Reviewer 1

Point-by-Point response

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.

Our response: We thank the reviewer for the overall positive evaluation, comments, and suggestions. In this response letter, the reviewer's comments are in italic, our responses listed always directly afterward. Suggested text that we will add or remove in the revised manuscript is stated between " " and new/changed text underlined.

Rev 1 Comment 1: *The introduction is to large extents well structured, though I am not sure if the subheaders are really needed.*

Our response: Thank you for this recommendation. We agree to remove the two subheaders and connect the two sections.

Rev 1 Comment 2: *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.*

Our response: We certainly agree. While the role of nutrients on carbon input and stock is briefly introduced in the first paragraph of section 1.2 (lines 71-75), the role of nutrients and microbial activity on soil carbon respiration could be further discussed. We will make the following revisions: We will extend the introduction and add information on nutrients in the revised manuscript, especially between lines 70-72 (see the suggested text underlined below). We will add information related to the work of (Fernández-Martínez et al., 2014; Liu et al., 2015; Jing et al., 2020; Kirsten et al., 2021; and Kallenbach et al., 2016, Mikutta et al., 2019).

"Change in the availability of nutrients such as nitrogen (N) and phosphorus (P) can alter the C cycle as they are tightly coupled to it due to the metabolic needs of plants and microorganisms. Studies have shown that CO_2 uptake by terrestrial ecosystems strongly depends on N and P availability (Fernández-Martínez et al., 2014). Furthermore, N and P can influence microbial growth and activities by changing organic substrate availability and quality (Liu et al., 2015;Jing et al., 2020). Nutrient availability is also driven by rock derived nutrients such as base cations. However, long-term chemical weathering in tropical systems has led to the depletion of rock-derived nutrients in soils and limited the capacity of microorganisms and plants to access new nutrients (Liu et al,2015; Vitousek and Chadwick, 2013). Therefore, variation in soil weathering stage and associated nutrients availability is likely to affect soil C turnover in tropical forests."

References added:

Fernández-Martínez, M., Vicca, S., Janssens, I. A., Sardans, J., Luyssaert, S., Campioli, M., Chapin Iii, F. S., Ciais, P., Malhi, Y., Obersteiner, M., Papale, D., Piao, S. L., Reichstein, M., Rodà, F. and Peñuelas, J.: Nutrient availability as the key regulator of global forest carbon balance, Nat. Clim. Chang. |, 4, doi:10.1038/NCLIMATE2177, 2014.

Jing, X., Chen, X., Fang, J., Ji, C., Shen, H., Zheng, C. and Zhu, B.: Soil microbial carbon and nutrient constraints are driven more by climate and soil physicochemical properties than by nutrient addition in forest ecosystems, Soil Biol. Biochem., 141, 107657, doi:10.1016/j.soilbio.2019.107657, 2020.

Liu, L., Gundersen, P., Zhang, W., Zhang, T., Chen, H. and Mo, J.: Effects of nitrogen and phosphorus additions on soil microbial biomass and community structure in two reforested tropical forests, Sci. Rep., 5, doi:10.1038/srep14378, 2015

Rev 1 Comment 3: *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 extending 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.*

Our response: We have introduced the role of aggregation briefly in lines 80-83. But we agree with the reviewer that this could be further strengthened. We propose to add the work of Fang et al. (2017), Rasmussen et al. (2018), Kirsten et al. (2021) and revise this section as follows:

"In contrast, stable microaggregates rich in iron (Fe) and aluminum (Al) oxyhydroxides found in abundance in tropical soils (Bruun et al., 2010; Torres-Sallan et al., 2017) seem to be of greater importance to stabilize C as concentrations of Fe and Al are commonly higher than in many temperate soils. For example, studies conducted across a wide range of tropical ecoregions showed that SOC is mainly regulated by Fe or Al-(hydr) oxides, more so than by clay content. In contrast, clay content is identified in many temperate regions as a major control on SOC (Rasmussen et al., 2018, Fang et al., 2018)".

References added:

Torres-Sallan, G., Schulte, R. P. O., Lanigan, G. J., Byrne, K. A., Reidy, B., Simó, I., Six, J. and Creamer, R. E.: Clay illuviation provides a long-term sink for C sequestration in subsoils, Sci. Rep., 7(1), 45635, doi:10.1038/srep45635, 2017.

Rasmussen, C., Heckman, K., Wieder, W. R., Keiluweit, M., Lawrence, C. R., Berhe, A. A., Blankinship, J. C., Crow, S. E., Druhan, J. L., Hicks Pries, C. E., Marin-Spiotta, E., Plante, A. F., Schädel, C., Schimel, J. P., Sierra, C. A., Thompson, A. and Wagai, R.: Beyond clay: towards an improved set of variables for predicting soil organic matter content, Biogeochemistry, 137, 297–306, doi:10.1007/s10533-018-0424-3, 2018.

