Point-by-point response Referee #2

Assessing soil redistribution at forest and cropland sites in wet tropical Africa using $^{239+240}Pu$ fallout radionuclides

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We appreciate the reviewer sees the relevance of the study and supports publication. The reviewer calls for methodological clarification and discussion regarding the burial of eroded and already depleted sediments and the use of both cropland plateau and forest sites as reference. We thank the reviewer for the valuable advices and revised the manuscript accordingly. Please see our detailed answers (in italics) to the comments below:

Major reviewer comments

The main issue with the technique applied to tropical Africa is related to the aforementioned 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.

We agree that source area depletion is a limitation of the mass balance model for soil redistribution assessments in areas facing high soil loss rates. A way to understand the potential effect of $^{239+240}Pu$ source depletion and corresponding sedimentation is to reduce the increment depth and thereby increase the number of samples to achieve more depth explicit $^{239+240}Pu$ information. This information would be interesting, but increases the sample amount and associated costs substantially. With the sampling design applied in this study, which is based on two *layers from 0-60 cm and 60-100 cm at the cropland foot-slope sites, a very high topsoil depletion would be indicated by lower topsoil* $^{239+240}$*Pu activities compared to the deeper soil layer, which was not observed. Hence, our approach takes this process to some extent into account, but not on high resolution due to limitations in the amount of samples that were able to be analysed. The aim of the study is to understand the potential application of fallout radio nuclides in tropical Africa. Therefore, we also sampled organic layers to understand* $^{239+240}$*Pu plant uptake and depth increments at plateau sites to understand a rather undisturbed depth distribution. For a follow-up study in tropical Africa, we would not analyse the organic layers in the forest but take an additional plough layer sample increment at foot-slope locations into account. In the revised version of the manuscript, we discuss the limitations of the mass-balance model and include a section for a suggested sampling design:*

"The measured 239+240Pu activity at the foot-slope positions may underestimate the 239+240Pu inventory due to two potential processes: (i) Sedimentation exceeds the sampling depths that does not include the full inventory and (ii) deposition of sediments that are already depleted in 239+240Pu as the inventory at the eroded source area has been subject to pronounced soil and corresponding 239+240Pu loss. However, an indication for both processes would be an increasing 239+240Pu activity with depth, which is not reflected in the data (Fig. 3)."

"For future 239+240Pu based soil redistribution investigations in tropical cropland areas a two layered sampling scheme is suggested that individually samples the plough layer and the deeper soil layer (e.g. 0-20 cm and 20-60 cm at slope and an additional 60-100 cm sample at foot slope sites). The two layered sampling scheme accounts for the large number of samples falling below the detection limit and allows for a better understanding of the 239+240Pu depth distribution and thereby depletion status of the sediment source area. In tropical forests, the sampling scheme can be focused on mineral soil as the activity in the litter and organic layer does not substantially contribute to the 239+240Pu inventory. Sedimentation at the three forest study sites did not exceed 60 cm at the foot slope, which suggests that a sampling depth of 0-60 cm is sufficient to include the full inventory. Nevertheless, additional deeper soil sampling, with reduced increment depth (e.g. 20 cm) at foot slope locations, is suggested to account for different environmental conditions and potentially higher foot slope sedimentation."

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.

We fully agree that terraces lead to very complex erosion and deposition processes that would be reflected in the $^{239+240}$*Pu inventories. However, the study sites do not show any terracing, which is in the DR Congo and Ugandan* *parts not common. This is not true for Rwanda, which where terracing is common for cropland slopes. To make this clear, we point at non-terracing in the study area description as follows:*

"The cropland sites represent the typical smallholder farming found in the region, which is based on small non-terraced fields with non-mechanised tillage practices."

Why do you either use a mean forest reference 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?

We found a large discrepancy between the plateau sites of forests and croplands. In theory, both positions should be rather similar, as they have not been subject to substantial soil loss or sedimentation by tillage and water and received more or less the same fallout. Solely using the plateau sites would ignore potential losses of $^{239+240}Pu$ due to cropland use (e.g. harvest erosion of root crops), which might cause an altered reference. As we cannot name the correct reference, considering both as potential references is important to demonstrate uncertainties to the reader. To clarify this, we add the following information in the discussions.

"In this study, two different reference scenarios are taken into account to address potential differences of $^{239+240}Pu$ inventories between stable forest and cropland plateau positions: (i) Ref_{for} mean of specific forest sites (ii) and Ref_{plt} mean of cropland plateau positions of the specific sites."

Minor comment:

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.

Thanks! We modify the wording by adding "infrastructure limitations":

"However, there is limited knowledge on soil redistribution dynamics following land conversion into arable land in tropical Africa that is partly caused by infrastructure limitations for long-term landscape scale monitoring."

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

Thank you for this hint. We change the wording as follows:

"[…] in one of the most socio-economically and ecologically vulnerable regions of the world."

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.

