Response to Reviewers: egusphere-2025-4625

Title: Rapid soil degradation following deforestation in Eastern Africa
Authors: Author 1, Author 2, and Author 3
Date: April 22, 2026

Dear Editor, Dear Reviewers,

Many thanks for your constructive and insightful comments on our manuscript draft entitled "Rapid soil degradation following deforestation in Eastern Africa" by Summerauer et al. (egusphere-2025-4625). The comments improved the quality of the manuscript and helped us to present and discuss the obtained results in a more appropriate and clearer way.

Please find below a list of all relevant changes followed by the point-by-point responses to the two reviewers, with the original comments in black and our point-by-point responses directly below colored in blue.

We hope we addressed all comments to your satisfaction.

On behalf of all co-authors,

Laura Summerauer

List of relevant changes

General changes

- Simplified sentence structures to improve readability and flow.

Methods

- Elaborated on the presence of volcanic material and the influence of volcanic activity on the soils within the felsic region.
- Provided a more robust justification for the comparability between reference old-growth forest sites and hillslope sites.
- Performed a comprehensive re-calculation of soil organic carbon (SOC) and soil loss in the mafic region. This now includes a formal uncertainty analysis using propagated errors to ensure greater data reliability.

Results

- Replaced Figure 4 with a revised graphic that more accurately represents our methods and improves visual data interpretation.

Discussion and Conclusion

- Expanded the discussion regarding the influence of volcanic ash deposition and climatic variation.

- Included a detailed analysis of soil loss rates and the associated uncertainties identified during re-calculation.
- Formally outlined the technical limitations that rendered specific calculations unfeasible within the felsic study region.
- Revised the concluding remarks to shift from a site-specific focus to a broader need for improved soil management practices beyond *kalongo* soils.

Response to Referee #1

This is an interesting paper that explores the interactions between land use change on soils with two contrasting parent materials in Congo. There is a significant amount of work here and the team are to be congratulated on collecting a significant data set for the region. The results point to the importance of understanding the soil quality and, in particular, the availability of aluminium as the lower soil horizons are brought closer to the soil surface due to soil erosion. The focus on soil depth and its interaction with soil quality is often overlooked in erosion studies so this is good to see.

We appreciate the reviewer's time and effort reading and commenting on our manuscript. We thank the reviewer for their encouraging feedback and for highlighting the value of our dataset. We appreciate the recognition of our focus on soil depth, as the exposure of subsoil horizons via erosion is a critical yet frequently neglected factor in assessing aluminum-related constraints on soil quality. Rephrasing of mentioned sentences, graph descriptions, and methods section on the soil loss calculation made our manuscript clearer and easier to follow.

I have made a few of mostly minor comments below: Page 5 L123. Results are being introduced here. Suggest that they are placed in the results. The same data also needs to be presented for the mafic area.

We agree with the reviewer that the description of the allophanes using the Alox:Alpy ratio and pH values should be removed here. Initially, we had found it important to include it in the methods since these allophanes confirm volcanism in the region. However, we removed these results from this section (see also Reviewer 2).

P7 L155 change to 'difficulties in finding'

We thank the reviewer for the suggestion, which adds more clarity to this sentence.

Page 11 L225. The calculation of soil erosion depths and then the calculation of the land use change age are key to the paper, but the explanation here is hard to follow. Given the importance to the overall paper I suggest adding a figure and using it to help explain the concept of matching SOC contents to calculate soil depth change.

We agree with the reviewer that the description of the soil loss estimation is not clear enough. Since we already have a high number of figures in this manuscript, we focused on improving the understanding in the description and figure (see also reviewer 2).

Page 14 Figure 4 Depth in forest profile is a rather confusing term. Do you mean depth of forest profile? See my previous content about adding a diagram to help make this term clearer. Also label the axis the same as in the caption ‘equivalent depth in forest profile’. Better, consider using the term ‘ calculated soil depth change’.

We are grateful to the reviewer for pointing this out. We adopted the term ‘equivalent depth in forest profile’ and used it consistently throughout the manuscript.

