RC1:

General comments

This manuscript addresses the question whether organic resource quality, quantity, mineral fertilizer or site properties are most important in counteracting SOC loss under continuous maize cropping in central and western Kenya. The topic is relevant to a larger community of readers because it shows that the application of organic and mineral fertilizers cannot completely counteract SOC losses across sites of different soil properties. Based on repeated measurements over time (2002 to 2021) using a split plot design, the authors build mixed linear models to show that the reduction of SOC concentration during 19 years ranged from 42 % to 13 % in spite of adding organic and mineral fertilizer. The authors conclude that a complete halt of SOC loss is not possible even with applications of 4 t C ha-1 yr-1. However, on the landscape-scale only rates of 1.2 t C ha⁻¹ yr⁻¹ are realistic without risking losses of SOC and soil fertility at other locations. This shows that on deeply weathered soils more site-specific measurements are needed beyond the application of organic and mineral fertilizer to maintain SOC. In particular, due to the lack of existing long-term studies on the behavior of tropical soils, there would be an added value to the study. In general, the manuscript is well written and the data supports the conclusions. There are some aspects in the methods section, which needs to be addressed to enhance clarity. In addition, there are parts in the discussion section that are not necessarily needed and detracting from the storyline of the manuscript. Please see my comments on that below. If these concerns can be addressed, this would be a suitable paper for EGUsphere.

Again, we want to express our sincere thanks for taking the time to give us this detailed and constructive feedback. We agree that the points are all valid and we are confident that addressing them has improved the manuscript considerably. We have put special focus on the main points raised 1) clarifying definitions and methodology (CSE and statistical model) 2) Improving figure design and captions 3) shortening and streamlining the results and discussion, and removing parts that detract from the main message of the manuscript (pH, C-sequestration discussion, to much CSE discussion), or putting them in the annex/supplement 4) moving ill-placed parts to the methods (Fig. 7, potential SOC stocks pitfall Embu). A detailed response to each individual comment and what we have changed as a result in the review phase of the article is attached below (our comments in blue).

Below are the edits and comments on the manuscript

Abstract

Well written and very interesting discussion especially in line 23 – 25.

The study sites have been management for at least 16 years but there was a recent conversion from permanent vegetation to agriculture. Does this mean the permanent vegetation was already managed before the land conversion?

Yes, but the exact duration is not clear. We clarified this statement.

N fertilizer or no fertilizer was the split-plot treatment. At all four sites, a loss of SOC , rather than gain, was predominantly observeddue to a recent conversion from permanent vegetation to agriculture. The average reduction of SOC concentration,

5 likely because the sites had been converted to cropland only a few decades before the start of the experiments. Across sites,

How representative is Maize cropping for the study region?

We added a note on this in the first sentence.

прыаль

In sub-Saharan Africa, maize is one of the most important staple crops, but long-term maize cropping with low external

Introduction

Overall well-structured and well written.

Line 43: Stabilization capability of SOC or what? I suppose you are talking about the reactivity of mineral surfaces towards sorption of organic matter in this context. You need to be clear here and elsewhere in the manuscript.

Yes, thanks. We did so.

to be a result of initially high SOC levels at the sites, favorable conditions for SOC decomposition, and a reduced stabilization capability of the 1:1 kaolinite clay minerals in tropical soils for SOC, due to low surface reactivity (Six et al., 2002; Sommer

Line 46 – 48: I would leave this part out since the focus of this paper is the interaction between fertilizer application and site properties and not the impact of land use history.

We agree.

adaptation to different soil conditions is needed. Land eover history also explains site specific SOC dynamics and can help to define the local potential for SOC sequestration. Indeed soils cultivated as pasture or forest have initially high SOC stocks that are difficult to maintain under cultivation (Lal, 2004).

Line 72: What do you mean with SOC dynamics in this context?

We clarified this.

As the main long-term goal of ISFM is to increase short-and short- and long-term soil fertility, in particular through increasing SOC, there is a need to better understand the effect of extent to which the rate and quality of organic resource additions on SOC dynamics influence the rate of SOC change under different pedo-climatic conditions. This can help to an-

Line 76 – 77: This belongs to the method section.

Agreed, we changed the sentences in a way that is more in line with an introduction.

and mineral N fertilizer affects SOC dynamics. Therefore To shed light on these questions, we analyzed data from four long-

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term experiments conducted at four different sites in central and western Kenya(established in 2002 and 2005, respectively). All four experiments had the same treatments with organic resource additions of the same quality, with exactly the same organic and mineral resource additions at each site. The aims were to study objectives of the study were (i) to quantify the

Line 88 – 89: Why would the resource quality be of more importance than site conditions? What do you exactly mean with site conditions? Does it refer to soil mineralogy, climate, or both?

We reformulated this.

 The efficiency with which new SOC is formed is influenced by both the site-pedo-climatic conditions and the organic resource quality, but the resource quality plays the strongest role. Hence, we expect a significant interaction of resource quality with site in the efficiency to store new SOC.

Material and methods

A general question. How did you account for the changing soil bulk density (due to land conversion and land management) over time when calculation SOC stocks?

We did not have enough data to do such a correction. Hence, our CSE calculations are only an approximation, which we now clarified in the methods.

Line 103 – 107: Are the soil classifications based on lab data from the reference soil profiles?

Yes, from the time of establishment. We added this info.

Line 115 – 121: I suggest providing a figure here visualizing your plot and sampling design. This would also help to understand the structure of the random fixed models better. A table showing the sampling dates for the different study sites and what was sampled (topsoil, subsoil, BD etc.) would be of added value.

