in relation to Fe). Furthermore, this study applies to data that were collected in a systematic sampling effort–hence these results should be considered more conclusive than those in earlier studies. The manuscript does a good job of balancing different statistical approaches, and stands as an example of how data-driven modelling tools (i.e. random forests) can be used responsibly in a processoriented way to compliment more traditional statistical approaches. While at points the interpretation slides into a more descriptive "data-mining" posture, it is also punctuated with insightful process-based insights. In short: overall this is a strong manuscript!

ANSWER: We highly appreciate this very thoughtful and appreciative review. Thank you for taking the time to carefully comment on our manuscript. We will address the suggestions in detail in the following response.

R_02: My main criticisms apply to the way the methods are presente–I think some details are left out or insufficiently documented. I also think that the methods and discussion sections could use more of a "road map" at the start–particularly the discussion, which dives into a description of the correlations between different variables where it could start with some pithy statements summarizing the high level process-based interpretation.

A: We really appreciate these comments. We will address them in detail under the corresponding *Specific comments*.

R_02: I also would appreciate a bit more discussion of the underlying geographic patterns in the context of African geology (perhaps just a paragraph). I realize that the existing geospatial products don't allow for a thorough quantitative analysis of geologic state factors, but some limited qualitative might be good. More specifically the authors might address how parent material, soil age, and erosion rates vary (or do not vary) across the sampling locations, and how these might exert some influence on the results independent of climate.

A: Thank you for this comment. We agree that there seems to be no appropriate geospatial product for lithology that allows for a thorough quantitative analysis. We added a paragraph called *Geographic patterns* in the discussion section where we did a qualitative interpretation of some geographical patterns, such as lithology and soil age. In terms of erosion we added some details in the paragraph *Land cover* in the discussion section (those details are also based on a comment and some references from the Reviewer#1).

Line 465: "

Geographic patterns

All soils result globally from the same soil-forming factors (climate, organisms, topography, parent material and time) and are formed by similar processes (e.g. oxidation, reduction, leaching, transport and accumulation). This might explain to some extent, why similar soil and climate parameters are important to explain SOC content variation in sub-Saharan Africa as compared to other regions. However, significant differences are still visible in subtropical and tropical soils, especially in terms of mineralogy, weathering and soil formation, which are related to important differences in climate, soil age, parent material and vegetation (Buringh 1970). Such differences do occur between soils from sub-Saharan Africa, which do not only differ greatly in their soil properties and climate (Table 1), but also in vegetation, parent material, soil age and their vulnerability to degradation (Jones et al. 2013). However, due to the lack of precise geospatial products for those parameters for sub-Saharan Africa, we can only discuss them here qualitatively.

The AfSIS sites (Figure 1) are mainly derived from two parent material types: i) metamorphic rocks and ii) volcanic rocks (Hartmann and Moosdorf 2012; Jones et al. 2013; Schlüter 2008). Metamorphic rocks are most commonly found in West Africa, Southern Africa and Madagascar. These regions are characterized by old cratons, except for Madagascar, which is influenced by Mesozoic volcanism (Schlüter 2008). Most of these soils are classified as Ferralsols according to the WRB soil classification system (Jones et al. 2013). This partly explains, why the AfSIS soils from those regions are usually highly weathered with low pH_{H2O} values. In contrast, soils derived from volcanic rocks are mainly found in East in the Great Rift Valley. These soils are usually younger and less weathered (Buringh 1970), which is also mirrored in soil properties within the AfSIS data set. These soils are characterized by lower CIA values and higher Al_{ox} and Fe_{ox} concentrations compared to other soils in the AfSIS data set. Outside of the influence of volcanic rocks, Ca^{2+} rich soils are frequent in East Africa and are dominated by a high concentration of Ca_{ex} and high pH_{H2O} values. Since Al_{ox}, Fe_{ox}, and Ca_{ex} were important predictors of SOC in our analyses, the SOC content is usually also higher at AfSIS sites in East Africa compared to sites in West Africa and Southern Africa.

Although certain soil properties, in combination with climate variables, are important to explain SOC concentration variation, different soil forming factors, such as parent material and soil age, are important to understand, which soil properties will dominate – at least at this large-scale approach. To link those two aspects more quantitatively on continental-scales might be a direction for future studies."

