**Point-by-point response to anonymous Referee#3 comments**

Dear Referee#3,

We would like to thank you for your time and thorough evaluation of our manuscript "*The role of geochemistry in organic carbon stabilization in tropical rainforest soils*", (https://doi.org/10.5194/soil-2020-92). We are very pleased that you positively assessed our work and recognized its relevance. Your comments helped us to significantly improve our manuscript and we want to sincerely thank you for the constructive and valuable insights.

We have addressed all comments and suggestions to the best of our ability. Please find below a point-by-point response to all the concerns raised and how we addressed them. Reviewer original comments are highlighted in grey. New text to be added or modified in the manuscript has quotation marks and is blue-colored in the response.

We hope you find our response and changes to the manuscript satisfying and we are looking forward to hearing from you.

Yours sincerely,

The authors

*REVIEWER#3 COMMENT 1: "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?"*

**Our response:** Thank you very much for pointing this out. Indeed, we agree that this statement needs more clarification. We can´t provide solid quantitative data to show that soil moisture and oxygen conditions are different in the valley compared to non-valley positions. But based on our qualitative field observations in the valley positions (e.g. nearby river systems, high water saturation) and soil profile descriptions (e.g. fluvial materials, gleyic properties) we concluded that the soil environmental conditions are markedly different compared to non-valley positions. This is especially true for the valley positions in the mixed sedimentary region. Despite having the lowest content of pedogenic oxides, which are highly relevant in C stabilization at the corresponding non-valley positions, we observed similar SOC stocks in the valley as for non-valley positions. Valley positions in the mafic region show even higher SOC stocks although having less pedognic oxides compared to non-valley positions. Based on this missing link between SOC stocks and stabilization partners, we infer that C stabilization in valley positions is dominantly driven by reduced decomposition rates as a function of restricting soil environmental conditions resulting from fluvial activity.

We would expand the short discussion in the supplements to provide further information to the reader.

Additions in supplementary short discussion:

"Valley bottoms might be affected not only by sediments derived from associated hillslopes but also from material redistribution in the entire catchment during flood events (Douglas and Guyot, 2005). In addition, soil moisture conditions at the valley bottoms might be affected not only from interflow from the hillslopes but also from temporarily high ground water levels (Bonell, 2005). These fluvial and hydraulic conditions then lead to higher SOC stocks there than at the non-valley positions by reducing the decomposition rates as a function of restricting soil environmental conditions (Wiaux et al., 2014). For example, despite having the lowest content of pedogenic oxides, which are important stabilization partners at the non-valley positions, we observe similar SOC stocks in the valley positions as for non-valley positions in the mixed sedimentary region. Valley positions in the mafic region show even higher SOC stocks although having less pedognic oxides compared to non-valley positions."

*Used Literature:*

Bonell, M.: Runoff generation in tropical forests, in: Forests, Water and People in the Humid Tropics, edited by: Bonell, M., and Bruijnzeel, L. A., Cambridge University Press, New York, USA, 314-406, 9780521829533, 2005.

Douglas, I., and Guyot, J. L..: Erosion and sediment yield in the humid tropics, in: Forests, Water and People in the Humid Tropics, edited by: Bonell, M., and Bruijnzeel, L. A., Cambridge University Press, New York, USA, 407-421, 9780521829533, 2005.

Wiaux, F., Cornelis, J.-T., Cao, W., Vanclooster, M., and Van Oost, K.: Combined effect of geomorphic and pedogenic processes on the distribution of soil organic carbon quality along an eroding hillslope on loess soil, Geoderma, 216, 36-47, 10.1016/j.geoderma.2013.10.013, 2014.