Thanks for this suggestion. We added these to the supplement.



Figure A1. Correlation Example of temporal trends the split plot design of SOC and TNthe long-term trials: Aludeka. Displayed are Red areas indicate the site specific least square means for bothbare fallow plot.

Sampling dates	Sites sampled	Properties samples	Depth
2002	Embu, Machanga	SOC, BD	<u>0-15 cm</u>
2004	Embu, Machanga	SOC, BD	<u>0-15 cm</u>
2005	Embu, Machanga	SOC, BD	<u>0-15 cm</u>
2005	Aludeka, Sidada	SOC	<u>0-15 cm</u>
2006	Embu, Machanga	SOC	<u>0-15 cm</u>
2008	Embu, Machanga	SOC	<u>0-15 cm</u>
2012	Embu	SOC	<u>0-15 cm</u>
2013	Embu, Machanga	SOC	<u>0-15 cm</u>
2015	Embu, Machanga	SOC	<u>0-15 cm</u>
2018	Embu, Machanga, Aludeka, Sidada	SOC	<u>0-15 cm</u>
2019	Embu, Machanga, Aludeka, Sidada	SOC	<u>0-15 cm</u>
2021	Embu, Machanga, Aludeka, Sidada	SOC, BD	0-15-30-50 cm

Table A1. Overview of the soil data that were available for this study.

Did the bare and control plot received N, P and K fertilizer?

Yes, we clarified this.

roots (and root exudates). In addition, a randomly allocated quarter of each split plot was kept as bare fallow throughout the entire duration of the experiments, receiving the exact same inputs but with no maize planted and with all emerging weeds removed by regular weeding. This was done to study the SOC dynamics without any additional inputs from roots or

Line 146: How exactly was the soil moisture content measured?

We added this information. Please see below.

Line 145: Was the soil bulk density measured before or after the 8 mm sieving?

We agree that we had not described this part well, and it is clarified now.

storage until further analysis in the laboratory. Samples were then For further analysis, a sub-sample was broken and crushed
 by pestle and mortar and sieved through a 2 mm sieve. Prior to Stone contents of both sieving steps were recorded. The soil moisture content of air-dried samples was based on drying a sub-sample at 105°C for 24h and subsequently calculating the dry soil weight per known core volume. Bulk density was determined based on the calculated absolute dry weight of stone-free samples and and the stone-corrected volume of soil cores, assuming a density of stones of 2.65 g cm⁻³. At all sites but Aludeka (with 1, 5 and 12% of stones in 0-15, 15-30 and 30-50 cm depth, respectively), stone content was very low (below 1% on

175 average). Prior to C and N analysis, samples were finely ground with a ball mill, then soil C and N concentrations contents

Line 150: Was the soil pH measured on the 2 mm fraction or on powdered samples?

On the 2mm fraction. This info was added.

were measured by dry combustion using an elemental analyzer (CHN628, LECO Corporation, Michigan, USA). In addition, soil pH (H₂O) was determined on the unmilled 0-15 cm topsoil samples (2-mm sieved) taken in 2018. Because soil pH values

Line 152: Did you also tested the subsoil samples from the latest sampling campaign for carbonates? Did you tested with HCl because subsoil samples were not analyzed for soil pH? Please correct me if I´m wrong.

We did not test for carbonates but know from earlier samplings that pH < 6.5 until at least 60 cm depth and therefore the presence of carbonates is impossible. We added this info.

contents. From additional deeper soil sampling down to 60 cm depth carried out in 2018 (data not shown here), it is known that
the soil pH was lower than 6.5 in all depths considered.

Line 177 – 180: Could you please explain this step one more time for me?

We agree that this was a bit hard to understand have not detailed the individual effects. We hope that it is now more clear.

experiment (as initial measurements were not available for all plots). The initial fixed effects in the model were interactions random-site model were 1) time since the experiment started, 2) the interaction of time since the experiment started with the organic resource treatment, the 3) the interaction of time since the experiment started with the mineral N treatment and the

210 interaction of organic resource treatment with mineral N treatment() the three-way interaction of time since the experiment started with organic resource and mineral N treatments. In the site specific model, all these-fixed-site model, the fixed effects were further allowed to be site specific, by adding an interaction with site for each of them. In this model, site was the only fixed effect that was also allowed to have an intercept of its own and not only an interaction with time. 1) the interaction

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of site with time since the experiment started, 2) the three-way interaction of site with time since the experiment started and with the organic resource treatment, 3) the three-way interaction of site with time since the experiment started and with the mineral N treatment and 4) the four-way interaction of site with time since the experiment started, organic resource and mineral N treatments. Moreover, the fixed-site model, had a fixed site effect without interactions. This assured that different

Line 186: Can you explain me what site-specific variance means in this statistical context?

We allowed a site-specific residual variance. This has been specified.

225 sampling in different years. Additionally, visual inspection of model residuals revealed variance heterogeneity between sites. Consequently, a site dependent residual variance was allowed in the models. After selecting an appropriate random effects

Line 202 – 205: It is hard to follow how you calculated the carbon storage efficiency. Could you please explain it to me again? I think this is an important part, which needs to be clear and easy to follow.

We agree that this important part was not clearly enough explained. We rewrote this section to more clearly describe how we transformed trends in SOC (%) to SOC stock estimates, how we fit the linear regression of SOC stock trends to annual C additions and how we interpreted the slopes of these regressions as CSE.