Line 451: "This might be due to the high variation of SOC content within the different land cover classes at this large spatial scale ("Figure 1a). For example, the class cropland contains a wide variety of cultivated plots – we did not have more detailed information about land management practices. Fujisaki et al. (2018b) showed that SOC stock changes in tropical cropland soils are mainly driven by C inputs.

On the other hand, it is also known that land use changes and their impact on soil physico-chemical properties are scale-dependent and are likely to be more distinct at smaller scales (Holmes et al. 2005; Holmes et al. 2004). For example, land management and land degradation (i.e. erosion) are known to impact SOC stocks on regional scales in sub-Saharan Africa (Winowiecki et al. 2016a). However, we lacked the detailed data necessary to disentangle the impacts of different practices. Additionally, since the focus of our work was on natural soil physico-chemical and climate properties, we did not further investigate those anthropogenic factors at this large spatial scale. Future studies are needed to better understand the impacts of land management and carbon storage potential in soils across sub-Saharan Africa at different scales (Fujisaki et al. 2018a; Vanlauwe et al. 2015). Overall, our data for sub-Saharan Africa suggests that SOC content on a continental scale is better explained by stabilization potential in soils (climate, geochemistry) than by different aboveground C inputs (vegetation)."

Specific comments:

R_02: Lines 39-40: The phrase "complex analytical approaches with a large number of parameters" is somewhat opaque. Perhaps substitute something more specific?

A: Line 40: "Assessing the state of soils and their potential response to climate and land-use change requires carefully designed sampling strategies, combined with systematic analytical and statistical analyses across locations and scale (IPCC 2019)."

R_02: Lines 62-63: To be fair here: there is an implicit representation of competition between microbes and minerals in Earth System models via clay content. There are two issues in this case: (1) competition between minerals and microbes is not represented in an explicit, mechanistic way; and (2) clay content doesn't capture the relevant aspects of soil mineralogy or chemistry. I think this manuscript addresses the latter issue more than the former.

A: A similar comment was brought up by the Reviewer #1. We agree that the paragraph about model approaches was not that accurate and have now updated it. Since this is not the main focus of our manuscript we would like to keep this paragraph rather short. However, we think it is an important aspect worth mentioning in the introduction. We have revised the paragraph as follows:

Line 61: "SOC stabilization is commonly conceptualized as competition between accessibility for microorganisms versus chemical associations with minerals (Oades 1988; Schmidt et al. 2011). These processes are often only considered implicitly by models (Blankinship et al. 2018; Schmidt et al. 2011). Instead, models commonly rely on broader variables such as clay content, which is used as a proxy for sorption and other organo-mineral interactions (Rasmussen et al. 2018; Schmidt et al. 2011). These more generic variables integrate a variety of stabilization processes which can be difficult to disentangle. They might even differ in their relative importance and may not adequately capture soil mineralogy and chemistry across different ecosystems and climate zones. Hence, improving the predictive capacity of such models requires not only a better understanding of the factors that control SOC dynamics, but also verification (or falsification) of those new findings in regions that are underrepresented in field studies and models."

R_02: Lines 129-131: Was this digestion quantitative? I believe some silicates are resistant to aqua regia. Perhaps clarify whether these should be considered total elemental pools or simply aqua-regia-digestible pools, as this may influence the interpretation of the CIA (though probably not much I imagine).

A: Line 142: "Aqua regia acid digestion was applied for major and trace elements, including Al, Ca, K and Na. Although this method does not give absolute total contents, it does give results sufficiently close to accepted values for different soils (McGrath and Cunliffe 1985)."

R_02: Line 160: It would be good to include a short overview paragraph at the start of the statistical analysis section explaining the overall strategy. It seems that several approaches were applied to the same data: linear mixed effects models, regression trees, and random forests. I can see how the approaches complement each other (the mixed effects models seem more conservative and permit statistical hypothesis testing while accounting for non-independence of the data, but the CART based approaches can handle non-linearity). This is explained later, but the readers will benefit from a quick signpost at the start. Similarly, the discussion section is hard to follow at the start. I strongly recommend adding a concise paragraph at the beginning of the discussion that identifies the major results. As it stands now the discussion dives right into the details and I can only identify an emergent narrative at the end.

A: Statistical analyses section: Line 179: "We used several statistical approaches to analyze our data set, including linear mixed effects models, regression trees and random forests. We compared the results of these three different methods to confirm key findings and derive complementary insights. In brief, we used linear mixed effects model to handle the clustered and dependent sampling design of the AfSIS data set, whereas regression trees and random forests enabled us to account for non-linearities within the data. More precisely, we used regression trees as a qualitative tool to explore and understand the structure of the data, whereas random forests offered more generalizable models. All statistical analyses were performed within the R computing environment (Version 4.0.0, R Core Team 2020). The R Markdown file in the SI provides the code to reproduce all our analyses."