*REVIEWER#3 COMMENT 2: "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)."*

**Our response:** Thanks for pointing out this discrepancy. In fact, this was also a comment from reviewer#1 and I kindly repeat the according response here:

"Fossil organic carbon (FOC) is of geogenic origin. It is organic carbon deposited during sedimentation and undergoes coalification or kerogen transformation during diagenesis (Buseck and Beysacc, 2014). In our study, FOC is characterized by high C / N ratios, depleted in N and free of $^{14}C$ due to the high age of rock formation. We have now defined the term FOC more precisely in the according sentence in the abstract:

"At several sites we also detected fossil organic carbon (FOC), which is organic C of geogenic origin and is characterized by high C / N ratios, depletion of N and free of $^{14}C$. Here, FOC constitutes up to 52.0 ± 13.2 % of total SOC stock in the C depleted subsoil.""

The term fossil organic carbon describes its origin the best since it is part of the fossil-fuel formation process (Berner, 2003). Therefore, we would like to keep the term "fossil organic carbon (FOC)" throughout the manuscript, since this term is used by other colleagues studying FOC also in a soil biogeochemical context (Bukombe et al., 2021). This helps to be consistent with terms across connected manuscripts in the same special issue.

*Used Literature:*

Berner, R. A.; The long-term carbon cycle, fossil fuels and atmospheric composition, Nature, 426, 323-326, https://doi.org/10.1038/nature02131, 2003.

Bukombe, B., Fiener, P., Hoyt, A. M., Doetterl, S.: Controls on heterotrophic soil respiration and carbon cycling in geochemically distinct Afrcian tropical forest soils, SOIL DISCUSSIONS, in review, https://doi.org/10.5194/soil-2020-96, 2021.

Buseck, P. R. and Beyssac, O.: From organic matter to graphite: Graphitization, Elements, 10, 421–426, https://doi.org/10.2113/gselements.10.6.421, 2014.

*REVIEWER#3 COMMENT 3: "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?"*

**Our response:** Thank you very much for this interesting question. FOC is better described as a C fraction with long turnover times and not as a recalcitrant C pool. Our study in accordance with other recent studies show that FOC is dynamic and decomposable (Bukombe et al., 2021; Hemmingway et al., 2018). The decomposition of FOC is most likely a energy demanding process since it undergoes coalification and kerogen transformation (Buseck and Beysacc, 2014) and microbes will preferentially consume more easily available organic C forms similar to the case of pyrogenic C (Czimiczik and Masiello, 2007; Knicker, 2011). The long turnover times are potentially even more enhanced, when the FOC bearing parent material is distal from the surface and not exposed to weathering and microbial processes.

*Used Literature:*

Bukombe, B., Fiener, P., Hoyt, A. M., Doetterl, S.: Controls on heterotrophic soil respiration and carbon cycling in geochemically distinct Afrcian tropical forest soils, SOIL DISCUSSIONS, in review, https://doi.org/10.5194/soil-2020-96, 2021.

Buseck, P. R. and Beyssac, O.: From organic matter to graphite: Graphitization, Elements, 10, 421–426, https://doi.org/10.2113/gselements.10.6.421, 2014.

Czimczik, C. I., and Masiello, C. A.: Controls on black carbon storage in soils, Global Biogeochem. Cycles, 21, 1-8, 10.1029/2006GB002798, 2007.

Hemingway, J. D., Hilton, R. G., Hovius, N., Eglinton, T. I., Haghipour, N., Wacker, L., Chen, M.-C., and Galy, V. V.: Microbial oxidation of lithospheric organic carbon in rapidly eroding tropical mountain soils, Science, 360, 209–212, https://doi.org/10.1126/science.aao6463, 2018.

Knicker, H.: Pyrogenic organic matter in soil: its origin and occurrence, its chemistry and survival in soil environments, Quat. Int., 243, 251-263, 10.1016/j.quaint.2011.02.037, 2011.

*REVIEWER#3 COMMENT 4: "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."*

**Our response:** Thanks for pointing this out as it clearly shows an apparent typo in the manuscript. According to the meta-analysis of Ito and Wagai (2017), tropical soils contain up to 15 % Fe-oxides (and not 50 %) in the clay fraction which is still much higher compared to temperate soils. The sentence will be corrected accordingly:

"Clay-sized mineral fractions in tropical soils are composed of up to 15 % pedogenic oxides, which is usually much higher than in temperate soils (Ito and Wagai, 2017)."