Page 25 Line 501. While there is a nice bit of symmetry with the introduction here. It seems to me that the requirement for more sustainable soil management is not restricted to just kalongo soils.

The reviewer is correct. We rephrased the sentence in a more general way.

Response to Referee #2

This is an interesting study in which the authors focus on erosional processes as an explanation for soil degradation in two regions that contrast in soil mineralogy in the Albertine Rift Valley in Africa. Whereas most studies on soil degradation in tropical regions avoid steep slopes (and erosional processes), this study is different by focusing on areas that have experienced substantial erosion. This makes it both interesting but also challenging. Another interesting part of the study is that the authors compared a region where soils are formed on mafic rocks with a region where soils are dominated by felsic rocks, which affect the secondary minerals in these regions. The felsic area also experience influence from volcanic ashes in the Holocene.

We thank the reviewer for their positive feedback regarding the unique focus area of our research. We also thank them for their detailed and thorough commentary on our manuscript. We found the comments on our soil loss estimates and their implicit assumptions to be especially helpful. To avoid any misunderstanding, we re-wrote this part of the methods (see also Reviewer 1) and added calculations of uncertainties. Moreover, we discussed our assumptions and the resulting uncertainties in more detail. We are convinced that these changes substantially improved our manuscript.

Writing. While generally well written, the manuscript suffers in some places from long and winding sentences. The most extreme example is between lines 103 and 109 which all appears to be one sentence. Also, sometimes you try to put too much information in one sentence, which makes them difficult to understand. For example, I did not understand what you were trying to explain in lines 166-167. There are more examples like these and I would like to encourage the authors to critically look through their manuscript for long or complicated sentences because they make your manuscript in parts difficult to read.

We thank the reviewer for drawing our attention to these hard-to-parse sentences. We revised the manuscript, focusing on simplifying overly long sentences in particular, to ensure clarity. Additionally, we had the final text reviewed by a native English speaker to ensure grammatical accuracy.

Soil analyses. I understand from the manuscript that a substantial part of the soil analyses were conducted using soil infrared spectroscopy which is an indirect method (you write it was used to 'support' the quantification of key soil variables). I can understand why it was done, probably given a lack of reliable soil analytical equipment and laboratory capacity. Although I think this is OK for variables such as SOC and TN, I remain quite skeptical when it is used for other variables such as pH (CaCl2), ECEC, and pyrophosphate and oxalate extractable Fe and Al. My main reason is that I don't see how these variables can affect the infrared spectroscopic information from soil samples. It remains a black box and if you have unusual soil samples or outliers that are not included in your calibration samples you may work with erroneous data, so you add another level of uncertainty to the quality of your data and thus your analysis.

We thank the reviewer for raising this important point. As noted in the current text (L189-195), mid-infrared spectroscopy was employed only where calibration models yielded sufficient predictive accuracy; detailed performance metrics are provided in the supplementary material. In cases where accuracy was found to be insufficient—such as for P_{resin} —the method was not utilized, and a subset of samples was measured using standard laboratory techniques instead. So, while the reviewer is correct that some variables, such as SOC and TN, provide generally better predication accuracy than others, we are confident in our application of spectroscopy for all variables for which it was used.

Assumptions.The most important and critical part of the interpretation of the data is the comparison between forested are eroded hillslopes. This is described in 2.5 and I had to read it three times to understand what was done. I appreciate the approach that you chose, but it is also my task as a reviewer to question how solid your assumptions are. If I understand it correctly, it was assumed (1) that originally (before clearing) SOC profiles and SOC stabilization mechanisms in the cleared hillslopes were similar to the SOC of the forested hillslopes. Then, it was assumed that (2) the SOC levels of the topsoil from the cleared hillslope sites were correctly matched to the comparable levels of SOC of the forest profiles, which was somewhere below the surface of the forest profiles. Finally, (3) it was assumed that the soil above this level, which was still present in the forest profiles, had been eroded from the cleared hillslopes. That is quite a lot of assumptions and it would be really good if you had some independent parameter to test these assumptions, which I don't think you have provided.

