



EGUsphere, referee comment RC1
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Comment on egusphere-2022-1416

Anonymous Referee #1

Referee comment on "Managing soil organic carbon in tropical agroecosystems: evidence from four long-term experiments in Kenya" by Moritz Laub et al., EGUsphere,
<https://doi.org/10.5194/egusphere-2022-1416-RC1>, 2023

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 $4 \text{ t C ha}^{-1} \text{ yr}^{-1}$. However, on the landscape-scale only rates of $1.2 \text{ t C ha}^{-1} \text{ yr}^{-1}$ 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.

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?

How representative is Maize cropping for the study region?

Introduction

Overall well-structured and well written.

Line 43: Stabilization capability of SOC of 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.

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.

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

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

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?

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?

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

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.

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

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

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

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

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.

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

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

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.

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.

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

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

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

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?

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.

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

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.

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

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

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.

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.

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.

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

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?

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 CO₂ emissions and yields, which is not part of your study or covered by any data.

Line 362 – 368: Very good point.

Line 373: I'm curious, does the quality of farmland manure change depending on the

animals and their food?

Line 378: What is the meaning of these classes?

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

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

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.

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

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.

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

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?

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?

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.

Conclusion

Overall well written.

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.

Minor comments

Line 345 – 347: Split the sentences into two.

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

Line 770: Check formatting of the reference.