

EC2

The current paper describes how conservation tillage management may or may not impact SOC stocks in two field experiments in Zimbabwe. While this theme is not very novel, the study does stand out in targeting understudied SSA-cropping systems and in the robustness of the methodological approach. It is unfortunate that only little effort was put into quantifying root biomass and overall plant-C inputs as this leaves the analysis rather speculative at points. Another critical point is that the rationale for comparing CO₂ emissions between the various combinations of tillage x mulch retention x crop rotation (maize vs. maize & pea) is not really motivated. The net balancing of C-in and outputs results into changes in the SOC stock. So what is then the added value of on top comparing soil CO₂ effluxes in the growing season (and once also in the 'dry season')? What did the authors hope to learn in addition to what they could not deduce from just looking at the SOC stock data? Most sections are overall in good shape, aside from the discussion. Its structure requires further work and there are a number of important comments on the at times rather limited interpretation of mechanisms behind found changes in SOC between the various treatments. If these points are adequately addressed the current paper could make a good contribution to the state-of-the-art on CA in low-input farming systems.

We would like to thank the Editor for the comments that have helped improving our manuscript. In trying to answer the comments raised above, we resorted to highlighting questions/comments raised above and answering them separately. Therefore, in black are the Editor's comments/questions and in blue are our responses. We also answered the detailed/minor comments below.

It is unfortunate that only little effort was put into quantifying root biomass and overall plant-C inputs as this leaves the analysis rather speculative at points.

It is true that we had to estimate belowground C-inputs using root:shoot ratios because root sampling is very destructive. The site is a long-term experiment but the plot sizes are small (6*12 m) especially for treatments with rotation (6*6 m). It was therefore not possible to sample for root biomass and distribution. However, annual aboveground biomass and inputs to the soil have been measured since the establishment of the experiment. In this case, a common practice is to estimate belowground carbon inputs using root:shoot ratios (Bolinder et al., 2007; Jones et al., 2009; Villarino et al., 2021; Cardinael et al., 2022). We agree there is uncertainty using this approach, but we believe this is still valuable to estimate carbon inputs to the soil between the different systems as it can explain the presence or absence of additional SOC storage.

We agree that the motivations behind the CO₂ measurements were not explained adequately. In a recent paper (Shumba et al 2023b), we presented N₂O and CH₄ emissions. We decided to show the CO₂ emissions in the SOC related paper as it is easier to interpret changes in CO₂ efflux when looking at SOC stocks and at biomass production. The second point is that we compared CO₂ efflux on the maize rows and on maize inter-rows. Mulch was only applied in maize inter-rows, and fertilizer applied on the rows. We were expecting to observe a higher CO₂ efflux in inter-rows with mulch than in inter-rows without mulch, but we did not. We however found a higher CO₂ efflux on the rows than in the inter-rows, that we attribute to more autotrophic respiration from roots. While there were no significant differences in SOC stocks between rows and inter-rows, we showed that it was crucial to consider spatial heterogeneity (rows vs inter-rows) when investigating CO₂ efflux as a chamber position either on the rows or on the inter-rows might overestimate or underestimate fluxes at the plot scale. We have now better explained this in the manuscript.

Detailed/minor comment:

L96 and 67 seem to contradict each other, rephrase perhaps one

We agree with the comment and we rephrased L67 by condensing it and joining it with previous sentence to finally read as follows:

“The potential of CA to increase SOC stocks and thereby mitigate climate change has, however, been much debated (Corbeels et al., 2020a) but the general understanding is that, this potential is relatively low (Du et al., 2017; Powlson et al., 2014, 2016; Cheesman et al., 2016; Corbeels et al., 2020a).”

L91 ‘have’ instead of ‘has’

Agreed and we effected the changes as suggested.

L A motorized handheld corer was used to take >30cm soil samples: what diameter was then sampled?

The motorized corer had an inside diameter of 10 cm. We have included this dimension in the materials and methods section (Section 2.3).

L208 provide more details on the CHN analyzer

More details were added as suggested where we also summarised the procedure and gave the specifications of the CHN elemental analyser as follows:

“SOC was analysed in the ISO9001:2015-certified IRD LAMA’s laboratory in Dakar by dry combustion on 100-mg aliquots of soil (ground to < 200 µm) using a CHN elemental analyser (Thermo Finnigan Flash EA1112, Milan, Italy).”

Fig2. The quality is rather low, consider something else than the current hatching

This comment is very helpful and the hatching for the 5-10 and 15-20 cm depths were changed in Figure 2.

Fig.3 I do not see the point in repeating the 0-5cm SOC concentration data here in the bar charts.

We have deleted the bar charts as we agree with the Editor’s comments.

3.3 first part could be condensed further

In agreement with the comment, we have condensed the first paragraph without compromising on key results points.

Table 1 lowercase letter to designate significantly different means should not be placed in superscript

We agree with the comment and it was addressed accordingly in Table 1.