2.4.2 Estimation of carbon storage efficiency

From the temporal trends of SOC eoncentration content, we further estimated the change of SOC stocks in 0-15 em depth, with

- 240 the goal to derive the derived an estimate of the apparent carbon storage efficiency (CSE_a) of the different organic resources in the 0-15 cm soil layer, i.e., a measure of efficiency to retain C. The CSE_a has been defined as the fraction of C inputs contributing to C storage in the soil (Manzoni et al., 2018), e.g., in our case how much the annually added C through organic resources is found in the soil changed the trend of SOC stocks compared to the control treatment. To do so, we multiplied first the least square means of the change in annual SOC concentration content obtained by site and treatment from the mixed
- 245 model, with the mean-fixed-site model had to be transformed to mean annual change in SOC stocks. The mean BD of each site estimated from topsoil measurements to obtain the mean annual change in SOC stocks was used for this (treatment-specific differences in BD were absent). These BD estimations were also derived using a mixed linear model, from the available BD

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measurements which had been conducted in the experimental year 1, 2 and in the calendar year 2021 at each of the sites. Then,
a site- and organic resource-specific linear regression between the estimated. This model did not contain any temporal trend;
different methods of BD measurements in initial years and in 2021 were used (core method vs direct measurement from the soil augers), so any trend would have likely been an artefact. While assuming this constant BD for SOC stock calculations is only a rough approximation and not fully consistent with the equivalent soil mass approach, we nonetheless considered that it was a valuable approach to quantify CSE_a. Another potential limitation of these calculations considering only the 0-15 cm soil

layer (soil layer in which organic resources were incorporated), is that CSE_a may be underestimated if significant portions of
 carbon inputs are stabilized in deeper soil layers from e.g.leached dissolved organic carbon.
 In the final step of CSE_a estimations, a linear regression was fit, with the calculated mean annual change in SOC stocks for

0-15 cm as the response variable and the amount of annual organic resource C applied was fit, using a site-specific intercept. C input as the independent variable (i.e., 0, 1.2 and 4 t C ha⁻¹ yr⁻¹). These regressions were site- and organic resource-specific, so that estimates of CSE_a per site and organic resources could be compared:

(1)

$260 \quad dSOC = Site + C_{in} : OR + C_{in} : OR : Site$

Here, *dSOC* is the mean annual change in SOC stocks in 0-15 cm (t C ha⁻¹ yr⁻¹). Site is the site specific intercept, C_{in}, the amount of annual C input (t C ha⁻¹ yr⁻¹) and OR the type of organic resources. Note that ":" represents interactions and that there was no OR-specific intercept. The intercept was set to site specific, i.e., not allowed to vary between different organic resources at the same site (i.e., the SOC change at 0 t C ha⁻¹ yr⁻¹ did not vary between treatments). The slope of this regression was taken on the numerical variable C_{in} represented the yearly change in SOC stocks (in 0-15 cm) per t C ha⁻¹ yr⁻¹ of organic resources added. It was thus interpreted as an estimate for CSE_a of the different organic resources at the different sites (Manzoni et al., 2018) and we tested whether significant differences existed in the CSE_a between the between slopes for different organic resources at the and at different sites (i.e., testing for a significant effect of organic resource treatment, site and their interaction). Estimated least-square means of the slope were converted into percent from t C t C⁻¹ by multiplying them by 100.

Results

Figure 1: For me this is the key figure of the manuscript thus I have some questions for clarification. Is this the data normalized to the initial SOC content? Are the bars showing the data from all years/ sampling campaigns? What is the sample size for each bar? I would add that information in the caption.

Thanks for pointing this out. We added all this requested information to the caption.

Figure 1. Annual changes of SOC concentrations-contents in the top 0-15 cm soil layer in different organic resource treatments across sites. Annual The annual change in SOC concentrations is contents represent the slope of the regression against experimental time from the random-site model, using all available data (i.e., 5 (Sidada, Aludeka) or 14 (Embu, Machanga) repeated measurements from 3 replicates at 4 sites, comprising both mineral N treatments due to absence of mineral N effect). They are displayed as percentage of initial SOC (intercept of the model set to 100%). Treatments that share with the same capital letters-letter do not differ significantly from each other in the annual change of SOC (all p < 0.05). The error bars indicate the 95% confidence interval-intervals for the annual change of SOC. *Abbreviations: CT, control; SD, sawdust; MS, maize stover; TD, Tithonia; CC, Calliandra; FYM, farmyard manure.*

Figure 2: What is the reason that some data points are above the initial SOC content at 0 years after establishment?

That they are normalized mean initial SOC content per site and that especially Machanga had low SOC and large variability. We added this information.

Figure 2. General trend in the of SOC concentrations contents in the top 15 cm soil layer under different organic resource treatments across sites. The change in SOC concentrations contents is shown as a percentage of initial SOC concentrations contents (mean by site). Same lowercase letters indicate the absence of a significant difference in the temporal development between the different organic resource treatments, while the mineral N additions did not have a significant effect (all p < 0.05). Large initial fluctuations in and values above 100% at the Machanga site are a combined result of measurement variability and low SOC contents. The grey shaded areas constrained by the dashed lines indicate the 95% confidence intervals for true mean of SOC at different times.

Figure 2: The caption says something about dashed lines indicating the confidence intervals but they are not shown in the figure.

This is an oversight from our side. It is meant to be the grey areas in this graph only – we changed it accordingly (see above).

Figure 3: The legend showing the N application looks weird.

