<u>Author response to referee #2</u>

We thank reviewer #2 for his/her constructive comments and suggestions to improve our manuscript. Below we have formulated a first reply to the main concerns raised by the reviewer, where we provide an overview of the main changes we intend to make in the revised version of our manuscript:

- We will add information regarding the physical characteristics of the soil under forest and grassland, where we show that both sampling locations have comparable initial conditions, providing more confidence to ascribing the observed differences in SOC and $\delta^{13}C$ to changes in vegetation.
    - The topographical characteristics (hillslope length and slope gradient) of the forested and grassland transects will be carefully compared, where additional information on the slope gradient of all transects will be added. Slope gradients are very similar for all sampled transects, where the transects under forest are slightly longer than the grassland ones. The possible influence on erosion processes will be discussed in a revised version of the manuscript.
    - Soil texture data for all soils will be discussed, where no significant differences between grassland and forested hillslopes are observed.
- An improved description of the $\delta^{13}C$ and SOC profiles for the grassland and forest soils
- Clarification of some of the statements made, where additional information or references will be added where needed.
- Inconsistencies in figure captions, references, equations and units will be resolved and verified throughout the manuscript.

Reviewer comments are indicated in *italics*, our responses in regular font.

Response to reviewer #2

GENERAL COMMENTS

I have read with interest this paper, which describes the consequences of vegetation change and erosion processes on SOC dynamic and stocks. It is an interesting research objective, and the purposes of this work would fall within the aims of this journal. In general, I think the paper is interesting and has potential. However the manuscript needs some improvements, outlined in the specific comments, but its main shortcoming is outlined below. The study is based on the comparison of toposequences under forest and grassland and the assumption that the soils under these different vegetations were identical or at least very similar before the vegetation change. However, the paper gives almost no information on these soils, either from a chemical or physical aspect. Some parameters, such as texture, have a strong link with the dynamics and stocks of organic matter. How can we be sure that the very large decreases in C stocks observed under pasture is indeed due to deforestation and the erosion it induces, if we do not know that the soils are really comparable? A presentation of the main characteristics of the soils (if only in the supplementary material) is necessary before we can put forward the hypotheses set out in the discussion. This manuscript, after the necessary improvements and corrections, would be acceptable for publication.

REPLY: We thank the reviewer for their overall positive evaluation and the detailed suggestions to improve the manuscript. To test our hypothesis whether the differences in SOC and $\delta^{13}C$ between grassland and forest profiles are linked to vegetation changes, we agree that additional information on our soil transects would be valuable. Therefore, additional information on the slope gradient of all transects will be provided in the revised manuscript. We found that slope gradients are similar for all transects, even though the lengths of grassland transects are slightly shorter than the forest ones. We will also include information on soil texture data, which are available and show no significant differences between soils under grasslands and forest. Other specific comments have been addressed point-by-point in our replies below.

SPECIFIC COMMENTS

Abstract

Lines 17-18: the time span allowed by the $\delta^{13}C$ to study the past dynamic of soil carbon ranges from years to millennia (rather than centuries)

REPLY: Thank you for this, we will modify “centuries” to millennia as suggested.

Line 20: the SOC is low, not extremely low.

REPLY: Thank you for this, “extremely low” will be changed to “low”.

Line 23: “…which show typical profiles under C3 vegetation, with a slight increase with depth.”

REPLY: Thank you for this suggestion, the sentence will be rephrased as suggested.

Line 30-31: “…suggesting a recent expansion of grass vegetation, and/or that the valleys are depositional areas from organic matter eroded from the hillslopes.”

REPLY: Thank you for this suggestion, the sentence will be rephrased as suggested.

Lines 31-33: “Our approach, based...determine changing vegetation cover”. This is true, but it has already been done in different parts of the world and published in many publications in the last 40 years. As this sentence is written, it sounds like a new approach.