We thank the reviewer for their detailed comments regarding the underlying assumptions of our calculations. First, we will amend the way we describe our method and assumptions according to the approach presented in the reviewer's comment to increase clarity of the method to the reader. Second, we acknowledge the uncertainty that these assumptions may entail but would like to elaborate why we are still confident in their validity and how they can support the estimates we made. In the absence of additional data, we landed on this comparative method to estimate the amount and rate of soil loss, which we acknowledge (both here and also in the text) is not ideal. Nevertheless, the resulting soil loss rates show strong alignment with erosion rates from previously published, peer-reviewed literature. We believe this agreement validates the inclusion of these calculations as a valuable contribution to the study. However, we fully agree that these estimates involve significant uncertainties. In the revised manuscript, we explicitly emphasized these limitations and clarified that these results should be interpreted as approximations (e.g., in L221-225).

Let's first look at (1). You write that the reference data from undisturbed sites were retrieved from the TropSOC database v.1.1. If we do a study in my group where sites are compared, we try to select our own reference sites. We make sure that soil types are comparable and we make sure that soil texture is comparable and that the distance between sites in relatively small.

We appreciate the opportunity to clarify the regional context of our study. Conducting fieldwork in the Eastern Democratic Republic of Congo and Western Uganda presents significant logistical and security challenges, where the lack of infrastructure and political instability often constrain sampling campaigns. The selected forest reference sites represent the closest remaining primary old-growth forests on comparable parent material within these regions and the dataset was created within the same group of researchers (last author Doetterl is also the PI of the former project "TropSOC"). Given the absence of any closer primary forest, the TropSOC sites would have been the exact locations of reference sites for our study. Thus, we believe that already published data from these remote and hardly accessible forest sites for which we have great on-sight knowledge and which lied within the exact same study region as ours, provided a suitable reference for our work. To address the reviewer's specific questions within this comment, we separated them into the following four bullet points:

- I was wondering if you can provide the minimal, maximum and mean distance between reference sites and cleared sites?

  The map (Figure 1 in the manuscript) illustrates the location and the distance between reference and cleared sites. The distances ranged between 5 and 50 km and we clarified this further in the text.

- Are they having similar parent material?

  Yes, the soils of all sites (including the reference sites) developed on the same parent material within each region. This is detailed in Doetterl et al. (2021).

  Doetterl, S., Asifiwe, R. K., Baert, G., Bamba, F., Bauters, M., Boeckx, P., Bukombe, B., Cadisch, G., Cooper, M., Cizungu, L. N., Hoyt, A., Kabaseke, C., Kalbitz, K., Kidinda, L., Maier, A., Mainka, M., Mayrock, J., Muhindo, D., Mujinya, B. B., . . . Fiener, P. (2021). Organic matter cycling along geochemical, geomorphic, and disturbance gradients in forest and cropland of the African Tropics – project TropSOC database version 1.0. Earth System Science Data, 13(8), 4133–4153. https://doi.org/10.5194/essd-13-4133-2021

- Are you comparing similar soil types, soil textures? Did you test this?

  Absolutely. We are only comparing the same type of soils with similar textures. We clarified this in the manuscript. Please see the comment below for more details on the texture matching (Assumption 2).

- In the discussion you mention for the felsic region that croplands were likely established on more fertile soils, which you use to explain why soils on cropland actually had higher SOC contents than forests. But it also put into question your approach. Can you exclude that you are also comparing different soil types in the mafic region?

  In the felsic region, we clarified that volcanic activity—specifically the deposition of ash and mudflow—has substantially influenced the topsoil characteristics. While soil types and parent materials are otherwise comparable across the region, the local distribution of this volcanic material is highly variable (e.g. due to topography, wind patterns, and precipitation) and was not known and quantifiable prior to sampling.