This is a property of ggplot and using the geom_ribbon feature. We did not find a way to remove this, and since it is still readable, we kept it as is.

Figure 3: There are some site without any data for 5 and 10 years after establishment. How does this affect the robustness of the temporal trend comparison between treatments?

Using a mixed model with site-dependent residual variances assures that the model captures lower data density in the comparison of least square means.

Figure 4: A better solution is needed for the color scheme differentiating between organic and N applications. It is hard to see the difference between the color brightness.

We agree and have changed the figure accordingly (using dashed bars instead).



Line 265 – 269: This should go in the method section.

We agree and have moved this part.

Changes in SOC stocks based on equivalent soil masses are the most reliable way to assess carbon sequestration or losses, but in contrast to the SOC concentrations of our study, were only available for 2021. Hence, apart from the one time point SOC stock assessment (Fig. 5), we tested whether the A highly significant correlation emerged between losses in SOC content (0-15 cm) and the observed SOC stocks in 2021SOC stocks for the top 2500 t ha⁻¹ equivalent soil mass were in alignment with the observed temporal trends of the SOC concentration in the 0 - 15 cm soil layer, i.e., whether soils with the highest losses in SOC concentration had the lowest SOC stocks in 2021. A highly significant correlation emerged between these two types of SOC

145 assessment in the planted plots, explaining 81% of total variation for at the clayey sites, Embu and Sidada, and about 56% for

Figure 5: The ANOVA letters and error bars are hard to read and do overlap. This needs to be fixed. In addition, the ANOVA letters are hard to read against the dark grey color of the bars.

Thanks for this hint. We have removed the overlap between error bars and ANOVA letters. We also moved the letters for the 0 – 2500 kt below the bars to make them better readable.



Figure 5. Estimated least square means for the SOC stocks across sites in the cumulative equivalent soil masses of 0 to 2500 and 2500 to 7500 t soil ha⁻¹ (bottom and top bar, respectively). As there was no significant interaction between the cropping status (being bare or planted) and the organic resource and N treatments, the results are displayed as the estimated least square means for different organic resource and N treatments for planted plots (left plot) and as mean SOC stocks from all organic resource and N treatments in the bare compared to planted plots (right plot). Error bars display the 95% confidence intervals. Same lowercase letters indicate the absence of a significant difference in SOC stocks from between treatments at the same site and soil mass layer which share no lowercase letter are significantly different from ench other (above bars, 2500 to 7500 t ha⁻¹; below bars, 0 to 2500 t ha⁻¹; all p < 0.05). Same capital uppercase letters indicate the absence of a significant difference of a significant difference for the whole 0 to 7500 t ha⁻¹ equivalent soil mass layer at the same site (p < 0.05).

Figure 5: Use the same wording for describing the meaning of the ANOVA letters as in all other figures in the caption for consistency.

Thanks for this hint, we harmonized the caption accordingly (see above).

Line 2581 – 281: This is already interpretation and should go in the according section.

We removed the part of the sentence.

to topsoil trends of SOC concentration, suggesting relatively small interactions between top and subsoil SOC dynamics. In fact, a content, A significant association between the temporal trends of topsoil SOC concentrations contents and subsoil SOC

Figure 6: Why do the error bars now represent the upper half of the 95 % confidence intervals? In all other figures, it presented the 95 % confidence interval.

This was an oversight from our side. In fact, we changed this already in the submitted version, but forgot to change it in the text.

Section 3.5: Why is soil pH a target variable now? The whole manuscript deals about the effect of organic fertilizer application on SOC concentration and stocks. Is this

important for the story? Otherwise, delete it together with figure 6 for streamlining the result section.

While we think that pH is also an aspect of soil fertility, we agree that it detracts from the story, and can also be corrected much easier than SOC. Hence, we reconsidered showing pH, and moved it to the Supplement.

Discussion

For me the discussion is a mixed bag. It contains solid explanations and frames the data in a broad context but is also contains sections which are detracting from the overall good work of this manuscript. I think it can be improved by shortening those parts to emphasize on the key messages. Please see my comments below.

Line 317 – 318: Could you please provide some information about the topography of the study sites? Since there are signs of extremely strong soil erosion in some sites, I'd expect either a slope gradient or measurements against soil erosion in other sites. Is the eroded material deposited somewhere else in the study sites? This should be very clear how you account for soil redistribution processes within your study sites.

We added the slopes to the Material and Methods section. Soil erosion mainly occurred at the Machanga site, which was surprising given the very gentle slope. As mentioned, we unfortunately do not have any measurements of erosion, and thus we could also not directly account for it. Erosion was also never the aim of this study and rather an unwanted side occurrence. We still wanted to include this observational knowledge inform readers that the rates of SOC loss are likely a combination of actual turnover of SOC and are only partly due to soil erosion.

-in Machanga(FAO, 1998; IUSS Working Group, 2014). All sites have almost flat land surfaces with gentle slopes in all sites except Embu (Machanga, 2.5%; Sidada, 2%; Aludeka, 1%; Embu 5%). However, the Embu site has been terraced to reach a

125 flat surface of the plots as at the other sites. The land-use history prior to the establishment of the experiments differed between

Line 323 – 324: I would not describe the results of the other studies. Instead, just reference to them.

Ok, we shortened this accordingly.