REPLY: We agree that this approach has been previously applied in different parts of the word, however not yet in Madagascar. We have therefore rephrased this sentence as follows “The method we applied, which is based on the large difference in $\delta^{13}C$ values between the two major photosynthetic pathways (C3 and C4) in (sub)tropical terrestrial environments, provides a relatively straightforward approach to quantitatively determine changing vegetation cover in Madagascar.”

Introduction

Lines 87-90: “The stable carbon isotope ratio…show a different degree of isotope fractionation”. It is necessary to cite references

REPLY: The following reference will be added: Cerling and Harris (1999).

Materials and methods

Line 101: the rainfall is not very high; many tropical regions have average annual rainfall between 1500 and 3000 mm or more.

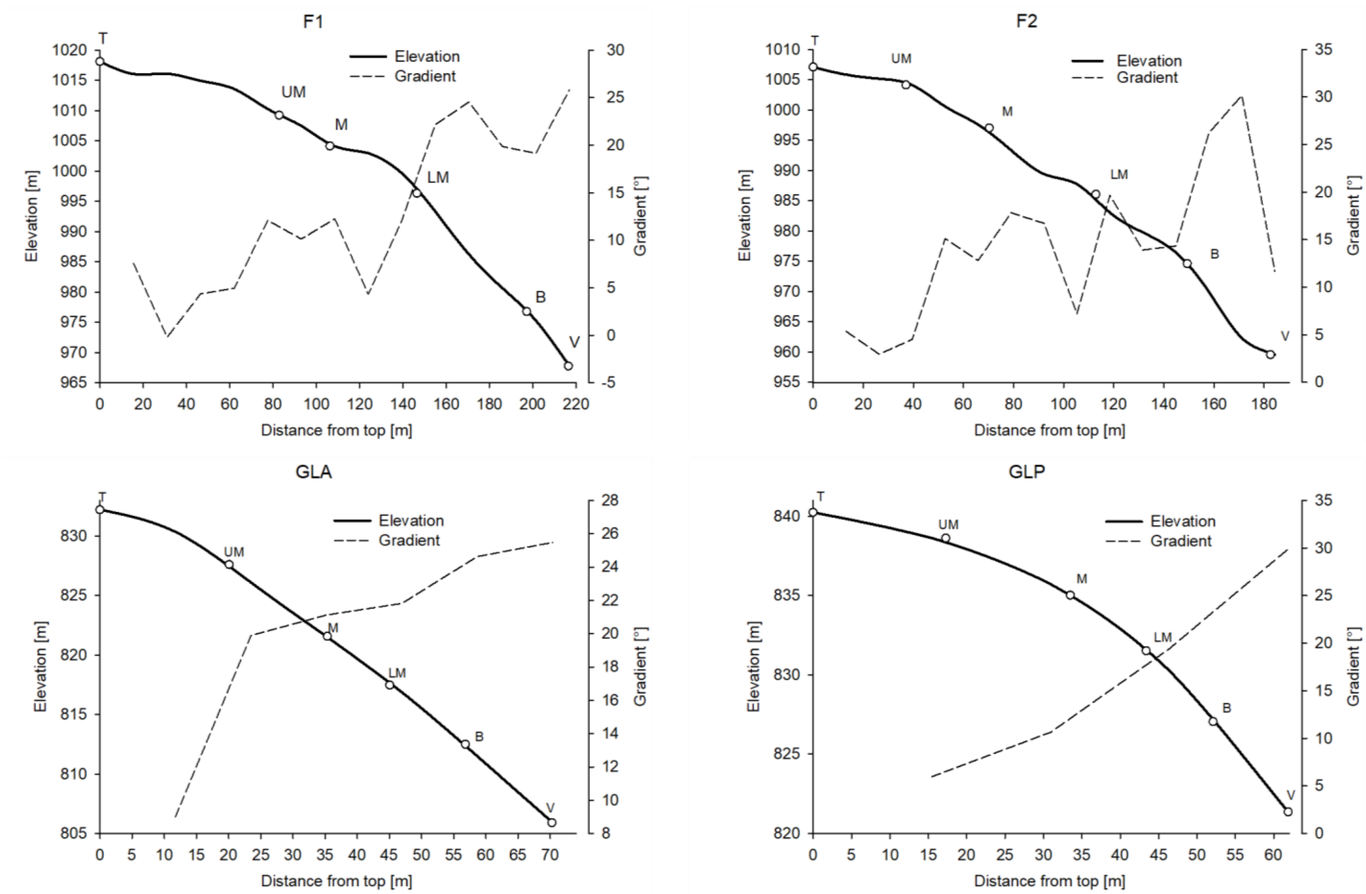
REPLY: We agree - the term “high” will be removed.

