SOIL Discuss.,
https://doi.org/10.5194/soil-2020-95-RC2, 2021

Interactive comment on "Assessing soil erosion of forest and cropland sites in wet tropical Africa using $^{239+240}$Pu fallout radionuclides" *by* Florian Wilken et al.

Anonymous Referee #2

Received and published: 16 January 2021

First, I want to congratulate the authors with the very interesting and thorough study. The paper discusses the urgent problem of soil erosion which seriously impacts the region and there is still a huge lacuna of understanding on its dynamics. The topic is very suitable for the journal and the technique is novel in its geographical context. In that specific context, there is one major issue with the technique which needs further addressing in the study. The paper itself is written clearly and besides some minor issues is very suitable for publication in the journal.

Main comments

- The main issue with the technique applied to tropical Africa is related to the afore

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Discussion paper

mentioned high potential rates of soil erosion, which can cut deep into the saprolite through gully and badland formation. If soil is eroded from deeper saprolites (low activity) upslope and deposited downslope on surface soils, it lowers the activity of the plow layer because of sedimentation and not because of erosion. If I understand the model correctly, the main assumption is that lower activities will indicate erosion and higher rates sedimentation. How did you correct for the potential lowering of activities through sedimentation of erosion subsurface soils? I read in line 310 of the discussion that you use higher activities in the subsoil as a proxy for sedimentation, but there is not much mention of how this is incorporated in the mass balance model and estimations of soil redistribution. I think this effect (and the assumptions of the model) should be explained more clearly in the methodology. Moreover, these assumptions should also be included in the discussion as a limitation of the technique in the specific geographic context.

- Related to this comment is the potential effect of terracing. Were there terraces in the study fields? If so, I expect it would greatly influence the results of the model since you would get erosion and sedimentation patterns on a very small scale that might not be picked up by the sampling resolution.

- Why do you either use a mean forest references or a mean cropland plateau as reference. Wouldn't it be more relevant to pick a reference of a forested plateau soil closest to the cropland area?

Minor comments

Abstract: - Lines 18-19: After reading the introduction I know what you mean with the following statement 'challenging local conditions for long-term landscape scale monitoring'. However, when first reading the abstract I did not, so it might be beneficial to clarify this statement.

- Line 26: The most vulnerable regions of what? Soil erosion or socio-economically?

Introduction: - In my opinion, it would be beneficial to add a brief statement on the effects of soil erosion on sediment yields (see the review of Vanmaercke et al. 2014 about sediment yields in sub-saharan Africa) in the introduction. In that context, I'm also missing a mention of the climatic drivers of the high rates of soil erosion in the region (such as high intensity rainfall, intra- and interannual variations) in the introduction (you mention it in the method section).

- The discussion of the issues of population pressure, soil erosion and food security is portrayed quite linear (e.g. more people is more deforestation hence more soil erosion). There is evidence from Eastern Africa that the reality is not so linear (see: Tiffen 1994- More People, Less Erosion, or Wynants et al. 2019- Drivers of increased soil erosion in East Africa's agro-pastoral landscapes).

- Line 39: Social impact should be social impacts (plural).

Methods:

- Line 100: is there terracing or other soil conservation measures? Were the terraces slow-formed through natural processes or were they built? These deserve a mention since they are very influential for the redistribution of soil. Especially terracing can completely alter the 240Pu profile in a very short period. Figure 1 shows large scale vegetation patterns, that are not always relevant on the plot scale. In this context, it seems beneficial to the study to add some photos of the study areas (could also be as supplementary information).

- I would expand section 2.5. At the moment, you don't explain why you test these different scenarios and their statistical relevance.

Results:

- Do you have figures of soil redistribution for the forest sites?

Discussion:

- Could there be an effect of vegetation cover on the original fallout, wherein dense forest cover could influence the activities of fallout radionuclides in the soil? What was the vegetation cover situation during the time of peak fallout? These questions deserve some discussion.

- Line 353: when referencing values observed globally it is important to reference empirical studies and not studies where soil erosion values are modelled (such as the Borelli et al. 2017 study).

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