The generally observed SOC losses in our study corroborate the results of Sommer et al. (2018), who reported similar SOC
 415 losses at two sites in western Kenya, both close to Sidada. Another recently published study from a long-term trial of compost application of about 3 t C ha⁻¹ a year in Ivory coast, could also not maintain initial SOC levels (Cardinael et al., 2022). recent studies under similar conditions (Sommer et al., 2018; Cardinael et al., 2022). It seems thus, that maintaining SOC in arable

L331 – 332: Does this mean that C stabilization against microbial decomposition was more effective before land conversion, which would explain the initial high SOC stocks?

Yes, we added this.

in Machanga. Furthermore, a-loss of SOC usually occurs when natural vegetation is converted to arable land (Sanderman et al.,
 2017) and about 50% of initial carbon-C is usually lost (Guo and Gifford, 2002; Lal, 2018), suggesting a better stabilization of SOC under natural vegetation. In the tropics, the SOC loss due to land use change is usually more severe than that SOC

Line 341 – 354: This part does not really fit into the storyline of the manuscript. The key message of this manuscript is that application of organic and mineral fertilizer even in high quantities cannot maintain SOC in tropical soil systems. But here you are discussing CO_2 emissions and yields, which is not part of your study or covered by any data.

Reconsidering this section, we agree that it could be removed to streamline the manuscript and we did so.

Line 362 – 368: Very good point.

Thanks for pointing this out, helping to streamline the MS.

Line 373: I'm curious, does the quality of farmland manure change depending on the animals and their food?

Yes, we added a sentence on that.

2017)and pH (Mucheru-Muna et al., 2014). One important consideration here is that manure quality depends on the animal species and that higher quality feed generally results in higher quality manures (Sileshi et al., 2017). Our study also confirmed

Line 378: What is the meaning of these classes?

This is explained in the next comment.

Line 378 – 382: Is this section important for your key message? Otherwise, I would cut it.

We agree that it is not a main outcome, so we removed it to shorten the MS.

not consistently across sites (e.g., not in-at Embu). The postulated advantage of class 2 organic resources, such as *Calliandra*, relative to class 1 organic resources, such as *Tithonia* (Kunlanit et al., 2014), was, however, not supported by our results. Kunlanit et al. (2014) hypothesized that a higher SOC formation of class 2 organic resources was related to the polyphenol and lignin content, increasing synchronisation between microbial demand and availability by preventing leaching of nutrients and

⁴⁸⁵ dissolved organic carbon. A potential reason for similar performance of *Calliandra* and *Tithonia* could thus be, that they only differed in terms of polyphenol contents but not in lignin.

Line 389 – 390: I do not understand this sentence. Can you please explain it once more? What is the regulatory effect in this context?

After reconsideration we removed this sentence because it was only speculation.

formation. In the first case, lignin but not polyphenols would be responsible for the regulatory effect, as lignin and polyphenols 495 combined were similar in farmyard manure and *Calliandra*. Xiao et al. (2021) found that an the increased microbial carbon

Section 4.3: All other section titles are stating the key take away. I would do the same here for consistency and stating how the effect of mineral N fertilizer (on what?) looks like.

We agree and changed the section title.

4.3 Effect of No coherent SOC response to mineral N fertilizer

Line 408 – 412: I would cut this section. It is also more on the speculative site.

We agree and removed it.

N fertilizer may affect the microbial carbon use efficiency. For example, Silva-Sánchez et al. (2019) found reduced microbial carbon efficiency in treatments of mineral N addition. This reduced efficiency may be related to root-exudate related priming (Kuzyakov et al., 2000), which could have been higher in the +N treatments. Also, while the treatments in our study received

520 basal P and K, it cannot be ruled out that this unresponsiveness to mineral N addition in the control was due to a lack of other essential nutrients (see e.g. Mtangadura et al., 2017). In this case, higher N availability might lead to higher priming as plants of higher biomass try to obtain other missing nutrients. use efficiency in forest soils that received mineral N fertilizer.

Line 414 – 423: For me, this is the most important message of the manuscript, which opens a very interesting discussion and framing it into a regional context.

Thanks for pointing this out. We agree and this is also why we have mentioned what realistic rates may be in the conclusion.

Line 434: Rephrase the beginning of the sentence ("In the light of the results of our study (...).").

This sentence was completely removed in the shortening of the article.

In the light of the results of our study, suitable measures should thus increase the input quantity and quality at the same time. For example, intercropping can produce more biomass than sole crops on the same surface due to complementary use of

Line 441 – 447: Why is this not mentioned in the method section before? Now it comes as a surprise that there were technical problems with sampling subsoils. Is the data quality of the subsoil SOC stocks good enough to draw conclusions?

We agree that this part needed to be moved to the Materials and we did so. We do mention the issues for Embu as a potential explanation for strange results for the bare plots there. As there was no difference between treatments in subsoil SOC for Embu anyway, we do not think that it affected our conclusions other than potentially not being able to capture some differences.

and 1000 mm length (Giddings Machine Company, Windsor, USA) in all at the other sites. This The reason we had to use a
 different and partially open soil auger in Embu was that the other auger type repeatedly broke due to the very hard subsoil.
 Sampling full cores allowed for bulk density (BD) estimations by weighing fresh samples, determining the soil moisture content

Figure 7: You are introducing here new methods and results in the discussion section. In addition, it is difficult to differentiate between the colors. Is this figure necessary for your key message?

We agree, and we have added accordingly parts to Methods and Results. We also added numbers to the figure to differentiate between experiments. We think it is necessary to display our idea that carbon formation pathways may be more important than pathways of losses.