Line 103: “the mean annual temperature varies between 18 and 24°C” Really? Not the mean monthly temperature?

REPLY: We thank the reviewer to notice this, we will correct this statement accordingly: “The mean annual temperature is 20.6°C, ranging between 11°C in July and 28°C in January (Ferry, 2009)”.

Line 118-120 AND Figure S3: the length and the gradient of the hillslopes are different under forest and grassland. Could this have an effect on erosion processes?

REPLY: We agree that the sampled hillslope transects under forest are longer (217 and 184 m) than the sampled grassland profiles (62 and 70 m). However, the slope gradients (derived from the 12 m resolution TanDEM-X DEM) of the four transects are comparable, with maximum slope gradients of 30° and 25° for the forest transects and 29° and 25° under grassland. In the revised manuscript, the supplementary Figure S3 will be improved and a revised version of this Figure will include the change of the gradient along the transects, a provisional version is shown below.



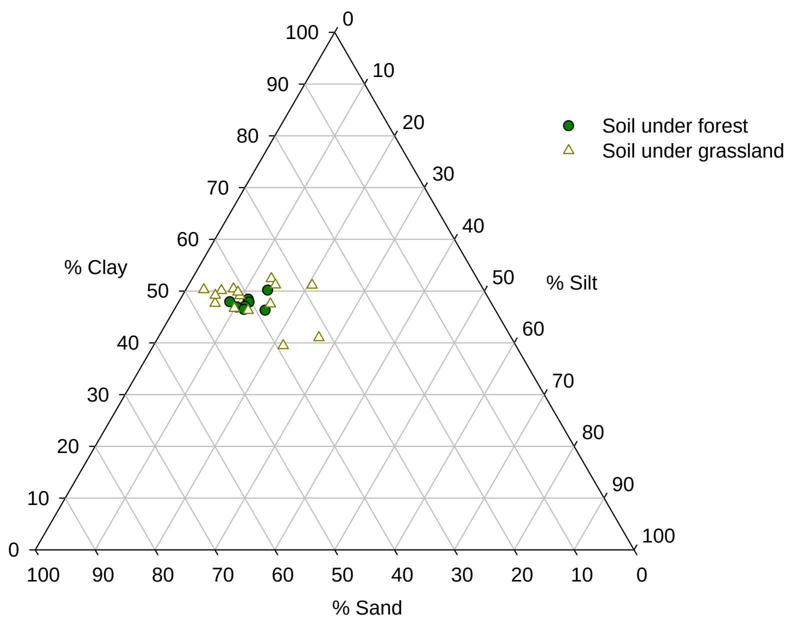
The two main types of soil erosion on hillslopes are water erosion and diffusive erosion. Water erosion rates typically increase with increasing slope length and gradient (Govers et al., 1994). Diffusive erosion fluxes are approximately proportional to the slope gradient (Heimsath et al., 2005; Pelletier and Rasmussen, 2009; Roering et al., 1999).