*Used Literature:*

Ito, A., and Wagai, R.: Data descriptor: global distribution of clay-size minerals on land surface for biogeochemical and climatological studies, Sci. Data, 4, 1-11, 10.1038/sdata.2017.103, 2017.

*REVIEWER#3 COMMENT 5: "Line 140. "composition" or "decomposition"?"*

*Our response:* Thanks. This typo will be corrected:

"In contrast, valley positions will show higher SOC stocks compared to plateau positions due to deposition of C-rich topsoil material and limited decomposition of SOC."

*REVIEWER#3 COMMENT 6: "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."*

**Our response:** This is a very important comment. For clarifying hypothesis (i): Using rock-derived macronutrients like P, Ca, Mg and K and C stabilization-relevant mineral phases like pedogenic oxides together with clay content as a proxy for soil redistribution, we would have expected pronounced differences between plateau, slopes and valley positions. In the case of simple soil redistribution along the catenae, we would have expected lower nutrient/ mineral contents at eroding slopes and higher contents at the depositional valley positions (Wiaux et al., 2014). In the case of enhanced weathering along eroding slopes triggered by soil rejuvenation by exposing unweathered parent material to the surface, we would have expected more nutrients/ minerals along the eroding slopes compared to stable plateau positions (Chadwick and Asner, 2016). These topography-driven differences in mineral content should be correlated with differences in SOC stock across the catenae. We would like to refer to the manuscript section "1.3 Topographic controls on SOC dynamics in tropical forests" where the above mentioned processes are briefly discussed. However, we could not observe such differences caused by hillslope processes when using soil geochemical properties as a proxy parameter. This is in line with a recent study of our colleague who analyzed $^{239+240}$Pu activity and inventories as a means for direct measurement of erosional soil removal. Here we found that the $^{239+240}$Pu, inventories, sampled along the same catenae as used in our study did not show topographic patterns, which indicates little or no soil erosion (Wilken et al., 2020).

For clarifying hypothesis (iii): We would like to refer to the connected study of our colleague, who studied the soil microbial activity on the same soil samples. Here, we show that microbial activity and readily available nutrients are highest in the topsoils across all studied gradients. Nevertheless, the authors agree that mentioning priming effects in the hypothesis lead to confusion since it was not directly measured in this study.

Therefore, we would rephrase the hypotheses in the study aim section accordingly to reduce verbosity:

"(i) SOC stocks and geochemical soil properties sensitive to soil redistribution will vary as a function of topographic position.

(ii) C stabilization mechanisms in highly weathered tropical soils will be driven by geochemical soil properties as a function of parent material composition.

(iii) Fossil organic carbon content in C-bearing parent material will vary as a function of soil depth."

*Used Literature:*

Chadwick, K. D., and Asner, G. P.: Tropical soil nutrient distributions determined by biotic and hillslope processes, Biogeochemistry, 127, 273-289, 10.1007/s10533-015-0179-z, 2016.

Kidinda, L. K., Olagoke, F. K., Vogel, C., Kalbitz, K., and Doetterl, S.: Patterns of microbial processes shaped by parent material and soil depth in tropical rainforest soils, SOIL DISCUSSIONS, in review, https://doi.org/10.5194/soil-2020-80, 2020.

Wiaux, F., Cornelis, J.-T., Cao, W., Vanclooster, M., and Van Oost, K.: Combined effect of geomorphic and pedogenic processes on the distribution of soil organic carbon quality along an eroding hillslope on loess soil, Geoderma, 216, 36-47, https://doi.org/10.1016/j.geoderma.2013.10.013, 2014.