2.5 First-order decay model for comparison of SOC loss with other experiments

Finally, we compared the losses of SOC in the control treatments to losses reported in other experiments. A simple modelling approach was used assuming a yearly 1st order kinetic SOC loss:

280 $SOC_{loss} = SOC_{initial} * (1 - k * t)^{yr} - SOC_{initial}$

Here, SOC_{lass} and $SOC_{initial}$ correspond to the initial SOC contents and the SOC loss at the end of the experimental period (g kg⁻¹), k, is the annual loss of SOC under a base temperature of 10°C (g g⁻¹ SOC), and t, a site-specific rate modifier, based on site mean annual temperature (MAT) and a Q_{10} of 2:

(2)

(3)

$$t = 2^{(\frac{MAT-10}{10})}$$

285 The annual turnover of SOC (*k*), was manually calibrated, and the Nash Sutcliffe modelling efficiency was calculated for assessing the goodness of model fit, as follows:

$$EF = \frac{\sum_{z=1}^{n} (O_z - \bar{O})^2 - \sum_{z=1}^{n} (O_z - P_z)^2}{\sum_{z=1}^{n} (O_z - \bar{O})^2}$$
(4)

Here, *EF* is Nash-Sutcliffe modelling efficiency, O_z is the measured SOC loss of the *z*-th site at experiment end, \bar{O}_y the mean loss of SOC in all experiments and P_z the simulated value corresponding to O_z .

Section 4.5: Is the CSE_a important for your key message? I would shorten the section especially from line 462 – 478 since you are not presenting detailed data on soil geochemical properties besides texture. This discussion part is rather vague.

We think that CSE is important because it determines how much SOC will be stored in the end. We agree that the mentioned lines could be shortened and did so.

texture (Mainka et al., 2022). For example, mafic parent material has shown a higher potential to store SOC in aggregates than felsic material (Reichenbach et al., 2021). As a consequence, it is unlikely that rainfall and temperature combined with texture
 are sufficient to explain differences in CSE_a, as put forward by some authors (Schimel et al., 1994; Smith and Waring, 2019)
 Even a temperature dependent CSE_a (Frey et al., 2013) would not suffice to explain the higher CSE_a observed in Aludeka compared to Machanga (*Calliandra*, *Tithonia*) or in Sidada compared to Embu (farmyard manure): Sidada has higher temperatures than Embu and in Aludeka and Machanga temperatures are similar. Clearly, a better understanding of the interactions of play, a

role here (Zech et al., 1997; Reichenbach et al., 2021; Keller et al., 2022; Mainka et al., 2022) and all soils except Aludeka can

600 be considered as effectively C saturated under the current maize monocropping (Castellano et al., 2015). Overall, there is a lack

Conclusion

Overall well written.

Thanks

Appendix

In my opinion, figure A1 is not necessary since you are already stating the high correlation between SOC and TN in the text. I would just state the correlation coefficient and the significance level in the text.

We removed it and did as you suggested.

Minor comments

Line 345 – 347: Split the sentences into two.

We have removed this part, as you suggested above.

Figure A2: The y-axis of the middle panel has the wrong label. It show be 0 - 7500 kt ha⁻¹.

Thanks. In fact, it was the wrong figure in the middle panel even. This has been corrected.

Line 770: Check formatting of the reference.

Thanks, has been adjusted.

RC2:

We want to thank the reviewer for this valuable feedback. We have addressed all points raised by reviewer 2 and it improved the manuscript considerably. We removed the part about C sequestration, and improved the description and discussion of CSE. We also focused the discussion on the points that are supported by the results, removing speculative parts. A detailed response to each individual comment and what we have changed as a result in the review phase of the article is attached below (our comments in blue).

General comments

The paper is well-written and focuses on the decline of soil fertility as expressed in SOC contents and the role of SOC in C sequestration. However, the latter appears mainly from the discussion. In reading the introduction I was not quite sure why you also refer to SOC stocks and C sequestration (e.g. in lines 47-48). If you want address both soil fertility decline and the role of SOC in the global C cycle, then these should be both discussed in the introduction.

Thanks for this comment. We have removed the parts on SOC sequestration in the introduction and discussion to streamline the manuscript.

adaptation to different soil conditions is needed. Land cover history also explains site specific SOC dynamics and can help to define the local potential for SOC sequestration. Indeed soils cultivated as pasture or forest have initially high SOC stocks that are difficult to maintain under cultivation (Lal, 2004).

of understanding on how CSE_a is influenced by the interactions between organic resource quality, soil carbon saturation, climate and soil mineralogy are needed to effectively target soilsthat have the potential to sequester SOC. This calls for standardized

The carbon storage efficiency as explained in section 2.4.2 is calculated only in the top 15 cm. How does this relate to the tillage depth? If you calculate the SOC stock based on the soil mass of only the top 15 cm and you homogenize the soil until e.g. 25 cm, you miss a significant part of the SOC stock that originates from the input of organic matter. The carbon storage efficiency will then be underestimated by nearly 50% in this example.

The incorporation depth is also to about 15 cm, as this is the depth down to which the soil is manually tilled using a hand hoe. Yet, we agree that leached DOC is not accounted for. We thus have added these limitations to the methods and also refer to CSE as an approximation in the discussion.

was a valuable approach to quantify CSE_a . Another potential limitation of these calculations considering only the 0-15 cm soil layer (soil layer in which organic resources were incorporated), is that CSE_a may be underestimated if significant portions of earthen inputs are stabilized in denser soil layers from a a locabed dissolved error in arthen

255 carbon inputs are stabilized in deeper soil layers from e.g.leached dissolved organic carbon.

