Feedback from the reviewers is written in italic, while our responses are written in in green. When changes were made to the manuscript, we included a screenshot of this part using track changes.

**Reviewer 1**

*This paper presents a very interesting experiment set in Kenya to explore the impact of agricultural nutrient management on stabilized SOC in a robust experimental design. The introduction strongly stresses the lack of studies on the effects of agricultural management practices on SOC stocks in sub-Saharan Africa.*

*The experiment consists in 4 sites of maize monocropping (1 clayey in central Kenya, 1 sandy in central Kenya, 1 clayey in western Kenya and 1 sandy in western Kenya), with 4 treatments per site (control, control+N, Tithonia diversifolia amendment and farmyard manure amendment), 3 replicates per treatment in each site, and 3 sampling depths, leading to a total of 144 samples. The authors studied SOC, N, δ13C, and Δ14C, and used a size- and density-fractionation protocol to separate the POM and MAOM fractions.*

*The main result is that, unfortunately, long-term, continuous application of OM does not seem to lead to an increase in SOC stocks, neither in topsoil nor subsoil, although it helps slowing down the SOC loss. While the findings themselves form a new and important piece of knowledge, they also highlight the potentially large gap between the results obtained in temperate zones and the field reality in sub-Saharan Africa, stressing the need for more regional studies in the tropical lands. Overall, a very nice and interesting paper!*

We thank the reviewer for taking the time to read our manuscript and for providing detailed and constructive feedback. This is greatly appreciated. Please find our responses to the feedback below.

*Here are some thoughts:*

A summary illustration of the experiment (visually combining Table 1 and Figure S1, and showing the 4 treatments/3 replicates) could be useful. That being said, the experiment is well-described.

Thanks for this suggestion. We believe that the layout of the experiment (4 different treatments at 4 sites, at which 3 depths were samples) is clear from the description in the methods, and from the layout of Fig. 1 in the manuscript, while their location is shown in Fig. S1. Combining all this information in a single figure seems like an overload of information to us. Therefore, we respectfully opted to not include an additional figure to show this layout.

*L180: out of curiosity, do you see an explanation for the different values of SPT needed for good separation?*

Assuming that POM has a similar density across the sites, the optimal density of SPT to separate POM from MAOM depends on the density of the minerals. Below the optimal density, not all POM is floating in SPT, which is detected by a higher OC content of the MAOM compared to the optimal density. Above the optimal density, minerals start to float in the SPT, which is detected by a lower OC content of the POM (Cerli et al., 2012; doi.org/10.1016/j.geoderma.2011.10.009). The lower optimal SPT density of 1.6 g $cm^{-3}$ at Aludeka therefore indicates that the density of minerals at that site is slightly lower than those at the other sites where the optimal SPT density is 1.7 g $cm^{-3}$. We note that the optimal SPT density for soils collected in Mediterranean to temperate regions was 1.6 g $cm^{-3}$ according to Cerli et al. (2012). Using the same methods, our result of an optimal SPT density of 1.7 g $cm^{-3}$ thus shows the importance of testing the optimal density for soils with different mineralogies, such as tropical soils, to ensure an optimal cut-off between POM and MAOM. It is also worth noting that Cerli et al. (2012) did not test an SPT density of 1.7 g $cm^{-3}$ (the next higher density was 1.8 g $cm^{-3}$), so it's not possible to assess whether a density of 1.7 g $cm^{-3}$ would also have been appropriate for their soils.

In the manuscript, we now make it clear that we tested densities in steps of 0.1 g $cm^{-3}$, as this information was lacking: "To determine the optimal density of the SPT, a range of densities from 1.4 to 1.8 g $cm^{-3}$, in 0.1 g $cm^{-3}$ increments, was used on a subset of samples from all four sites, following the methods outlined by Cerli et al. (2012).".

in a Falcon tube. To determine the optimal density of the SPT, a range of densities ~~between~~ from 1.4 ~~and~~ to 1.8 g $cm^{-3}$, in 0.1 g $cm^{-3}$ increments, was used on a subset of samples from all four sites~~(Cerli et al., 2012)~~, following the methods outlined by Cerli et al. (2012). The optimal density of SPT was determined as the one leading to the highest OC content of the light fraction

*L189: how did you make sure to retrieve all your MAOM during the rinsing of the floculant, since you added floculant because you couldn't retrieve all the floating MAOM?*

Thank you for bringing this to our attention. Based on your question, it seems we did not clearly formulate this part of the procedure. We did not intend to rinse out the flocculant. Rather, because the addition of the flocculant affects the electrical conductivity (EC) values, this measure could not be used to determine when sufficient SPT was rinsed out of the samples, as EC values remained high due to the presence of the flocculant. Therefore, using additional samples to which no flocculant was added (which resulted in substantial losses of minerals, so these samples were not used for further analyses), we determined that four rinses were sufficient to reach EC values below 50 µS $cm^{-1}$.

To avoid any confusion, we now reformulated this in the manuscript to: “As EC values are increased by the presence of the flocculant, preventing to reach values below 50 µS $cm^{-1}$, all samples to which flocculant was added were rinsed four times. This number of rinses was shown to be sufficient for additionally processed samples from all sites, to which no flocculant was added. Rinsing those samples four times resulted in EC values below 50 µS $cm^{-1}$.”.

second washing step onwards. ~~Samples to~~ As EC values are increased by the presence of the flocculant, preventing to reach
205 values below 50 $\mu$S $cm^{-1}$, all samples to which flocculant was added were rinsed four times~~, as the EC values were increased by the flocculant, preventing to reach~~. This number of rinses was shown to be sufficient for additionally processed samples from all sites, to which no flocculant was added. Rinsing those samples four times resulted in EC values below 50 $\mu$S $cm^{-1}$.