Furthermore, selective deforestation of more fertile land is a global phenomenon that is difficult to decouple from regional studies. We believe that the presence and absence of these beneficial volcanic inputs to soil may have driven historic deforestation patterns but are not the primary driver of modern-day deforestation. We made this potential confounding issue more explicit in the revised manuscript in order to address the reviewer's concern regarding land-use bias.

For the mafic region, we stated more clearly that all sites remained highly comparable and developed uniformly on mafic parent material, as detailed in the current methods section.

Assumption (2) is also critical, because with each centimeter deviation from a correct match you introduce more errors. Therefore, it would be really good if you had some independent variable in your soil profiles to check how good the match is between forest and cleared sites. Would it be possible to use soil texture to test your match? Another option might be the 13C values of SOC, since they typically change systematically with soil depth, it should be possible to check with such independent data whether your match between forest and cleared sites is correct. I am asking for this because later in the manuscript you argue that there is almost no replenishment of new organic matter (e.g. from the Eucalyptus monocultures), but this could also the result from wrong 'matching' of the forest and cleared profiles. An idea might be not to match the topsoil of the cleared area but focus on the subsoil of the cleared sites to match with the forest profiles.

We thank the reviewer for these insightful suggestions regarding independent validation of our site matching. We addressed these points in the revised manuscript as follows:

- Radiocarbon and SOC data: We clarified that our matching is supported independently by both radiocarbon and SOC data. The fraction modern (*Fm*) values in the mafic soils indicate that very little fresh organic matter is stabilized or incorporated into the SOC pool of degraded hillslopes compared to forest topsoils. In fact, the cleared sites fall primarily within the same subsoil range of the forest profiles (with the exception of one abandoned site; see figure below), which supports the interpretation that the cleared sites simply represent a deeper section of the same initial forest soil profile as the reference sites. We feel that this justifies our depth-matching approach.

- δ^{13}C: We agree with the reviewer that δ^{13}C data would provide valuable information for comparing/matching topsoils of cleared sites with forest reference profiles. However, δ^{13}C was not measured for these samples. We chose to prioritize radiocarbon analyses to specifically address an improved understanding of SOC replenishment.

- Soil texture matching: Texture-based matching is not feasible in this specific setting because the textures are more or less consistent after the top 30 cm. In the mafic region, forest soils maintain a consistently high clay content (70–80 %) to a depth of at least one meter (see Supplementary Figure D2 in the manuscript). Because our measured clay data for cleared hillslopes fall within this same narrow, high-clay range, texture does not provide a sufficiently sensitive gradient to differentiate or match specific soil horizons.