The discussion section should be carefully read in order to a balance between the results of the long-term trials and their possible explanation from processed based

research. In order to strengthen the link to the long term experiments it would be helpful to consistently refer to the figures and tables in the discussion section (e.g. the statement in line 449-451 can be checked in Fig.3 while your reference to Fig. 4 in line 455 is very helpful).

Thanks for this comment. We added this additional reference and others where suitable.

The results of our study showed clearly that even at high rates of organic resource addition (4 t C ha⁻¹ yr⁻¹), SOC generally decreased (i.e., of four sites in this study, only Aludeka showed increased SOC; Fig. 4). This was indicated by the

As it stands there is a risk of speculation on the role of microbial processes, the redistribution of organic amendments and the input of organic matter through roots. The authors show that they have a broad knowledge on these topics, but they should carefully evaluate to what this theory is supported by the long-term trials (see e.g. the discussion FYM lines 383-390, lignin and polyphenol lines 381-384 or root input lines 426-430).

Thanks for this comment, we have carefully reviewed the whole discussion, removing parts that were mostly speculative.

Line 13 and throughout the document 'concentration' refers to a dissolved substance in a liquid. Content is a broader term that can be applied to both liquids and solids. I would recommend using 'content' throughout the manuscripts. Please note that you use SOC content in line 41.

We agree that content is the better choice of words. We have changed it throughout the manuscript.

SOC <u>concentration content</u> for the control and the farmyard manure treatments at 4 \pm ton C ha⁻¹ yr⁻¹, respectively. Adding *Calliandra* or *Tithonia* at 4 \pm ton C ha⁻¹ yr⁻¹ limited the loss of SOC <u>concentrations</u> contents to about 24% of initial SOC,

Detailed comments

Lines 154-164 Please specify if the soils contain rock fragments. For SOC stocks these need to be taken into account (see Poeplau et al in SOIL 2017 or 2018)

You are correct and we have added how we accounted for stones (in both volume and weight).

soil weight per known core volume. Bulk density was determined based on the calculated absolute dry weight of stone-free samples and and the stone-corrected volume of soil cores, assuming a density of stones of 2.65 g cm⁻³. At all sites but Aludeka (with 1, 5 and 12% of stones in 0-15, 15-30 and 30-50 cm depth, respectively), stone content was very low (below 1% on

175 average). Prior to C and N analysis, samples were finely ground with a ball mill, then soil C and N concentrations contents

Section 2.4.2 The calculation of the CSE is quite complex. An equation would be much appreciated in order to evaluate the results e.g. Table 3.

In accordance with the other reviewer comments, we have revised the description. We have also added the requested equation.

C input as the independent variable (i.e., 0, 1.2 and 4 t C ha⁻¹ yr⁻¹). These regressions were site- and organic resource-specific, so that estimates of CSE_a per site and organic resources could be compared:

260 $dSOC = Site + C_{in} : OR + C_{in} : OR : Site$

(1)

Here, dSOC is the mean annual change in SOC stocks in 0-15 cm (t C ha⁻¹ yr⁻¹). Site is the site specific intercept, C_{in} the amount of annual C input (t C ha⁻¹ yr⁻¹) and OR the type of organic resources. Note that ":" represents interactions and that there was no OR-specific intercept. The intercept was set to site specific, i.e., not allowed to vary between different organic resources at the same site (i.e., the SOC change at 0 t C ha⁻¹ yr⁻¹ did not vary between treatments). The slope of this regression
was taken on the numerical variable C_{in} represented the yearly change in SOC stocks (in 0-15 cm) per t C ha⁻¹ yr⁻¹ of organic

Line 286 Please check (and also avoid 'respectively' as it does clarify the text). Do not FYM treatments show the lowest decrease and SD the highest decrease?

We agree to remove the "respectively". We also agree that we need to change the text, so that is becomes clear that highest losses correspond to lowest CSE. This was done.

The efficiency with which organic resources were converted into SOC varied by site and treatment (Table 3). The treatments with respectively the highest and lowest decrease in SOC concentration-content (FYM and SD), corresponded to the treatments

with the highest and lowest CSEalowest and highest CSEa, respectively. The highest CSEa for FYM was found in at Sidada

Please check line 318 and rephrase.

This sentence was rephrased.

405 3% of initial SOC concentrations contents per year, are likely not only from caused by SOC mineralization but also from crosion. It isby erosion. Despite the gentle slope of the experimental site, Machanga showed extremely strong signs of topsoil

Line 378 Have these classes of organic residues already been discussed?

Based on a comment by the other reviewer, we removed this discussion about classes, as it distracts from the main message.

not consistently across sites (e.g., not in at Embu). The postulated advantage of class 2 organic resources, such as *Calliandra*, relative to class 1 organic resources, such as *Tithonia* (Kunlanit et al., 2014), was, however, not supported by our results. Kunlanit et al. (2014) hypothesized that a higher SOC formation of class 2 organic resources was related to the polyphenol and lignin content, increasing synchronisation between microbial demand and availability by preventing leaching of nutrients and

485 dissolved organic carbon. A potential reason for similar performance of *Calliandra* and *Tithonia* could thus be, that they only differed in terms of polyphenol contents but not in lignin.

Line 399 The discussion of the stochiometry mainly concerns the CN ratio. Other nutrients such as P and K or micro nutrients are not considered. Would not it be better to discuss CN ratios rather than the broad term 'stochiometry'? (see your note of caution on line 410).