Based on the topographical characteristics only (i.e., assuming the same vegetation cover) of our hillslopes, we can thus expect diffusive soil erosion fluxes to be similar for all four transects which would result in lower diffusive erosion rates on the forested slopes as they are longer. Similarly, one might expect higher water erosion rates on the the lower half of the forest transects when considering topography only as these are longer than the grassland profiles. However, the effect of slope length on water erosion rates is non-existent under dense, natural vegetation (Cerdan et al., 2010, Zhao et al., 2021) and it is therefore unlikely that there would be significant differences in erosion rates between the grassland and forest slopes if they would only have a different topography. The differences in erosion rates due to differences in topography are more than likely far less important than those related to differences in vegetation cover. Water erosion rates are minimal under forest, given the protection provided by the dense vegetation cover (Cerdan et al., 2010; Zhao et al., 2021). A grass cover that is well below 100% does offer far less protection: consequently, actual water erosion rates may be expected to be significantly higher on the grassland slopes in comparison to the forest slopes (Carroll et al., 2000; Silburn et al., 2011; Zhao et al., 2021).

Line 123-126: Why is there such a large distance (about 60 km) between the soil profiles under the forest and those under the grassland? Were there no adequate situations for the grassland soils closer to the forest? Important information about the soils is missing, which could be in the supplementary material: are the soils under forest and under grassland really similar, in chemical and physical terms. One of the objectives of the paper is to assess the effect of vegetation change on carbon stocks. Several soil parameters, such as texture, can influence organic matter stocks, so it is important to know whether the soils are similar.

REPLY: We agree with the reviewer that the distance between the forest and grassland profiles is relatively large. The main rationale behind the site selection was that (i) grasslands on the western side of Lake Alaotra were the main focus, as these represent a large and continuous/homogeneous area with characteristic vegetation cover, for which we hypothesized that vegetation changes (deforestation) may have occurred long enough in the past to result in differences in SOC inventories and characteristics. The nearest zone of pristine forest is located on the eastern side of Lake Alaotra – given the wide alluvial plain that results in a fairly high distance between sites. While grasslands area are also present on the eastern side of the lake, they represent a much more narrow strip of land which may have been deforested relatively recently so that SOC inventories might still reflect the forest cover that was present until recently. However, we paid careful attention to ensure that the topography of the transects was as equivalent as possible.

We agree that the chemical and physical characteristics of our soil should be comparable in order to verify our hypothesis of a shift in vegetation. The soils at both the forested and grassland sampling site are defined as ferralsols (Andriamananjara et al., 2017). We further verified the assumption of comparable soils by analysing the texture of the soil under forest and grassland. These results will now be included, we did not observe significant differences in texture of soils under grassland and forest (p-value =0.663 (sand); p-value=0.723 (silt) and p-value= 0.232 (clay)). In the new version of the manuscript, we will add the soil texture diagram (see below) to the supplementary figures, add a paragraph describing the used method to derive the soil texture and report the results of the texture analysis in the text.



New supplementary Figure: Texture triangle (clay, silt and sand) of soil under forest and grassland.

Results

Line 207-208: The description of the C profiles is too brief and even wrong! For example, for the F1UM profile the SOC content varies from 60 to 200 cm, between 0.3 and 0.9 %, not 0.1 and 0.2 %.

REPLY: We apologise for the error. We have verified and corrected these numbers and have further elaborated the description of these results:

"The OC content (%OC) of the forest profiles ranged between 1.5 and 4.8% for F1 and between 2.9 and 5.6% for F2 in the upper 0-10 cm (Figure 2a and 2b and Table S1). Overall, the %OC trends of the profiles were similar for the different sampling locations, where the %OC was highest in the topsoil, decreasing exponentially with depth over the first ~60 cm. At 190 cm depth %OC decreased to 0.1-0.3% for both profiles. For forest transect F1, %OC content in the upper ca. 60 cm was higher at the UM and LM position compared to the other hillslope positions. These differences were particularly apparent in the upper ca ~20 cm of the profile. This difference between the hillslope positions was less marked in F2. However, the %OC was highest for the B-F2, UM-F2and LM-F2 when compared to the V-F2, T-F2 and M-F2."

Line 210-211: The description of the $\delta^{13}C$ profiles is too brief.