Fang, K., Qin, S., Chen, L., Zhang, Q. and Yang, Y.: Al/Fe Mineral Controls on Soil Organic Carbon Stock Across Tibetan Alpine Grasslands, J. Geophys. Res. Biogeosciences, 124, 247–259, doi:10.1029/2018JG004782, 2019.

Kirsten, M., Mikutta, R., Vogel, C., Thompson, A., Mueller, C. W., Kimaro, D. N., Bergsma, H. L. T., Feger, K. H. and Kalbitz, K.: Iron oxides and aluminous clays selectively control soil carbon storage and stability in the humid tropics, Sci. Rep., 11(1), 1–12, doi:10.1038/s41598-021-84777-7, 2021.

Rev1 Comment 4: *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.*

Our response: We will revise the hypotheses following the key points suggested. We propose to change lines 93-96 as follows: "We hypothesize that (1) specific soil respiration and the $\Delta^{14}C$ signature of potential soil respiration in tropical soils are primarily controlled by geochemical properties related to soil fertility (such as potential cation exchange capacity and the sum of base cations) derived and varying with soil parent material. These variations in soil fertility can stimulate or inhibit microbial activity and increase or decrease soil C decomposition rates. (2) The presence of pedogenic oxides can increase SOC stocks and decrease C respiration rate by creating an energetic barrier for C decomposers through direct complexation with organic molecules or by forming stable (micro)aggregates. (3) Topography controls specific soil respiration and its $\Delta^{14}C$ signature indirectly through erosion and deposition processes as well as the hydrological conditions that limit soil microbial growth and activity and slow down soil C decomposition rates."

Rev 1 Comment 5: 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 setup 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.

Our response: Thank you for this suggestion. We will revise the description of the study sites and present the chemical characteristics in the form of a table as suggested by the second reviewer as well. The text will be revised and shortened. Regarding the statistics and the nested approach, we explored this approach but it did not improve our results. We tested two generalized linear models: one with all factors including geochemical regions, depth intervals, and their interaction; and another one where depth intervals are nested within geochemical regions. We compared the two models using the ANOVA test. The result shows that nesting does not improve the results (see Table below).

Table: Analysis of deviance between Model 1 (where depth intervals are nested within geochemical region) and Model 2 (where we consider interaction of geochemical region and depth intervals). The ANOVA() function performs a test comparing the two models. Here the associated deviance is nearly zero. This provides evidence that model 2 containing the interaction is superior to model 1 with depth interval nested within geochemical region.

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Analysis of Deviance
Model 1: sqrt(SPR) ~ region/Depth Interval
Model 2: sqrt(SPR) ~ region: Depth Interval
  Resid. Df Resid. Dev Df   Deviance
1        76     6.6006
2        76     6.6006  0 1.7764e-15
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Rev 1 Comment 6: *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.*

Our response: We agree to shorten the discussion section in the revised manuscript. We will do this while keeping the main ideas that are in line with the research questions and hypotheses. We propose the following revision in the discussion part:

" We will remove lines 421-428 (see removed text underlined below) due to their minimal contribution to the discussion. In addition, lines 426-427 provide the same information as in lines 407-410.

<u>While bioavailable P showed a clear distinction between geochemical regions, no strong linear trends were identified with respect to SPR or the $\Delta^{14}C$ of respired CO_2 (Fig. 4a, c). In contrast, total dissolved nitrogen was strongly correlated with SPR and the $\Delta^{14}C$ of respired CO_2 (Fig. 4b, e) in particular for the mafic and felsic regions. Interestingly, rates of activity for extracellular enzymes mining C, N, and P (data from Kidinda et al., 2020 in review) are similar across all three geochemical regions but differ between top and subsoil. While the activities of N and P mining enzymes were positively correlated to SPR ($p = 0.01$-0.1) in the felsic and mafic region, we found no significant correlation for the mixed sediment region (Fig. 4d-f). No significant correlation with SPR was found for dissolved organic carbon or C mining enzymes for any region (data not shown)."</u>

Rev 1 Comment 7: *In addition, it includes the analysis of a much larger set of parameters, 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.*

Our response: Figure 4 presents variables initially included in the rPCA and represented by the RCs. While the RCs address issues like autocorrelations and model overfitting, they don't provide information on direct relationships between initial variables. In Figure 4 we try to present these relationships masked by RCs. We agree that having this information in the appendices would shorten the discussion section and improve the readability. In the revised manuscript, we will move Figure 4 into the appendix section and revise it to the most essential parts related to the discussion. We believe that these changes will create a direct connection between discussion sections and improve readability.