*L196: how was the subsampling done, by hand or with an automatic subsampler?*

The subsample was collected by hand. We added this to the manuscript as follows: “For the latter, visible pieces of POM were avoided during the manual subsampling for fractionation, which may at least partly explain the lower OC recovery compared to soil mass recovery.”.

the sample, compared to the non-ground subsample used for fractionation. For the latter, visible pieces of POM were avoided during the manual subsampling for fractionation, which may at least partly explain the lower OC recovery compared to soil mass recovery. The most likely pathway for OC loss during fractionation was during the rinsing of SPT from MAOM, as not

*L197: any other plausible pathway for loss?*

The other potential loss pathway is the loss of both POM and MAOM that stuck to the sonicator tube. As this was minimal, and because we didn’t have a reliable way of distributing lost C between POM and MAOM, we assumed that the main loss was through losses of MAOM during rinsing the SPT, as this loss pathway was clearly observed.

*L257: would you say the C:N data (fig.S9) support the idea of ‘MAOM-contaminated’ POM in sandy soils?*

Yes, the relatively low C:N values of POM in some of the treatments at the sandy sites are in line with the lower OC concentrations of POM for these treatments, compared to the clayey sites. Even though we tested for the optimal SPT density, this suggest that the POM fraction was nonetheless contaminated with minerals.

This is now included in the manuscript in section 3.2 as follows: “The C:N ratios of POM were consistently higher than those of MAOM, indicating a separation between the two fractions, although with a varying degree between sites (Fig. S9).” And: “The latter suggests that some MAOC might have been present in the POM fractions at the sandy sites, which is supported by the lower C:N ratios of POM in certain treatments at the sandy sites (Fig. S9).”.

275 The C:N ratios of POM were consistently higher than those of MAOM, indicating a ~~clear~~ separation between the two fractions, although with a varying degree between sites (Fig. S9). While the C:N ratios of replicates of MAOM for the same treatment

varied more widely, ranging from ca. 10 to 40 % (Fig. S11). The latter suggests that some MAOC might have been present in
280 the POM fractions at the sandy sites~~.~~, which is supported by the lower C:N ratios of POM in certain treatments at the sandy sites (Fig. S9). In contrast, the OC concentrations of the MAOM fractions were more consistent for replicates of the same

*L304: you specified the site of Machanga was subjected to strong erosion; is the erosion homogeneous on the whole site (all treatments equally affected)? Did you quantify it?*

The field trial at Machanga has only a very minor slope (estimated to be < 1 %), along which erosion took place (which was initially not expected). The erosion rate has, unfortunately, not been quantified. As there were no visual indications of preferential erosion at certain parts of the field, and the treatments were distributed across the field, there is no reason to assume that certain treatments were affected more by soil erosion than others.

Based on this comment and a comment by the other reviewer, we now describe this in more detail in the methods section where the sites are described (section 2.1): “The sandy site at Machanga had a gentle slope (< 1 %) and therefore experienced topsoil erosion throughout the experiment. Because treatments were randomized within horizontal blocks following the contour of the slope—and erosion affected the field broadly—it is unlikely that any treatment was disproportionately impacted. However, the erosion may have removed part of the topsoil and brought subsoil closer to the surface, causing some of the original subsoil (i.e., below 15 cm depth) to be included in the 0–15 cm samples.”.

~~it noted that the~~ The sandy site at Machanga ~~experienced intense~~ had a gentle slope (< 1 %) and therefore experienced topsoil



erosion throughout the experiment. Because treatments were randomized within horizontal blocks following the contour of the slope—and erosion affected the field broadly—it is unlikely that any treatment was disproportionately impacted. However, the
125 erosion may have removed part of the topsoil and brought subsoil closer to the surface, causing some of the original subsoil (i.e., below 15 cm depth) to be included in the 0–15 cm samples.

*L331: if I get it right, the portion of TD-derived MAOC can be quite high. However, you stated above that the MAOC stocks weren't much affected by the treatments. Would this mean that the turnover time of this fraction is (at least for part of it) rather short? It is interesting that these portions of "younger" MAOC are higher in the sites with quite high MAOC Δ14C ages.*

Indeed, the TD-derived MAOC, as a portion of total MAOC was higher at the sandy sites compared to the clayey sites. However, because of the much lower MAOC stocks at the sandy sites, the total amount of stabilized TD-derived MAOC was not consistently higher at the sandy sites.

The input rate of *Tithonia* residues at all sites was equal. Given that turnover time of MAOC = MAOC stock / input rate, and MAOC stocks were substantially smaller at the sandy sites compared to the clayey sites, this suggests that the MAOC turnover rate was indeed faster at the sandy sites. However, as we did not actually calculate the turnover times, we prefer not to mention this in the manuscript.

*L367: wasn't your 3rd hypothesis: 'continuous organic matter addition at the soil surface has no long-term effect on MAOC accumulation in soil layers below 15 cm depth'? I don't see how it is rejected.*

Thanks for noticing this, that is correct (as also pointed out by the other reviewer). This is now corrected in the manuscript as follows: "This is limited to results for the top 15 cm, as no significant differences in subsoil (15--50 cm) SOC stocks (Laub et al., 2023) and MAOC (this study) were detected, thereby confirming our third hypothesis.".

dynamics at these field trials. This is limited to results for the top 15 cm, as no significant differences in subsoil (15–50 cm) SOC stocks (Laub et al., 2023a) and MAOC (this study) were detected, thereby ~~rejecting~~ confirming our third hypothesis.