REPLY: The description of the $\delta^{13}C$ will be improved by adding few lines as follows:

At the surface, the $\delta^{13}C$ values only showed minor variations, between -27.1 and -25.5‰ (Figure 3a and 3b). $\delta^{13}C$ values increased with depth and reached a value of -24.1 ± 0.6‰ at a depth of 60 cm. Below this depth, $\delta^{13}C$ values no longer showed a systematic variation with depth but varied within a narrow range. No distinct trends were observed for the different sampling positions, where only the $\delta^{13}C$ of F1-T were slightly higher throughout depth when compared to the other sampling positions.

Line 218-219: It would be better to say that in the first few decimeters, these two profiles have lower SOC values than the other profiles.

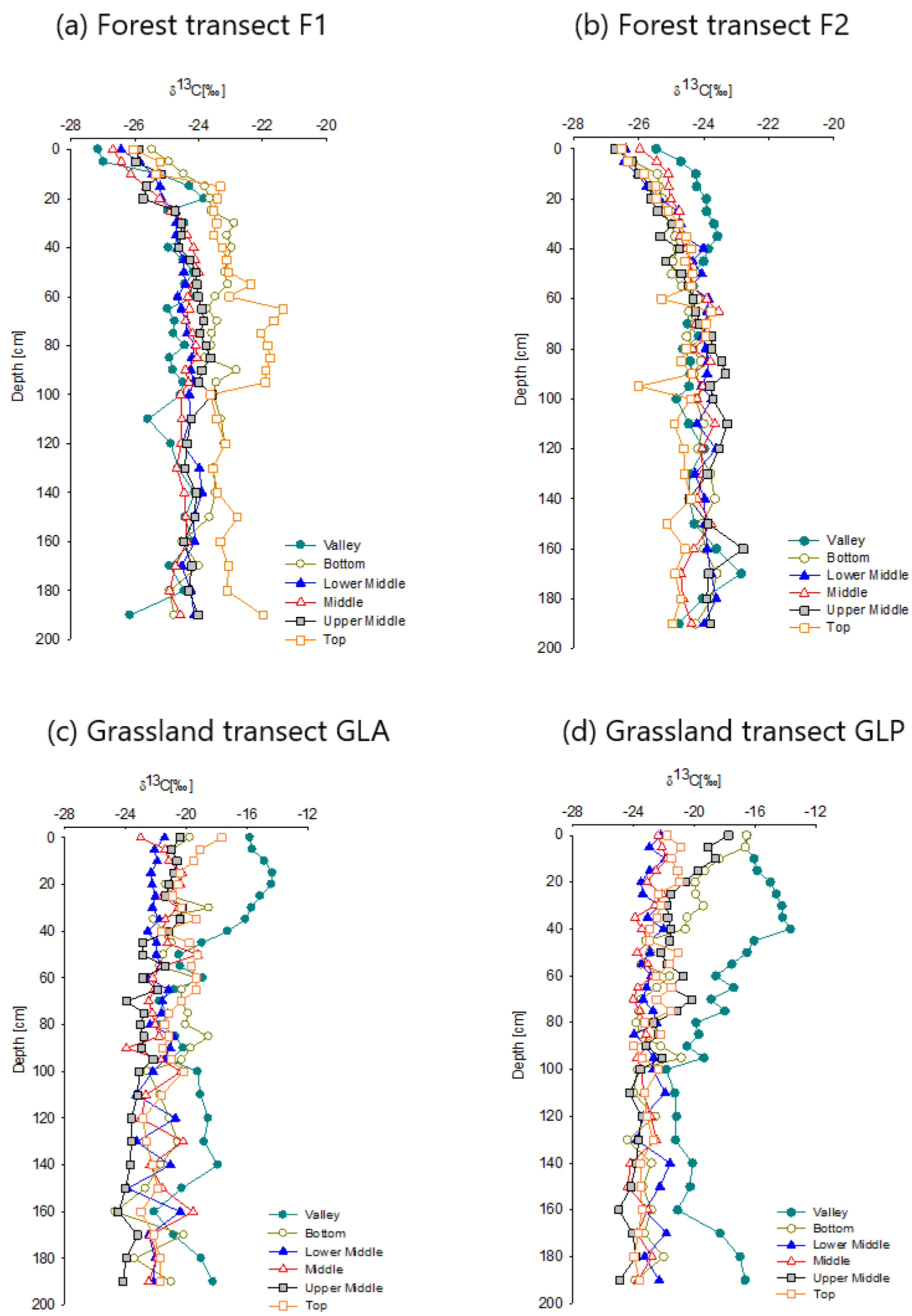
REPLY: Thank you for your suggestion, we will change it accordingly.

