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Comment on soil-2020-92

Anonymous Referee #3

Referee comment on "The role of geochemistry in organic carbon stabilization against microbial decomposition in tropical rainforest soils" by Mario Reichenbach et al., SOIL Discuss., https://doi.org/10.5194/soil-2020-92-RC3, 2021

Dear editor and authors,

I really appreciate the opportunity to read and comment on the manuscript Soil-2020-92 entitled "The role of geochemistry in organic carbon stabilization in tropical rainforest soils". Overall, the manuscript is very interesting and covers a topic of global interest, and as such, it would fulfill the requirements for publication in your Journal. I would recommend accepting, although a few minor issues should be dealt with first. I acknowledge the fact that other reviewers participated in the discussion and gave detailed suggestions to improve the original version of the manuscript. For the version of the text I have read, there are no major issues that would require extensive reworking. So, only minor issues will be pointed out in the following text and I hope my comments can still be useful.

Abstract

Lines 22-23. I could not find strong evidence to support the claim that "fluvial dynamics and changed hydrological conditions had a secondary control on SOC dynamics in valley positions, leading to higher SOC stocks there than at the non-valley positions". This should be better explained. How can the reader agree that "fluvial dynamics and changed hydrological conditions" can be inferred from the results reported and help to explain higher SOC stocks in valleys than in non-valley positions?

Lines 23-24. I believe the term "Fossil organic carbon" could be more precisely described and referred to as "geogenic organic carbon" in the whole manuscript. In fact, geogenic organic carbon was used by the authors themselves elsewhere in their text (e.g., page 3, line 116).

Introduction, Section 1.2. Environmental and geochemical controls on SOC dynamics in tropical forests

Lines 69-70. To what extent geogenic carbon (FOC) is preserved owing to its inherent

chemical properties (e.g., recalcitrance) or the specific conditions (e.g., burial of organic carbon mixed with mineral particles) under sedimentary environments?

Line 86. Please provide a reference in which the authors have reported pedogenic oxides contents above 50% in the clay fraction of tropical soils. I agree that some tropical soils can exhibit more than 50% of pedogenic oxides, but such soils are not the norm as implied in the text. Please check.

Introduction, Section 1.4 Study aims

Line 140. "composition" or "decomposition"?

Hypotheses

All hypotheses proposed should be rephrased and put into simpler functional relationships e.g., y = f(x). This would reduce verbosity and give the reader a glimpse on how each hypothesis would be effectively tested. I believe the hypotheses can be better used to guide the reader through the Discussion section as well.

Hypothesis (i): what parameters would be used/measured to determine the control of topography on lateral fluxes of water and mineral mass? This is not clear to me.

Hypothesis (ii): I understood the context, but verbosity can be reduced.

Hypothesis (iii): I found the third hypothesis particularly confusing as it includes a reference to "priming effects", which were not measured in this study.

Example to rephrase the third hypothesis:

iii) Geogenic soil carbon stocks vary more consistently as a function of soil depth than landscape position or soil parent material.

Results, Section 3.1 Climate and topography

I believe the supplement 1 could benefit the reader if kept in the main text, all results reported therein are very nice. Besides, as far as I understood hypothesis (i), the observation of higher soil C stocks in valleys than in non-valley positions is important for this research.

Lines 350-353. The inference that "Even though valley positions are of the same geochemistry as the non-valley positions, geochemical soil properties in valleys were significantly different than at non valley positions, as fluvial activity and sedimentation unrelated to hillslope processes were dominant", seems quite speculative to explain higher C stocks in valleys relative to non-valley positions. In my opinion, a predominant effect of "fluvial and sedimentation" rather than "hillslope processes" would make sense only if the geochemistry in the valleys were significantly different from that observed in non-valley positions.

Results, Section 3.3 Variation in SOC properties with geochemistry

Lines 410-411. In the sentence "Note that while SOCbulk decreased strongly with depth in the mafic and felsic region, only a weak decrease of SOCbulk with depth was observed in the mixed sedimentary region (Fig. 4)", can we infer that SOC buildup followed the accumulation of sediments over time to a greater extent than C inputs from the local vegetation? How does the ^{14}C depth-trend compare to that observed in valley positions as shown in Fig. S2?

Results, Section 3.5 Explained variability and relative importance of predictors (non-valley soils)

Lines 471-472. Based on the observation that "Soil depth and $rPC4_{nv}$ explained 73 % of variability (R^2) in SOCbulk ($p < 0.01$). Soil depth contributed 82 % to the explanatory power of the model", how (in)sensitive tropical C pools may be to changes in climate or land use?

Results, Section 3.6 Partial correlations controlled for soil depth

It looks quite amazing that when the effect of depth is controlled, the explanatory power of the other variables included in the model does not increase substantially (except silt content). How does this trend compare to temperate ecosystems? What can be inferred about the relationship between pedogenesis and soil C accumulation in the tropics? What is the mineralogy of the silt fraction? Given the data shown in Table 3 and Figure 5, such information is very important for this research and would facilitate the discussion (lines 570-578).

Discussion, Section 4.1 Soil C stabilization driven by soil chemistry and parent material

To what extent the inference that "In contrast to our initial hypothesis that topography affects C stabilization in tropical forest soils through lateral material movements, we found no indication of this in our analysis (Supplementary results and short discussion therein)" can be reconciled with the observation of higher SOC stocks in valley positions, despite exhibiting similar geochemistry to non-valley positions?

Conclusions and outlook

Lines 410-412. "Differences in $\Delta^{14}C$ were best explained with soil depth and the presence of FOC, which appears to be decomposable by microbial communities under more fertile, topsoil conditions." There is an apparent redundancy here since $\Delta^{14}C$ would co-variate with FOC and factors limiting microbial respiration at depth should be more important than soil fertility.

Please also note the supplement to this comment:

https://soil.copernicus.org/preprints/soil-2020-92/soil-2020-92-RC3-supplement.pdf