To point out that soil erosion does not only lead to soil redistribution but to huge amounts of sediment delivery (impressively shown in the high sediment load of rivers), we explicitly name sediment delivery as an important process affecting the region as follows:

"In particular, the White Nile-Congo rift (NiCo) region faces a strong impact of soil redistribution (Lewis and Nyamulinda, 1996; FAO and ITPS, 2015; Montanarella et al., 2016) and corresponding sediment delivery (Vanmaercke et al., 2014) due to steep terrain, high rainfall erosivity with a strong intra-annual seasonality (Fick and Hijmans, 2017) that causes sparse vegetation cover of the soil at the end of the dry seasons but also throughout the cultivation period (Lewis and Nyamulinda, 1996) due to non-mechanised farming."

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).

We follow the suggestion and add statements on the environmental drivers in the Introduction:

"In particular, the White Nile-Congo rift (NiCo) region faces a strong impact of soil erosion (Lewis and Nyamulinda, 1996; FAO and ITPS, 2015; Montanarella et al., 2016) due to steep terrain, high rainfall erosivity with a strong intra-annual seasonality (Fick and Hijmans, 2017) that causes sparse vegetation cover of the soil at the end of the dry seasons but also throughout the cultivation period (Lewis and Nyamulinda, 1996) due to non-mechanised farming."

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).

We agree that population pressure causes complex processes that do not lead to a linear increase of soil erosion. Our main intention is to point out that an increasing food demand is typically compensated by deforestation, which is the prerequisite for substantial soil erosion. After land use conversion, sustainable and unsustainable agricultural use might be applied, which is –indeed- not linear. We add "and often unsustainable use of soil *resources" to the sentence to clarify that unsustainable use of soil resources is 'often' practiced but not necessarily the case:*

"[…] the onset of potential soil erosion at previously undisturbed sites (Nyssen et al., 2004) and often unsustainable use of soil resources (Wynants et al., 2019)."

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

Thanks, done!

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.

We agree, but there is no terracing in both cropland study sites. To clarify this, we add this information to the test-site description:

"[…] based on small non-terraced fields with non-mechanised tillage practices."

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).

We add orthophotos and a panorama to the supplements to illustrate the landscape.

Figure S1

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

Thanks, we add explanation and the reasons behind the different scenarios in section 2.5 as follows:

"The mass balance soil mixing model was used to assess different scenario assumptions and their sensitivity. First, different 239+240Pu reference inventories were determined in two ways: (i) the mean 239+240Pu inventory of all forest sites of a specific region (Reffor; i.e. mean inventory of Kahuzi Biega forest for the DR Congo cropland sites and Kibale forest for the Ugandan cropland sites) and (ii) the mean 239+240Pu inventory of the cropland plateau sites of the specific region (Refplt). These land use specific reference scenarios are supposed to address potential differences between Reffor and Refplt to understand uncertainties associated with the reference determination. Second, the sensitivity of the ploughing and corresponding mixing depth is assessed using a 20±5 cm ploughing

depth deviation as the exact plough depth over the integration period cannot be accurately derived. Third, to address potential interannual variability of water erosion, a scenario with five extreme years producing the same total soil erosion as a 55 years continuous soil erosion rate was compared against the results of the first scenario."

Results:

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

As our results show that no substantial soil erosion is taking place at the forest sites, we are not showing a forest soil redistribution Figure. We add a sentence in the Discussion to make it more clear that soil redistribution at the forest sites does not exceed the effect of natural variability:

"The variation of forest 239+240Pu inventories due to bioturbation and fallout infiltration patterns (e.g. caused by throughfall or stem flow patterns) exceeds a potential soil redistribution impact […]. Hence, the forest sites are assumed to represent almost the entire 239+240Pu inventory of the baseline inventory without substantial loss due to soil erosion."

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.

The cropland and cropland reference site in DR Congo was arable land at the time of nuclear weapon tests, while the study site in Uganda was converted into cropland after the nuclear weapon tests (approx. 1970s). The effect of forest cover on the mean 239+240Pu inventory is expected to be low. However, as higher fallout heterogeneities are typically found in forests (stem flow, canopy through fall patterns etc.; see Discussions section "Soil redistribution in cropland of the NiCo region"), spatial $^{239+240}$Pu variability in the Ugandan study site might be higher. However, these initial patterns are likely to be homogenised to a certain extent due to lateral soil mixing by ploughing. We expand the discussion on the different land use conditions in the cropland test sites as follows:

"The different land use during the main time of atmospheric nuclear weapon tests (DR Congo=cropland; Uganda=forest) might have caused higher spatial variability of the initial 239+240Pu inventory associated with complex infiltration (e.g. stem flow) and troughfall patterns in forests (Hofhansl et al., 2012). However, these small-scale variation is likely to be homogenised after 40 years of lateral soil movement by plough-based arable use."

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).

Thanks for pointing at this. We change the text as follows:

"In comparison to values observed (Boardman and Poesen, 2006) and modelled globally (Borrelli et al., 2017) […]"