*REVIEWER#3 COMMENT 7: "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."*

**Our response:** Thank you for this positive feedback. We have been thinking about this ourselves and since we wanted to keep the result and discussion part streamlined and focused on mineral-related C stabilization, we would prefer to keep the supplements separate from the manuscript as it deals more with a soil environmental-driven stabilization mechanism. The focus of this manuscript are C stabilization mechanisms which are closely related to mineral-organic interactions and how they differ across soil geochemistry. Since we can exclude recent deposition of soil material from slopes in the valleys, we infer that valley SOC stocks are more driven by soil hydrological conditions than by mineral-associated C stabilization which are dominant at non-valley positions (see manuscript L350-353). In addition, the content of pedogenic oxides in valleys is similar or even less compared to non-valley positions. Therefore, higher valley SOC stocks cannot be explained by more abundant stabilization partners or thereby enhanced microaggregation. Thus, we interpret higher SOC stocks in the valley compared to non-valley positions as a result of reduced decomposition rates caused by variations in e.g. soil moisture content (see supplementary discussion).

However, if the editors share the same opinion as the reviewer, we happily incorporate the supplements to the manuscript.

*REVIEWER#3 COMMENT 8: "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."*

**Our response:** Thanks for this comment. I would kindly refer to the above response to reviewer#3 comment 1 and 7 as it deals with a similar topic. We admit that our conclusion is based on qualitative field observations and descriptive data of SOC stocks and oxide content and not on statistical tests due to the small sample size for valley positions and missing soil hydrological data (e.g volumetric soil moisture content, electrical conductivity, soil temperature). But we can observe very low pedogenic oxides combined with high SOC stocks in the valley positions of the mixed sedimentary region. Here, valleys are characterised by a high groundwater table. It is most likely that the majority of pedogenic oxides are reduced and washed away by fluvial dynamics as shown by the gleyic properties which we noted during our profile description work. Furthermore, valley bottoms might be affected by material redistribution in the entire catchment during flood events overprinting the mineralogical effect of the parent material on SOC dynamics as indicated by changes in gravel content and texture. Thus, soil moisture conditions at the valley bottoms and temporarily high groundwater levels may be restricting microbial activity. Fluvial and hydraulic conditions then lead, indirectly, to higher SOC stocks there than at the non-valley positions. This conclusion is rather hypothetical but provides the most likeable and easiest explanation with the data on hand.

*REVIEWER#3 COMMENT 9: "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 14C depth-trend compare to that observed in valley positions as shown in Fig. S2?"*

**Our response:** Thanks for this interesting question. We respectfully disagree with the inference that SOC stock buildup followed the accumulation of C bearing sediments and outpaced C input of local vegetation since we are talking about very different time scales. The deposition of organic material and its diagenetic transformation (e.g. kerogen transformation and coalification) happened over geological timescales (several 100 Ma years; Schlüter and Trauth, 2006) long before pedogenesis and vegetation evolution of the recent soil system started, whereas the turnover of recent C input happens on a decadal to millennial time scale (Trumbore, 2000; Trumbore, 2009). The authors would argue that during soil formation the topsoil SOC stocks are dominated by faster cycling plant-derived C pools whereas the subsoil SOC stocks are more influenced by FOC released from the sedimentary rock which cycles much slower when entering microbial-mediated SOC dynamics (probably due to the high energy demand for breaking down FOC). Interestingly, we observe comparable $\Delta^{14}C$ depth trends both in non-valley and valley positions which may indicate that FOC is not varying in its persistence with topography and changing environmental conditions.

*Used Literature:*

Schlüter, T., and Trauth, M. H.: Geological atlas of Africa: with notes on stratigraphy, tectonics, economic geology, geohazards and geosites of each country, Springer, Berlin, New York, 272 pp., 2006.

Trumbore, S.: Age of soil organic matter and soil respiration: radiocarbon constraints on belowground C dynamics, Ecol. Appl., 10, 399-411, https://doi.org/10.1890/1051-0761(2000)010[0399:AOSOMA]2.0.CO;2, 2000.