Line 236: The sentence "However, the cumulative…on the GLP hillslope" is unnecessary.

REPLY: We will remove this.

Figure 3c: THIS IS NOT THE GOOD ONE!

REPLY: Thank you for pointing out this error. We will add the correct sub-plot, verify the corresponding text, and check the full manuscript for correct Figure and Table references.



Discussion

Lines 253-277: All these explanations of the evolution of δ 13C values under C3 forest vegetation are excessively long. Since the end of the 80's, many articles have detailed this. This does not provide decisive information to answer the objectives of the paper.

REPLY: We had elaborated on this topic to provide the reader with the necessary background information to frame the observed decrease in $\delta^{13}C$ that we observed under forest, and to be able to properly compare this with the trends we found under grassland that are described from line 278. However, we agree this might be considered too extensive, and will reduce the length of this part by removing few sentences or summarize some information in the revised version of the manuscript.

Lines 293-295: I do not agree, in the topsoil (what depth exactly?), the C3 contribution is much lower than 70%! See the figure 6.

REPLY: This was indeed not clearly formulated, the profile interval we refer to here is the upper 0-50 cm, and we excluded the values in the valley profile. We will clarify this by changing the sentence as follows: " The contribution of C3 plant material to the SOC present in the upper 0-50 cm of these grassland soil profiles is estimated at ca. 70%, with exception of the valley position."

Lines 297: for GLP-V the δ13C value increases between the surface and 50 cm.

REPLY: We will correct his.

Lines 356-358: repetition of the lines 348-350

REPLY: Thank you for noticing this repetition. Lines 348-350 mainly point out the difference between of erosion which occurred in transects under forest vegetation and grassland vegetation, whereas lines 356-358 refer to differences in erosion between along the transects, i.e. that the erosion rates increase from the top towards the lower slopes. To clarify this, we will combine these 2 sections in the new version of the manuscript as follows: " This is confirmed by soil erosion rates derived from in situ ^{10}Be concentrations of the topsoil samples (5-15 cm) which indicates that both under grassland and forest erosion rates increases from the top towards the valley position, where the erosion rates are consistently higher under grassland when compared to forest."

Line 380: "…, while the outputs include CO2,…" or "…, while the outputs include CO2 emissions,…"?

REPLY: Thank you for your clarification. We mean here the CO_2 emission, it will be corrected as suggested.

Line 397-398: It is not true that all the studies cited found strong differences in SOC stocks between savannah and forest situations. Moreover, the stocks are not calculated and commented on.

REPLY: We apologize for the confusion due to missing references - we had intended to refer here to Rabetokotany-Rarivoson et al. (2015) and Razafindrakoto et al., (2018) who have investigated the SOC change due to land use change by following the different stages of deforestation that occurred in the humid rainforest of Madagascar. They indeed found that the SOC stocks in the soil under the final stage of deforestation (grasses) are always much lower than the SOC stock under the initial forest. We will rephrase this sentences as follow: " Rabetokotany-Rarivoson et al. (2015) and Razafindrakoto et al. (2018) found a strong difference in SOC between the initial forest vegetation and the final stage of deforestation which is characterised by non-forest vegetation (dominated by grasses)".