L382 ‘at UZF’ comes in a bit late in this sentence, move forward. Same remark also for the next sentence and ‘at DTC’

We concur with this comment and we moved both UZF and DTC forward so that readers can quickly comprehend the site being referred to. The sentences now read as follows:

“SOC accumulation rates at UZF differed significantly ($p < 0.05$) with soil depth where top soil layers (0-5, 0-10, 0-15, 0-20 and 0-30 cm) had SOC accumulation rates that were at least 6.9 times less than when considering the 0-100 cm soil profile (Table 2). In contrast, there were no significant ($p > 0.05$)

differences, at DTC in SOC accumulation rates with depth. On average, SOC accumulation rates ranged between 0.13 and 0.08 Mg C ha⁻¹ yr⁻¹ in the top soil (0-5 cm) to 0.33 and 1.16 Mg C ha⁻¹ yr⁻¹ for the whole 1 m soil profile at DTC and UZF, respectively. The depth and treatment interaction had no significant ($p > 0.05$) effects at both sites.”

L384 so also provide likewise figures for UZF

We agree with the comment and changes were addressed to include the average SOC accumulation rates for the top soil as well as the whole soil profile for UZF.

L391 ‘had’ instead of ‘has’; overall the sentences in this section 387-398 read strangely ‘treatment x had net loss or accumulation of SOC’ rephrase please in a more active form.

Comments were addressed as suggested and the paragraph now reads as follows:

“On the other hand, the different treatments in this study had significant ($p < 0.05$) effects in SOC accumulation / loss rates in the top 20 cm soil layer at both sites (Table 2). At DTC, NT had significant ($p < 0.05$) net loss of SOC in the 0-20 cm layer, ranging between -0.09 and -0.02 Mg C ha⁻¹ yr⁻¹, whereas NT treatments (NTM, NTR, NTMR) had SOC accumulation rates ranging from 0.17 to 0.38 Mg C ha⁻¹ yr⁻¹. However, maize stover mulching (NTM) had significantly ($p < 0.05$) higher SOC accumulation rates than CTR (2.9 – 4.2 times) and NT (5.2 – 13.5 times) in the top 15 cm and 20 cm layers, respectively. The different combinations of mulching and rotation under NT (NTM, NTR and NTMR) had no significant ($p > 0.05$) differences in SOC accumulation rates. Similarly, rotation treatments (CTR, NTR, NTMR) showed no significant ($p > 0.05$) differences in SOC accumulation rates. Thus, the full CA treatment had similar SOC accumulations rates to treatments with at least two combinations of CA principles (NTM and NTR) and to CTR.”

Use the same manner to denote significance level in Tables 1 & 2

We agree with the comment and it was addressed accordingly in Table 2.

L418 rewrite this sentence

We agree with the comment and the sentence was rephrased to read “There were significant ($p < 0.001$) differences in cumulative OC inputs between treatments (Figure 4).”.

L430 ‘0.05’ I assume?

The observation is correct. It was a typo- error and we have corrected to “0.05”.

L465 instead of > 50%, write maybe ‘over half’

The suggestion is acceptable. We have addressed accordingly to read as follows:

“This means that, over half of SOC stocks for this study were in the sub-soil (30-100 cm) which reflects on the importance of sub-soil SOC stocks.”

L469 ‘treatment effects’ what sort of treatments?

The comment is noble. We have specified that we are referring to no-tillage (NT) plus mulch and/or rotation treatments. The statement now reads:

“Significant effects of mulch and/or rotation under NT were restricted to the top 30 cm in our study as well as other studies in SSA (Dube et al., 2012; Powlson et al., 2016) and the world at large (Balesdent et al., 2018; Yost and Hartemink, 2020), which is most likely why the default soil depth for IPCC for SOC studies is 0-30 cm (IPCC, 2019).”

L472 long and difficult to understand sentence

We concur with the comment and we have split the sentence into two. We further condensed the second sentence after splitting as follows:

“However, this underestimates whole soil profile C storage (Harrison et al., 2011; Lorenz and Lal, 2005; Singh et al., 2018). Therefore, it is crucial to consider whole soil profile sampling when monitoring SOC storage in agricultural ecosystems to determine their C sequestration potential in the pursuit of climate change mitigation (Malepfane et al., 2022)”.

L491 add ‘and’ before ‘hence’

We addressed the suggested comment.

L497 ‘do not’ instead of ‘does not’, ‘add SOC’ is also not very well phrased further on in the sentence

Comment was considered and the sentence was rephrased accordingly.

L514 Be clear, supposedly you are referring to an N-fertilization effect onto maize, but that is not spelled out here. Do then also report on the found (in)differences in maize crop residue returns.

Thank you for the comment. We have clarified that there were no significant treatment effects due to maize-cowpea rotation, as expected from increased N cycling due to biological nitrogen fixation from the preceding cowpeas. Instead, at DTC, it was maize stover mulching that improved maize yields (Shumba et al., 2023) and subsequently belowground biomass.

L527-535 shorten and rewrite so that this reason for finding no difference in SOC stock when considering deeper soil profiles could potentially also be in part attributed to lack of replicates.

We took cognisance of the comment and we effected the suggested remark where we alluded that our study was limited to four replicates which might not have enough statistical power to detect significant differences between treatments. The statements are as follows:

“This can be alluded to an accumulation of uncertainty when cumulating SOC stocks in several soil layers with their respective error of measurement. This weakens the power of detecting statistically significant differences even where such differences exist (Kravchenko and Robertson, 2011). Kravchenko and Robertson, (2011) bemoaned the lack of enough replication when sampling deep soil horizons to reduce variability and the importance of post hoc power analysis to reduce Type II error. This study was limited to four replicates which might not have enough statical power to detect statistically significant differences between treatments.”

L545 best to refer once more to DTC here

We addressed the suggested remark.