Rev 1 Comment 8: *In addition, terminology changes also a bit (e.g. mixed region vs. sediment region) – double check, please.*

Our response: Thank you for noting this. Based on the elemental composition of the bedrock (see methods), we classified our study area into three geochemical regions including the mafic region, felsic region, and mixed sediment region. We will address any inconsistency throughout the manuscript.

Rev 1 Comment 9: *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.*

Our response: Since this comment is connected to other specific comments (detailed further below), we show briefly how we plan to address each of those comments here. But for details, we direct the reviewer to our specific responses to technical comments related to the discussion. We have planned to make the following changes in the revised manuscript:

1. We will remove lines 474-475; and lines 485-492: as it is overlapping with another paragraph in the "fertility and microbial" section (lines 409-420).
2. We will revise lines 460-468 as detailed in comment 23. In this section, we shortened the discussion on the role of mineral related C stabilization and tried to put it into a larger context in relation to other studies.
3. We also propose to revise lines 497-504 as suggested in comment 24. Here, we screened the text for repeated results and removed them to shorten the discussion.
4. Our response to the question related to microbial nutrient limitation can be found in the response to comment 2. Here, we proposed changes in the introduction section and introduced the nutrient-microbial relationship.

Rev 1 Comment 10: *Line 77: this could need a reference.*

Our response: We will support this statement with information from Addo-Danso et al. (2018).

Reference added:

Addo-Danso, S. D., Prescott, C. E., Adu-Bredu, S., Duah-Gyamfi, A., Moore, S., Guy, R. D., Forrester, D. I., Owusu-Afriyie, K., Marshall, P. L. and Malhi, Y.: Fine-root exploitation strategies differ in tropical old growth and logged-over forests in Ghana, Biotropica, 50(4), 606–615, doi:10.1111/btp.12556, 2018.

Rev 1 Comment 11: *Line 222: could you provide a reference for subsoil conditions?*

Our response: To our knowledge, this is the first study of its kind conducted in our study area on subsoil. Previous studies have mainly reported external environmental conditions. However, previous studies conducted in other tropical regions reported similar results for subsoils. Our statement is supported by the work of Wood et al. (2013) and we will add this information in the revised manuscript.

Reference added:

Wood, T. E., Detto, M. and Silver, W. L.: Sensitivity of soil respiration to variability in soil moisture and temperature in a humid tropical forest, PLoS One, 8(12), doi:10.1371/journal.pone.0080965, 2013.

Rev 1 Comment 12: *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.*

Our response: That is correct, there were no differences between plateau, upper slope, and midslope. As this was a result of statistical analysis, we decided to present this information in the first section of our results in place of the method section (see Lines 277-283 as shown below) and the paragraph below. Since the confusion came earlier in the statistical analysis section, we will therefore move this paragraph back to the statistical analysis section to avoid any confusion. We propose to move the following paragraph between lines 253-254.

"We found no statistical difference in specific potential respiration (SPR), total potential respiration (TPR), and radiocarbon content ($\Delta^{14}C$) between the plateau and slope positions within each studied geochemical region (mafic, felsic, and mixed sediment). Across geochemical regions and soil depths, depth profiles of SPR, TPR, and $\Delta^{14}C$ differed only between valleys and non-valley positions (see discussion for details). For non-valley positions, no statistically significant differences for SPR, TPR, and $\Delta^{14}C$ were found between sloping and plateau positions. Hence, all further analyses were done after splitting the data into two subsets: (1) Non-valley positions (plateau, upper slope, and midslope) versus (2) valley positions (valleys and foot slopes) (Fig. 1, 2)".

Rev 1 Comment 13: *Line 281: The sentence 'Within non-valley positions…' is redundant.*

Our response: Thank you for noting this. This is indeed redundant as the levels of non-valley positions are specified at the end of this line. We will revise this sentence as follows: "No statistically significant differences for SPR, TPR, and $\Delta^{14}C$ were found between the plateau and sloping positions."

Rev 1 Comment 14: *Line 299-301: Does this describe exactly the same as is stated in Line 286-288?*

Our response: Our aim was to describe the results at the levels of topography and geochemical regions separately. However, the two paragraphs provide indeed the same information. In the revised version, we will exclude lines 299-301(see text below):
"Differences in the subsoil, however, were not significant across regions (Fig. 1C). $\Delta^{14}C$ activity of both soil and respired CO_2 in the mafic and felsic regions were not significantly different from each other for both top- and subsoil to avoid repetition."