Trumbore, S.: Radiocarbon and soil carbon dynamics, Annu. Rev. Earth Planet. Sci., 37, 47-66, 10.1146/annurev.earth.36.031207.124300, 2009.

*REVIEWER#3 COMMENT 10: "Lines 471-472. Based on the observation that "Soil depth and rPC4nv explained 73 % of variability (R2 ) 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?"*

**Our response:** This is indeed a very relevant question. In general, tropical soils are very sensitive to land use change and SOC stocks are decreasing after conversion from forest to cropland as a result of reduced C input and tillage-induced destabilization of mineral-protected C (Guillaume et al., 2015; Jackson et al., 2017). At the same time, tropical soils strongly depend on this stabilization mechanism since C input is prone to fast decomposition (Zech et al., 1997). Our forest study sites are located in a highly undulated landscape under comparably similar climate across the three study regions. In regard to the high rainfall erosivity of our study region (Panagos et al., 2017), soils are especially vulnerable to erosion if deforestation continues to progress. We conclude that the studied soils are sensitive to land use change especially since most SOC is stored in topsoils, which are affected directly by disturbances. However, this is the subject of ongoing work.

*Used Literature:*

Guillaume, T., Damris, M., and Kuzyakov, Y.: Losses of soil carbon by converting tropical forest to plantations: erosion and decomposition estimated by δ13C, Global Change Biol., 21, 3548-3560, 10.1111/gcb.12907, 2015.

Jackson, R. B., Lajtha, K., Crow, S. E., Hugelius, G., Kramer, M. G., and Pineiro, G.: The ecology of soil carbon: pools, vulnerabilities, and biotic and abiotic controls, Annu. Rev. Ecol., Evol. Syst., 48, 419-445, https://doi.org/10.1146/annurev-ecolsys-112414-054234, 2017.

Panagos, P., Borrelli, P., Meusburger, K., Yu, B., Klik, A., Lim, K. J., Yang, J. E., Ni, J., Miao, C., Chattopadhyay, N., Sadeghi, S. H., Hazbavi, Z., Zabihi, M., Larionov, G. A., Krasnov, S. F., Gorobets, A. V., Levi, Y., Erpul, G., Birkel, C., Hoyos, N., Naipal, V., Oliveira, P. T. S., Bonilla, C. A., Meddi, M., Nel, W.,Dashti, H. A., Boni, M., Diodato, N., van Oost, K., Nearing, M., and Ballabio, C.: Global rainfall erosivity assessment based on high-temporal resolution rainfall record, Sci. Rep., 7, 1-12, 10.1038/s41598-017-04282-8, 2017.

Zech, W., Senesi, N., Guggenberger, G., Kaiser, K., Lehmann, J., Miano, T. M., Miltner, A., and Schroth, G.: Factors controlling humification and mineralization of soil organic matter in the tropics, Geoderma, 79, 117-161, 10.1016/S0016-7061(97)00040-2, 1997.

*REVIEWER#3 COMMENT 11: "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)."*

**Our response:** Thanks for your comment. It could mean that the relationship between the target and already included explanatory variables in the regression model were not masked by soil depth to such an extent compared to the excluded variables. In general, we observe that our explanatory variables gain or retain their prediction power in the partial correlation analysis pointing to their importance in explaining the target variables at all soil depths as stated in the manuscript (L543-544). This is comparable to a study comparing mineral-related C stabilization in top- and subsoils of temperate and tropical soils (Jagadamma et al., 2014). It shows that the mineral matrix is equally reactive and relevant for C stabilization at all soil depths like in our study. Similarly, Angst et al. (2018) demonstrated that in temperate soils clay content is an important predictor for SOC stock independent of soil depth.