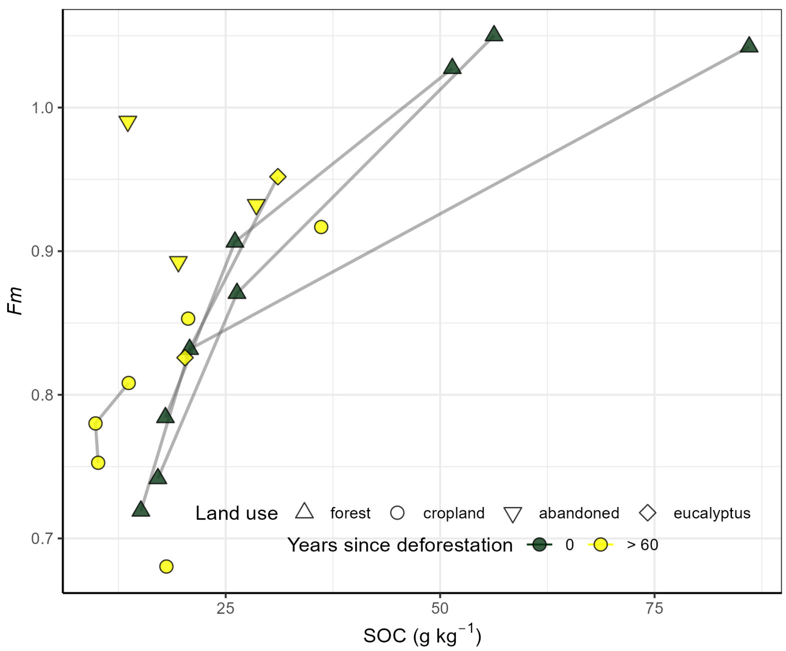

Figure 1: Relationship between fraction modern (*Fm*) and soil organic carbon (SOC) content across degraded hillslopes and reference forest sites. Data represent cropland (circles), Eucalyptus (diamonds), and abandoned cropland (upside down triangles) at sites cleared >60 years ago (yellow symbols). Forest reference profiles from Doetterl et al. (2021a, b) are shown as green triangles. Lines connect samples within the same soil core to illustrate depth-dependent trends.

Assumption (3) is also not supported by independent data. Although you mention in the text that you are careful with the interpretation of SOC losses (l 226) in the discussion you mainly focus on the erosion as major process of SOC losses. If this assumption is correct, you should be able to find some of the eroded soil material at the footslopes of these hillslopes. Did you find colluvium and are the amounts comparable to what you would expect based on your analysis in this manuscript?

We thank the reviewer for highlighting the logical downslope consequences that should be observable given our stated soil loss estimates. Indeed, we observed exactly this evidence, i.e. the presence of substantial colluvial deposits in the valleys of this region, with depths reaching up to 3 meters, in a separate study conducted in the region. These deposits clearly corroborate the extreme soil loss observed on the sampled hillslopes. Given the already extensive scope of the current dataset, we decided to treat the detailed analysis of these colluvial sediments in a separate, forthcoming manuscript. However, in the revised version of this paper, we explicitly mentioned that these valley deposits serve as independent evidence confirming the high soil loss rates reported and we referred to the published data in the other paper.

To provide answers to the long comment, we split the paragraph into three bullet points:

- I found the explanation of volcanic activity in the felsic region vague. It made me wonder how representable this region is for soils derived from felsic parent material. First, carbonate vulcanism is very rare and mostly consists of carbonates instead of silicates. I am not sure how this information fits with the occurrence of allophanes, as mentioned in the manuscript, which are silicates and not carbonates.

We apologize for the vague explanation of the volcanic activity and thank the reviewer for this helpful clarification regarding soil mineralogy. We acknowledge that allophanes are likely absent or present only in negligible quantities in the vicinity of Fort Portal, where carbonatic volcanism dominated. However, further south (e.g., towards the Kasese region), volcanic activity involving silicate-rich materials has likely led to the presence of allophanes also in our study area (Eby et al. 2009). We revises the manuscript to clarify that our previous explanation regarding allophanes was only applicable to certain parts of our study area.

We will emphasize that while the soils developed on silica-rich parent material, they have been substantially influenced by mid-holocene, carbonatic volcanism. Our study demonstrates that this volcanism has effectively 'decoupled' the current soil properties from those that developed solely from the underlying parent material. We explicitly addressed the added complexity introduced by the heterogeneous and site-specific distribution of these deposits in the revised manuscript.

Eby, G. N., Lloyd, F. E., & Woolley, A. R. (2009). Geochemistry and petrogenesis of the Fort Portal, Uganda, extrusive carbonatite. Lithos, 113(3), 785–800. https://doi.org/10.1016/j.lithos.2009.07.010

- Also, in the felsic region the sand content is much higher than in the mafic region. A higher sand content may have affected water infiltration rates which may have affected soil erosion. Could that be one of the explanations why you were not able to quantify erosion in the felsic region?

We agree with the reviewer that the sandy texture is a potentially important driver of erosion. Also, volcanic deposits are likely to affect erosion rates as well. However, as field-based measurements have shown, erosion is still comparable in the felsic as in the mafic region (see discussion/literature cited in L442-446). Regarding the estimation of erosion rates in the felsic region, we stated more clearly the reasons why soil loss calculation was not performed. Unlike in the mafic region, we were unable to identify a sufficient number of sites along the deforestation gradient and can only refer to published estimates from other studies. Given that our estimation approach relies on specific assumptions (detailed above), we determined that it would not yield reliable results in a system as complex and heterogeneous as our sampled felsic region. We clarified this point in the revised manuscript.