We agree and changed this accordingly.

in the +N treatment than in the -N treatment (Fig. 4). The impossibility to enhance a poor organic resource stoichiometry failure
 to enhance the performance of organic resources with low C:N ratios in building SOC by amending mineral nutrients from external sources either indicates indicates either that organic resource quality is determined by more than just ratios of nutrients, or that it is difficult for microbes to counterbalance the poor quality of organic amendments by taking up nutrients in the another states.

Lines 426-430 The discussion on root vs aboveground litter input cannot be related to the results of the long-term trials.

Because our results clearly show that external inputs alone are insufficient to maintain SOC, we still think that we should present some alternatives and discuss them briefly. Yet, in consideration of your comment, we shortened this part.

- 40 are known to Intercropping can produce more biomass than sole crops on the same surface due to complementary use of resources (i.e., light, nutrients, water; Malézieux et al., 2009; Bedoussac et al., 2015). Cereal-legume intercropping was shown to increase SOC in the long term compared to sole crops (Li et al., 2021b). Recent research (Prescott et al., 2021; Sokol et al., 2019) confirms plant C inputs through roots as most effective contributors to SOC increase (Denef and Six, 2006), because they form new SOC with higher efficiency than external above-ground organic resource inputs (Rasse et al., 2005; Jackson et al., 2017;
- 45 Sokol and Bradford, 2019), partly due to the fact that microbes can easily assimilate root exudates, which contributes to the formation and stabilization of microbial necromass (e.g. Wang et al., 2022), partly due to the proximity of inputs and microbes . The latter. The proximity of microbes to C inputs has been highlighted as the most important factor (Lavallee et al., 2018), so focusing only on the quantity of C inputs and ignoring quality may be misleading, as demonstrated by the poor performance of adding 4 t C ha⁻¹ yr⁻¹ saw dust and maize straw in this study (Fig. 1).-
- 50 In the light of the results of our study, suitable measures should thus increase the input quantity and quality at the same time. For example, intercropping can produce more biomass than sole crops on the same surface due to complementary use of resources (e.g., light, nutrients, water, etc.; Malézieux et al., 2009; Bedoussac et al., 2015). Especially the high quality inputs from cereal legume intercropping were shown enhance SOC build up in the long term compared to sole crops (Li et al., 2021b) . If intercropping is not suitable, crop rotations with legumes (Cong et al., 2015) and for SOC formation (Lavallee et al., 2018).
- 55 . Therefore, the use of crop genotypes with strong root systems (e.g. Van de Broek et al., 2020), may be alternatives. Our data partly shows the importance of roots by the is seen as a good option to build SOC (e.g. Van de Broek et al., 2020) The higher

Figure 7 The way to estimate the predicted SOC change should be explained briefly in the Materials and methods section.

We agree and have added corresponding sections to Materials and Results.

10

Finally, we compared the losses of SOC in the control treatments to losses reported in other experiments. A simple modelling approach was used assuming a yearly 1st order kinetic SOC loss:

$SOC_{loss} = SOC_{initial} * (1 - k * t)^{yr} - SOC_{initial}$

Here, SOClass and SOCinitial correspond to the initial SOC contents and the SOC loss at the end of the experimental period (g kg⁻¹), k, is the annual loss of SOC under a base temperature of 10° C (g g⁻¹ SOC), and t, a site-specific rate modifier, based on site mean annual temperature (MAT) and a Q_{10} of 2:

$t = 2^{(\frac{MAT - 10}{10})}$

The annual turnover of SOC (k), was manually calibrated, and the Nash Sutcliffe modelling efficiency was calculated for assessing the goodness of model fit, as follows:

$$EF = \frac{\sum_{z=1}^{n} (O_z - \bar{O})^2 - \sum_{z=1}^{n} (O_z - P_z)^2}{\sum_{z=1}^{n} (O_z - \bar{O})^2}$$
(4)

Here, EF is Nash-Sutcliffe modelling efficiency, Q_z is the measured SOC loss of the z-th site at experiment end, Q_y the mean loss of SOC in all experiments and P_z the simulated value corresponding to O_z .

Lines 462-475 Is there any indication of the effect of soil mineralogy from the data? If not this paragraph is purely based on the literature and not supported by the longterm trials. It should be at least be reduced.

We agree and have shortened the part considerably.

Possibly, there are differences in Possibly, differences in soil mineralogy between sites , which, under similar inputs, are the main explanatory factor for differences in SOC stocks (Zech et al., 1997; Reichenbach et al., 2021; Keller et al., 2022), next to texture (Mainka et al., 2022). For example, mafie parent material has shown a higher potential to store SOC in aggregates than felsie material (Reichenbach et al., 2021). As a consequence, it is unlikely that rainfall and temperature combined with texture 595 are sufficient to explain differences in CSE_a, as put forward by some authors (Schimel et al., 1994; Smith and Waring, 2019) - Even a temperature dependent CSE_a (Frey et al., 2013) would not suffice to explain the higher CSE_a observed in Aludeka eompared to Machanga (Calliandra, Tithonia) or in Sidada compared to Embu (farmyard manure): Sidada has higher temperatures than Embu and in Aludeka and Machanga temperatures are similar. Clearly, a better understanding of the interactions of play a role here (Zech et al., 1997; Reichenbach et al., 2021; Keller et al., 2022; Mainka et al., 2022) and all soils except Aludeka can

(3)

(2)

be considered as effectively C saturated under the current maize monocropping (Castellano et al., 2015). Overall, there is a lack 600 of understanding on how CSEa is influenced by the interactions between organic resource quality, soil carbon saturation, climate