Lines 400-401: That is true, but what does it add to the discussion, at this point. It would be better to delete this sentence.

REPLY: This will be removed.

Line 411: "The δ13C values of the forest profiles increased with depth, which is expected for soils developed for soils developed under C3 vegetation". It would be better to say that these 13C profiles are typical of soils under C3 vegetation for a very long time.

REPLY: Thank you for the suggestion. We will rephrase this sentence as suggested.

Lines 417-418: you cannot say that organic carbon input from the new grassland vegetation is not significant: it represents almost a third of the carbon stock!

REPLY: This description might have been somewhat unfortunate - the fraction of SOC from the grass vegetation indeed represents one third of the total SOC stock. What we aimed to communicate here, is that (i) total OC stocks in the grasslands are substantially lower than in forests, and (ii) that despite the absence of substantial new inputs from C3 vegetation, the bulk of the SOC stocks is still largely dominated (70%) by (old) C3-derived carbon.

To clarify our point, we will rephrase this sentence to make this point more clear and avoid misinterpretations.

Line 429: "This indicates that the response time to deforestation depends on the rate of depletion of the old C3 pool." What does this sentence mean?

REPLY: What we referred to here is that the time since deforestation is likely to be reflected in the fraction of the C-OC pool that has been mineralized / lost. We agree that the sentence might be unclear for readers and will therefore rephrase this.

Technical corrections

Introduction Line 42: Voarintsoa et al., not Voarintsoa and Cox

REPLY: Thank you for pointing this out, this will be corrected.

Materials and methods Figure 1a: in the caption, it is written "dotted black line", but it is a "dotted white line".

REPLY: This will be corrected in the new version.

Line 121: The supplementary material S3 does not show vegetation

REPLY: Thank you for pointing out this error, the correct Figure we should have referred to is S1; this will be corrected.

Line 148: in the equation, δ13C, not δ13.

REPLY: This will be corrected.

Line 197: for D(i), the unit of measurement is missing.

REPLY: We will add units for D(i) (cm) as well as for the bulk density (g/cm^3).

Results

Figure 2: in the caption: "middle" not "middles"

REPLY: We will correct this.

Line 207: the topsoil samples are 0-5 cm not 0-10 cm

REPLY: Will be changed to "in the upper 0-10 cm".

Line 209: "…between -25.5 and - 27.1‰ …"

REPLY: We agree that number format should be one number after the decimal point and it should be -27.1 and -25.5‰ (from low to high values). We will change this in the manuscript and keep our number format consistent.

Line 225-226: verify the profiles which show gradual decline: GLP-B, GLP-UM, GLA-T (not GLA-M)

REPLY: It will be verified and changed accordingly in the revised manuscript.

Line 240: "at different depths" appears two times

REPLY: This will be corrected in the new revised manuscript.

Line 280: "…values of -20 down…" The symbol ‰ is missing.

REPLY: The symbol ‰ will be added.

Line 350: In the references, Brosens et al. is indicated as published in 2022.

REPLY: The discussed in-situ ^{10}Be data have not yet been published and are not part of the Brosens et al. (2022) paper. Therefore, we will keep this reference as non-published.

References:

Andriamananjara, A., Ranaivoson, N., Razafimbelo, T., Hewson, J., Ramifehiarivo, N., Rasolohery, A., Andrisoa, R. H., Razafindrakoto, M. A., Razafimanantsoa, M. P., Rabetokotany, N. and Razakamanarivo, R. H.: Towards a better understanding of soil organic carbon variation in Madagascar, Eur. J. Soil Sci., 68(6), 930–940, doi:10.1111/ejss.12473, 2017.

Carroll, C., Merton, L. and Burger, P.: Impact of vegetative cover and slope on runoff, erosion, and water quality for field plots on a range of soil and spoil materials on central Queensland coal mines, Soil Res., 38(2), 313, doi:10.1071/SR99052, 2000.