As discussed at the end of section "4.3 Interpreting soil controls for predicting SOC dynamics", mineral-related stabilization processes are important in both tropical and temperate soils. However they are more important in the tropics for sustaining SOC stocks since C input is less compared to the temperate zone due to higher microbial decomposition rates (Zech et al., 1997). We infer that the weathering stage of tropical soils and therefore the amount of reactive mineral surfaces is highly important as it determines the size of the mineral-related C sink. Less weathered tropical soils (due to geological or anthropogenic disturbance) will have a low C sequestration potential since stabilization partners are rare and the accumulation of unprotected C is counteracted by high decomposition rates and vice-versa.

Until now, we don´t have XRD-data available on the mineralogy of the silt fraction. In general, the silt fraction of tropical soils consist of primary minerals like quartz, plagioclase, orthoclase, mica, illite and secondary minerals like gibbsite, kaolinite and pedogenic oxides in which the specific composition depends on the parent material (Martinez and Souza, 2020; Soares et al., 2005). We agree with the reviewer that additional mineralogical data on the silt and also clay fraction would facilitate the discussion. Regarding such labour-intensive work, we would declare this task as future work.

*Used Literature:*

Angst, G., Messinger, J., Greiner, M., Häusler, W., Hertel, D., Kirfel, K., Kögel-Knabner, I., Leuschner, C., Rethemeyer, J., and Mueller, C. W.: Soil organic carbon stocks in topsoil and subsoil controlled by parent material, carbon input in the rhizosphere, and microbial-derived compounds, Soil Biol. Biochem, 122, 19–30, https://doi.org/10.1016/j.soilbio.2018.03.026, 2018.

Jagadamma, S., Mayes, M. A., Zinn, Y. L., Gísladóttir, G., and Russell, A. E.: Sorption of organic carbon compounds to the fine fraction of surface and subsurface soils, Geoderma, 213, 79–86, https://doi.org/10.1016/j.geoderma.2013.07.030, 2014.

Martinez, P., and Souza, I. F.: Genesis of pseudo-sand structure in Oxisols from Brazil – a review, Geoderma Regional, 22, https://doi.org/10.1016/j.geodrs.2020.e00292, 2020.

Soares, M. J., Alleoni, L. R. F., Vidal-Torrado, P., and Cooper, M.: Mineralogy and ion exchange properties of the particle size fractions of some Brazilian soils in tropical humid areas, Geoderma, 125, 355-367, 10.1016/j.geoderma.2004.09.008, 2005.

Zech, W., Senesi, N., Guggenberger, G., Kaiser, K., Lehmann, J., Miano, T. M., Miltner, A., and Schroth, G.: Factors controlling humification and mineralization of soil organic matter in the tropics, Geoderma, 79, 117-161, 10.1016/S0016-7061(97)00040-2, 1997.

*REVIEWER#3 COMMENT 12: "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?"*

**Our response:** Thanks for this comment. To reduce redundancy in the responses, we would kindly refer to the discussion of reviewer#3 comments 1, 7 and 8. The simplest even though hypothetical explanation for higher SOC stocks in the valley positions is the environmental restriction of microbial activity independent of geochemical soil properties or lateral material fluxes along the catenae. Changes in gravel content with soil depth and sedimentary layering combined with gleyic properties observed during soil profile description indicate that valleys are more driven by fluvial activity which overprints the geochemical effect on SOC dynamics.

*REVIEWER#3 COMMENT 13: "Lines 410-412. "Differences in Δ14C 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 Δ14C would co-variate with FOC and factors limiting microbial respiration at depth should be more important than soil fertility."*

**Our response:** Thanks for pointing this out. The authors agree with the statement that factors limiting microbial respiration with depth are also important in explaining $\Delta^{14}C$ patterns. We would expand the section in following way:

"Differences in $\Delta^{14}C$ were best explained by soil depth as a proxy for factors limiting microbial respiration, which are more pronounced in sub- than in topsoils. The presence or absence of FOC explains $\Delta^{14}C$ patterns when comparing across regions."

We hope we have addressed all concerns and look forward to hearing from you.

Best regards,

The authors