



Comment on soil-2020-96

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Community comment on "Heterotrophic soil respiration and carbon cycling in geochemically distinct African tropical forest soils" by Benjamin Bukombe et al., SOIL Discuss., <https://doi.org/10.5194/soil-2020-96-CC1>, 2021

The article "Controls on heterotrophic soil respiration and carbon cycling in geochemically distinct African tropical forests soils" investigates the role of soil chemistry, fertility and geochemical composition as drivers of soil respiration under laboratory conditions in soils collected along slope gradients in tropical Africa. The article fits the scope of the journal and it will be of great interest for the journal readers.

The introduction is to large extents well structured, though I am not sure if the sub-headers are really needed. The introduction contains a lot of information on geochemical (e.g. Al Fe SiO₂) parameters influencing soil C dynamics, but not so much on available P or N, which turn out in this study to be strong determinants of soil respiration. Moreover, little information is given in the introduction on the role of aggregation, or microbial biomass as C sequestration 'pump' (or also about microbial enzymatic control). I would recommend to extend each of the topics a bit more to make the introduction to make the link even stronger towards the research questions and to the results presented. In addition, I think the hypotheses could be more specifically state, e.g. in line 93 to 96 it would be helpful to mention which change in geochemical properties would cause which response by microbial decomposers more 'explicitly', by stating the expected mechanism, or by hypothesizing under which conditions faster or slower soil C turnover could happen, and how this has been influencing soil C stocks in the long term.

The material and methods provide a detailed characterization of the study sites and the respective soil properties including many references to articles that are currently in review, which is a bit difficult to trace. The incubation experiment set-up is very clearly described and sound. Also the statistical analysis is provided in detail, which is great. One minor point that I could suggest to improve the role of soil depth would be to explore linear models and include soil depth 'nested' into topographic position and geochemical region (or nested per sample location), as the different depths are not independent of each other.

The results are well described, some specific suggestions are given below, the figures and tables are adequate. The discussion section is relative to the other parts quite long and reads a bit lengthy. It could maybe be a bit shorter or more streamlined towards the initial research questions and hypotheses.

For me surprising was that in the discussion section further analyses were presented that appear rather as an extension of the results, and may in my opinion therefore also maybe be rather moved to the results section. In addition, it includes the analysis of much larger set of parameter, which have not really been introduced before (e.g. dissolved organic C, bioavailable P or enzymes), and I thought, which would already be included in the rPCA analysis? Maybe it would be better to also move the graphs in the appendix section, as they rather support already stated emerging patterns. In addition, terminology changes also a bit (e.g. mixed region vs. sediment region) – double check please.

Moreover, large parts of the discussion are rather discussion the results of the study (see in the technical comments), but there could be more discussion relating the findings to other results found in other tropical/subtropical/montane forest sites and put the results more in to a larger context.

Finally, in the discussion a large part is about microbial nutrient limitation, it would be great to already introduce this as a possible control in the introduction in more detail.

Technical comments:

Line 77: this could need a reference.

Line 222: could you provide a reference for subsoil conditions?

Line 255: I guess there were no real differences between the plateau, mid-slope, slope positions – still it would be great to mention, why these different locations were not considered anymore.

Line 281: The sentence 'Within non-valley positions...' is redundant.

Line 299-301: Does this describe exactly the same as is stated in Line 286-288?

Line 315: I don't understand the x indicating no significant difference between depth intervals within geochemical regions, I am not sure which differences the letters demonstrate – within regions or across all regions and depths. Please can you clarify this?

Line 324: should there not be two different results? Or should this indicate the D14C of bulk soil and of respired CO₂ were highly correlated.

Line 393: delete 'from it'.

Line 429. Maybe introduce indicators for N & P limitation of microbial decomposers a bit earlier already.

Line 429: Could you repeat what is considered as poor quality? (e.g. CN ratios of soil organic matter or any other parameters?)

Line 434: check sentence – lower compared to what – and check tenses – fossil C content 'was' low.

Line 439: is there maybe also another study that shows that organo-mineral complexation could be saturated depending on which organo mineral complexes are present in soil (e.g. Quesada 2020, Dötterl 2018).

Line 443: add after the brackets:), in our study aggregation...

Line 460: I would recommend to put this entire section (The role of mineral related C stabilization mechanisms) more into relation with other studies, at the moment, it is rather focusing on either studies from the same data set and reads a bit as an extended results section and could be shortened a bit.

Line 506 the same suggestion as above, I think this section can be shortened too, screen for repeated results.

Line 538: check tenses – there is sometimes a switch between present and past tense within sentences.

Line 570: namely twice in the same paragraph