A: Discussion section: Line 349 (We moved the last paragraph to the beginning of this section and summarized our main findings at the beginning of the discussion): "Here, we focus on those variables that showed the most explanatory power in terms of SOC content across all models. We then compare their explanatory power with those reported in other studies for different regions. Short-range order minerals (Al_{ox}) and to some extent Fe_{ox} explained much of the variation in SOC concentration in wet regions with acidic and highly weathered soils. In contrast, Ca_{ex} explained much of the variation in dry regions, dominated by alkaline and less weathered soils. In addition, we discuss the role of clay and fine silt content, and of land cover, since they were important in other studies. However, in our study, the latter did not explain much of the variation in SOC content, which may be due to the large spatial scale. At the end of this section, we discuss the underlying geographic patterns that emerged in the data.

Some common predictors of SOC and dependencies between predictors (MAP/PET, pH_{H20}, CIA) emerged across all modeling approaches. Number of wet months, soil pH_{H2O} and weathering status (Figures 3 and 4) occurred as key parameters in the linear mixed effects models that influence how other parameters, such as Ca_{ex}, Al_{ox} and Fe_{ox}, explain SOC content variation across sub-Saharan Africa. In contrast, predictor differences were much smaller between topsoil (0–20 cm) and subsoil (20–50 cm) samples. This may partly be due to the large depth increments for each of the two sampling depths. However, since the identified SOC-controlling factors were similar for both depth layers, any differences were probably mostly driven by the fact that subsoil samples usually contain less SOC due to lower C inputs at depth (Jobbágy and Jackson 2000). Soil erosion at some sites might also dilute differences between the two depth layers, since water and wind can permanently remove surface soil.

These findings were supported by the regression trees (Figure A6) and partial dependence plots (Figure 5), where Ca_{ex}, Al_{ox} and Fe_{ox} seemed to be more important in explaining the variation of SOC concentration compared to pH_{H2O}, PET/MAP and CIA. For example, soil pH_{H2O} was important in the full linear mixed effects model, yet it mainly influenced Ca_{ex}, Al_{ox}, and Fe_{ox} concentrations in correlation with MAP (Figure 3d); the same was true for weathering (Figure 4b). Similar relationships have been found for temperate regions, where the importance of Ca_{ex} increased with increasing pH_{H2O} and decreasing precipitation, whereas the opposite was true for Al_{ox} (Oades 1988; Rasmussen et al. 2018). However, Rasmussen et al. (2018) did not identify Fe_{ox} as an important predictor of SOC content."

R_02: Lines 167-171: I understand that the transformation is necessary for comparing different predictors on the same scale. However, what does the transformation mean with respect to the functional relationships in the data? Are the models linear with respect to the original scale? I suspect not: a linear model fit to transformed data is not necessarily a linear model with respect to the original data. This is worth noting, even if the analysis stays the way it is.

A: It is correct that transformation and standardization of the data prior to linear mixed effects modelling does not mean that the original relationship between SOC and the predictors is always linear. We have clarified this in the text. This is one of the reasons why we also used regression trees and random forests. Fe_{ox}, for example, nicely demonstrates this point – it is not important in the linear mixed effects models, yet, it is really important in regression trees and random forests. This is because it does not have a linear relationship with SOC across its entire range – it only shows a strong correlation with SOC at low concentrations (see Figure 5e in the manuscript).

Line 198: "This only holds for the transformed and standardized data and the relationship between SOC and the predictors of the original data may not be linear."

R_02: Line 183: How was the hierarchical clustering done?

A: Thank you for the critical question. We realized that 'hierarchal clustering' is not the appropriate term here and apologize for any confusion that was caused by that. We used a built-in function called *cut_number* from the *ggplot2* package in *R* which allows to control for the number of groups, whereas the cut-offs are determined by the function internally to approximately equalize the number of samples in each group. We tried different numbers of groups to match common pH and CIA classes while trying to maximize the number of samples in each group (i.e. keeping the numbers of groups as small as possible) at the same time. The exact approach can be found in the R Markdown file in the SI (p 11-12). We have clarified this in the text.