- Climates are also quite different, but not included in any of the explanations of the observed differences between the two regions.

We agree with the reviewer that the distinct climatic conditions between the two regions are important factors. The felsic region experiences lower annual precipitation (-23%) and a higher mean annual temperature (+16%) compared to the mafic region. In the revised manuscript, we explicitly stated that these climatic variations could potentially influence soil properties and erosion rates. Further, there is no indication that the differences that we see consistently along the degradation gradient are overprinted by the climatic variability within the region, likely because the climatic variability within each gradient is small.

Overall this study is very interesting because it clearly goes beyond typical studies on land use change effect of soil carbon stocks. However, the weakness is in the many assumptions that are currently not checked independently. Maybe the solution is to conduct an uncertainty analysis and error propagation of your calculations, because you are introducing quite a lot of additional errors by your methodology of soil analyses and your assumptions. For guidance see: https://seismo.berkeley.edu/ kirchner/Toolkits/Toolkit$_0 5.pdf$

We thank the reviewer for their constructive feedback and for providing additional resources. As noted in our previous responses, we made uncertainties clearer in the results and discussion sections of the revised manuscript. Specifically, we calculated and reported the mean and propageted standard error of the means for the SOC content, SOC stock and depth estimates, the combination of which will best convey the uncertainty of our estimate method (see figure below).

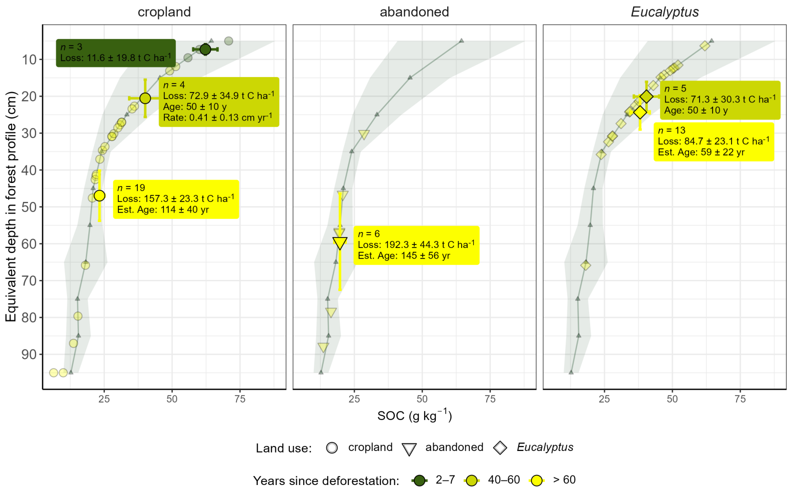

Figure 2: Mean Soil Organic Carbon (SOC) content plotted against the equivalent depth in forest profile for cleared hillslopes (cropland, abandoned, and *Eucalyptus*) along the deforestation chronosequence (2–7, 40–60, >60 years since deforestation) in the *mafic* region. The shaded green ribbon and associated green line represent the old-growth forest reference (Mean±SD, *n*=9). Vertical and horizontal error bars represent the propagated standard error (SE) for SOC content and equivalent depth in forest profile, respectively. Annotated boxes provide the calculated SOC stock loss ($\mathrm{t\,C\,ha^{-1}} \pm$SE). The cropland sites with 40–60 years since deforestation (50 ± 10 years) were used to derive the annual loss rate ($\mathrm{cm\,yr^{-1}}$). This rate was subsequently applied to estimate the conversion age (yr±SE) of the hillslopes cleared >60 years ago. Note: the category with 10–20 years since deforestation was excluded due to the low sample number (*n*=1).

There were a few other minor issues, but I have already spent a considerable amount of time on this review and hope you won't mind me leaving it at that. I hope you find my review helpful, and that it will enable you to improve your manuscript.

We thank the reviewer for their time and hope that the revised manuscript will meet their standard for publication.