Rev 1 Comment 15: *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?*

Our response: For the ANOVA analysis, we did the pairwise comparisons between soil depth intervals within geochemical regions and between soil depth intervals across geochemical regions. For Fig.1, x indicates "no significant difference between depth intervals within geochemical region. ANOVA tests were performed separately for non-valley and valley positions."

Rev 1 Comment 16: *Line 324: should there not be two different results? Or should this indicate the Δ14C of bulk soil and of respired CO2 were highly correlated.*

Our response: The sentence presents one result " the relationship between $\Delta^{14}C$ of bulk soil and $\Delta^{14}C$ respired CO_2". However, we agree that it might be confusing. We will revise this line as follows: We found a strong relationship (high R^2) between $\Delta^{14}C$ of the bulk soil and $\Delta^{14}C$ of the respired CO_2 ($R^2 = 0.81$, $p<0.1$).

Rev 1 Comment 17: *Line 393: delete 'from it'.*

Our response: Thank you for noting this, we will amend this in the revised version.

Rev 1 Comment 18: *Line 429. Maybe introduce indicators for N & P limitation of microbial decomposers a bit earlier already.*
Our response: We will add information in the introduction section, highlighting N and P limitations to microbial decomposers. We will add the following statement before line 429:

"Our data suggest that in mixed sediment region, poor soil fertility likely slows down rates of C cycling in soil. Soils in this region had the lowest available nutrients, with substantially lower concentrations of bioavailable P and N than soils in the mafic and felsic regions (Fig. 4). This adds to existing literature suggesting that nutrient limitation, especially N and P, can significantly slow down microbial growth and activity, hence lowering soil C turnover rates (Fang et al., 2014, Kunito et al., 2009)."

Rev 1 Comment 19: *Line 429: Could you repeat what is considered as poor quality? (e.g. CN ratios of soil organic matter or any other parameters?)*

Our response: We used lower nitrogen and higher C:N values to characterize the poor quality of SOC in the mixed sediment region. To clarify this statement, we will revise line 429 as follows: "In addition, the depletion of N and high C:N values (153.9 ± 68.5) of fossil organic C likely contributed to reducing soil respiration rates in the mixed sediment region (Whitaker et al., 2014)."

Reference added:

Whitaker, J., Ostle, N., McNamara, N. P., Nottingham, A. T., Stott, A. W., Bardgett, R. D., Salinas, N., Ccahuana, A. J. Q. and Meir, P.: Microbial carbon mineralization in tropical lowland and montane forest soils of Peru, Front. Microbiol. , 5, 720: https://www.frontiersin.org/article/10.3389/fmicb.2014.00720, 2014.

Rev 1 Comment 20: *Line 434: check sentence – lower compared to what – and check tenses – fossil C content 'was' low.*

Our response: We will revise this statement as follows: "However, respiration rates in the topsoil of the mixed sediment were also lower compared to the mafic or felsic region (Fig. 1), and fossil organic C content was low compared to the subsoil (Table 1). Thus, we conclude that soil fertility constraints such as soil exchangeable bases, and bioavailable P (Table B2) are likely more important contributors to lower respiration rates in the mixed sediment region than the presence of fossil organic C content."

Rev 1 Comment 21: *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).*

Our response: We will add new references supporting this statement and revise the sentence as follows: ".....but can be limited by the quality of organic matter sources, or by barriers related to organo-mineral complexation (Rasmussen et al., 2018; Quesada et al., 2020; Traoré et al., 2020; Kirsten et al., 2021)."

Rev 1 Comment 22*: Line 443: add after the brackets:), in our study aggregation…*

Our response: Thank you for noting this, we will revise this sentence.

Rev 1 Comment 23: *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.*

Our response: We agree to revise and shorten this section and put it in a general context in relation to other studies. We will revise "The role of mineral related C stabilization mechanisms" section as follows:

1. We will remove lines 474-475: this does not contribute much to the discussion. In addition, the data discussed here is not presented.
2. We will remove lines 485-492: This paragraph has some overlap with another paragraph in "the fertility and microbial" section (lines 409-420).
3. We will revise lines 460-468 as follows:

The role of mineral related C stabilization mechanisms

"Soil C stabilization mechanisms can play an important role in C cycling as reflected by considerable variability of both SPR and $\Delta^{14}C$ signatures in the investigated soils (Fig. A1). For tropical soils, these mechanisms are mainly driven by the abundance of Fe or Al-oxides that improve aggregate stability and ultimately limit microbial activity (Nagy et al., 2018; Kirsten et al., 2021). The importance of pedogenic oxide for SOC stocks was also observed in studies across a variety of ecoregions (Barthès et al., 2008; Rasmussen et al., 2018; Traoré et al., 2020; Quesada et al., 2020). However, despite their importance for SOC stocks, our data did not show a significant relationship between SPR and Fe or Al-oxides across soils in the investigated geochemical regions, except for a negative correlation for the mafic region (Fig. A1b, c). Even though the mafic soils were generally more fertile than soils in the felsic or mixed sediment region, SPR was lower and decreased more strongly with depth in mafic soils (75% decrease in deep subsoil compared to topsoil) than in felsic (33% decrease) soils (Fig. 1a). We argue that SOC stocks in the mafic region are higher and SPR lower due to the presence of mineral related stabilization mechanisms that are lacking in other regions. Consistent with this argument, Reichenbach et al. (2021 in review) found that higher SOC stocks in the mafic region compared to the felsic region are driven by higher amounts of Fe and Al pedogenic oxides that can build stable complexes with organic matter and support the formation of stable microaggregates."

References added:

Nagy, R. C., Porder, S., Brando, P., Davidson, E. A., Figueira, A. M. e. S., Neill, C., Riskin, S. and Trumbore, S.: Soil Carbon Dynamics in Soybean Cropland and Forests in Mato Grosso, Brazil, J. Geophys. Res. Biogeosciences, 123, 18–31, doi:10.1002/2017JG004269, 2018.

Rasmussen, C., Heckman, K., Wieder, W. R., Keiluweit, M., Lawrence, C. R., Berhe, A. A., Blankinship, J. C., Crow, S. E., Druhan, J. L., Hicks Pries, C. E., Marin-Spiotta, E., Plante, A. F., Schädel, C., Schimel, J. P., Sierra, C. A., Thompson, A. and Wagai, R.: Beyond clay: towards an improved set of variables for predicting soil organic matter content, Biogeochemistry, 137, 297–306, doi:10.1007/s10533-018-0424-3, 2018.

Traoré, S., Thiombiano, L., André Bationo, B., Kögel-Knabner, I. and Wiesmeier, M.: Organic carbon fractional distribution and saturation in tropical soils of West African savannas with contrasting mineral composition, doi:10.1016/j.catena.2020.104550, 2020.

Quesada, C. A., Paz, C., Oblitas Mendoza, E., Phillips, O. L., Saiz, G., and Lloyd, J.: Variations in soil chemical and physical properties explain basin-wide Amazon forest soil carbon concentrations, SOIL, 6, 53–88, https://doi.org/10.5194/soil-6-53-2020, 2020.

Kirsten, M., Mikutta, R., Vogel, C., Thompson, A., Mueller, C. W., Kimaro, D. N., Bergsma, H. L. T., Feger, K. H. and Kalbitz, K.: Iron oxides and aluminous clays selectively control soil carbon storage and stability in the humid tropics, Sci. Rep., 11(1), 1–12, doi:10.1038/s41598-021-84777-7, 2021.

Rev 1 Comment 24: *Line 506 the same suggestion as above, I think this section can be shortened too, screen for repeated results.*

Our response: Thank you for this comment. We will address this in the revised version of the manuscript. We propose the following changes in this section:

1. We will remove lines 497:504 since this paragraph discusses ideas that are already covered in the previous sections.
2. We will then revise lines 497-504 as follows:

"The presence of fossil organic C in the mixed sediment region (up to 52% of SOC stock in deeper subsoil) (Table 1), had a marked effect on SOC stocks in subsoils that would otherwise be similarly low to those of the felsic region (Fig. 1c). Consistent with this finding, a recent study shows that fossil organic C can largely contribute to SOC in subsoils (Kalk et al., 2020, in review). While fossil organic C in our study region is of poor quality as indicated by high C:N ratio values and depleted N, our data shows that fossil organic C was microbially available, under certain environmental conditions (i.e. topsoil conditions (Fig. 2))."

Rev 1 Comment 25: *Line 538: check tenses – there is sometimes a switch between present and past tense within sentences.*

Our response: Thank you for noting this. "Observe" should be "observed". We will correct this sentence and check for similar inconsistencies throughout the manuscript.

Rev 1 Comment 26: *Line 570: namely twice in the same paragraph*

Our response: We will delete the second (line 572) and revise that line as follows: C stabilization mechanisms, including the presence or absence of pedogenic oxides between our geochemical regions, were identified as indirect controls to explain variation in soil respiration through their effect on soil aggregation.