Line 210: "Soil pH_{H2O} and CIA data were grouped ~~using hierarchical clustering,~~ with the number of classes chosen to maximize and equalize the number of samples in each class and to correspond with common pH_{H2O} and weathering categories (Nesbit and Young 1982)."

R_02: Line 204: The spatial partitioning is really laudable. It is surprising how infrequently this is done, and it really should be a community standard. Thank you for being rigorous!

A: Thank you very much for this really positive feedback. We agree that this should become a standard when working with geospatial data.

R_02: Line 242: Please introduce the marginal/conditional R-squared values before mentioning here. To many readers this distinction might not be obvious.

A: Since this is the only time we use the term marginal, we decided to remove it and added in brackets what the R^2 in this case means.

Line 272: "The final linear mixed effects model for the entire data set (n = 1,601) had a ~~marginal~~ R^2 of 0.71 (excluding the proportion variance explained by the fixed effects *Site/Cluster/Profile*)."

R_02: Figure 2: The univariate linear regression fits in this figure are purely for illustration? Perhaps mention them briefly in the statistical analysis section.

A: It is correct that we added those regression lines in Figure 2 for illustrative reasons. The regression line follows the simple linear equation y ~ x. Since the linear regression line in the figure is not used in further analysis and not important for the discussion, we have now included the formula and clarifying information in the caption of Figure 2, rather than in the methods section:

Line 264: "Figure 1: a) Soil organic carbon (SOC) content [wt-%] for the different land-covers (cropland, forest, grassland, other (bushland, shrubland, woodland) by depth (topsoil: 0–20 cm, subsoil: 20–50 cm); b) SOC [wt-%] and clay and fine silt content [%] by depth; c) SOC [wt-%] and clay and fine silt content [%] by depth for three example sites that show contrasting trends. Gray area around fitted linear regressions (y ~ x, for illustration only) in b) and c) show the 95% confidence interval. For the relationship between SOC and clay and fine silt content for all individual sites, see Figure A4."

R_02: Figure 3 (and throughout): How were confidence intervals obtained? They are reported throughout the paper, but unless I missed something the method used to obtain them is not reported.

A: It is correct that we did not specify the method how we obtained the confidence intervals – this has now been corrected. To visualize and report the linear mixed effects models (including the confidence intervals) in Figure 3a,b, 4a, and A5 and in Table B2 to Table B7 we used the *sjPlot* package in R (Lüdecke 2020). Within the package we used the Wald-test approximation to calculate the 95% confidence interval (https://easystats.github.io/parameters/reference/p_value_wald.html). Based on this method the confidence interval is calculated the follow for each explanatory variable:

$$\textit{Confidence interval (95\%)} = \textit{Coefficient} \pm 1.96 * \textit{SE}$$

Where *Coefficient* is the coefficient of each explanatory variable from the linear mixed effects model and *SE* is the standard error of the maximum likelihood of the same explanatory variable.

Line 192: "The 95% confidence intervals were obtained by using the Wald-test approximation within the sjPlot and parameters R packages (Lüdecke 2020; Lüdecke et al. 2020)."

R_02: Line 289: How was the % variation explained obtained here? Is this an R-squared value for a reduced model? Or is it some sort of variable importance metric? Perhaps something is missing from the methods description?

A: We replaced the word *variation* with *data* and added an explanation in the method section. The percentage is referring to the relative number of observations in this particular node of the regression tree. In this particular case, the SOC content of 23% of the samples was predicted by using Fe_{ox} and MAT only.

Line 236: "Absolute values at the bottom of each node indicate the predicted SOC content [wt-%] and the percentage corresponds to the relative number of samples in this node (Figure A6)."

Line 320: "About 23% of the SOC data could be explained by Fe_{ox} and MAT alone."

R_02: Line 446: I hope that the data presented in this study are eventually made available in some easy-to-access way. A database of this size and completeness could be extremely valuable to other researchers and would be best archived on some sort of data repository rather than only available on request from the author.

A: Thank you very much for bringing up this important aspect of *Open Science*. We fully agree that the analyzed data set should be open and easily accessible to everyone. In parallel to this review process we are already working on this. We are planning to archive the dataset on the following repository were already other data from the same project has been archived: https://data.worldagroforestry.org/dataverse/icraf_soils. However, we still need to solve some legal issues.

Line 501: "The soil properties data set used in this study is available from the corresponding author upon reasonable request and will be available on https://data.worldagroforestry.org/dataverse/icraf_soils in mid-2021."

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