Cerdan, O., Govers, G., Le Bissonnais, Y., Van Oost, K., Poesen, J., Saby, N., Gobin, A., Vacca, A., Quinton, J., Auerswald, K., Klik, A., Kwaad, F. J. P. M., Raclot, D., Ionita, I., Rejman, J., Rousseva, S., Muxart, T., Roxo, M. J. and Dostal, T.: Rates and spatial variations of soil erosion in Europe: A study based on erosion plot data, Geomorphology, 122(1–2), 167–177, doi:10.1016/j.geomorph.2010.06.011, 2010.

Cerling, T. E. and Harris, J. M.: Carbon isotope fractionation between diet and bioapatite in ungulate mammals and implications for ecological and paleoecological studies, Oecologia, 120(3), 347–363, doi:10.1007/s004420050868, 1999.

Govers, G., Vandaele, K., Desmet, P., Poesen, J. and Bunte, K.: The role of tillage in soil redistribution on hillslopes, Eur. J. Soil Sci., 45(4), 469–478, doi:10.1111/j.1365-2389.1994.tb00532.x, 1994.

Heimsath, A. M., Furbish, D. J. and Dietrich, W. E.: The illusion of diffusion: Field evidence for depth-dependent sediment transport, Geology, 33(12), 949, doi:10.1130/G21868.1, 2005.

Pelletier, J. D. and Rasmussen, C.: Quantifying the climatic and tectonic controls on hillslope steepness and erosion rate, Lithosphere, 1(2), 73–80, doi:10.1130/L3.1, 2009.

Rabetokotany-Rarivoson, N., Andriamananjara, A., Razafimbelo, T., Ramifehiarivo, N., Ramboatiana, N., Razafimanantsoa, M., Razafimahatratra, H., Rabeharisoa, L., Bernoux, M., Brossard, M., Albrecht, A., Winowiecki, L., Vagen, T., Grinand, C., Vaudry, R., Rakotoarijaona, J.-R., Rahagalala, P., Rasolohery, A., Parany, L., Bürren, C., Saneho, H. J., Miasa, E. and Razakamanarivo, H.: Changes in soil organic carbon (SOC) stocks after forest conversion in humid ecoregion of Madagascar, XIV WORLD For. Congr. Durban, South Africa, 7-11 Sept. 2015, (September), 8p, 2015.

Razafindrakoto, M., Andriamananjara, A., Razafimbelo, T., Hewson, J., Andrisoa, R. H., Jones, J. P. G., van Meerveld, I., Cameron, A., Ranaivoson, N., Ramifehiarivo, N., Ramboatiana, N., Razafinarivo, R. N. G., Ramananantoandro, T., Rasolohery, A., Razafimanantsoa, M. P., Jourdan, C., Saint-André, L., Rajoelison, G. and Razakamanarivo, H.: Organic Carbon Stocks in all Pools Following Land Cover Change in the Rainforest of Madagascar, Soil Manag. Clim. Chang. Eff. Org. Carbon, Nitrogen Dyn. Greenh. Gas Emiss., (September 2018), 25–37, doi:10.1016/B978-0-12-812128-3.00003-3, 2018.

Roering, J. J., Kirchner, J. W. and Dietrich, W. E.: Evidence for nonlinear, diffusive sediment transport on hillslopes and implications for landscape morphology, Water Resour. Res., 35(3), 853–870, doi:10.1029/1998WR900090, 1999.

Silburn, D. M., Carroll, C., Ciesiolka, C. A. A., DeVoil, R. C. and Burger, P.: Hillslope runoff and erosion on duplex soils in grazing lands in semi-arid central Queensland. I. Influences of cover, slope, and soil, Soil Res., 49(2), 105–117, doi:10.1071/SR09068, 2011.

Zhao, M., Jacobs, L., Bouillon, S. and Govers, G.: Rapid soil organic carbon decomposition in river systems: effects of the aquatic microbial community and hydrodynamical disturbance, Biogeosciences, 18(4), 1511–1523, doi:10.5194/bg-18-1511-2021, 2021.