Dear Katerina,

Please find below a list of the intended actions. These reflect the comments of yourself and the most recent reviewers.

Regarding Reviewer 3's comments, we have obliged where we can. However, given the comments received it would appear the reviewer may have not reviewed the corrected version of the manuscript. Their comments do not appear to reflect the corrected version of the manuscript most recently returned to you.

Some of their comments e.g. " The concept of biochemical recalcitrance [...] is introduced in lines 67–68", and ", in section 2.1 I do not see systematic information on the type and amount of fertilization and organic matter inputs [...] it makes the reader wonder on the fertilization applied in the conventional field (no info given), and crop residue management in the regenerative field (no info given) [..]" relate to information that is included in the version of the paper most recently returned to you. We also note that Reviewer 3 has not reviewed the entire paper and may have missed the additional details they request, which are present in the discussion. However, we have actioned their comments where appropriate.

In addition, please also find attached a clean and tracked-changes copy of the manuscript reflecting these changes as outlined below.

Yours sincerely,

Sam Keenor

Reviewers/Editors Comments and Suggested Edits

Authors focus on assessing SOC stocks in the field, which is the amount of SOC stored across the soil profile at minimally 30 cm depth per unit area, according to the IPCC guidelines (Penman et al 2003, "Good practice guidance for land use, land-use change and forestry"). Especially in the case of reduced tillage practices as part of the transition towards regenerative farming, assessing SOC stocks at multiple depths is relevant. Reduced tillage is known to 'concentrate' SOC in the upper soil layers, as this SOC is no longer mixed with lower soil layers. Therefore, SOC contents at lower soil layers usually is 'diluted' with reduced tillage, so that SOC stocks across the entire soil profile may not be that different after all. Therefore, SOC stocks should have been measured at lowers depths, preferable even lower than 30 cm as subsoil processes are very different from topsoil processes (Rovira et al, 2022, CATENA). In this study, SOC is only measured at 0-7.5cm depth (line 144, and I think this is insufficient given the information above.	While the reviewer identifies that the depth of soil carbon stocks does have an impact upon the total carbon sequestration potential and any 'dilution effects', the limit of 7.5cm depth for measurement was a methodological constraint. However, this measurement depth was kept constant between both the regeneratively managed fields and the conventional control – thus we highlight that these effects would be equivalent in both cases, and changes in the soil carbon content of each soil would be valid to the depths at which we sampled for comparison. Given the scope of this project and the inability to re-sample we suggest no action. We have however, included a statement in the manuscript regarding the point raised by the reviewer as a limitation of the research. This statement is tethered to the citations provided by the reviewer.
Authors mention multiple times the carbon sequestration potential when interpreting the results (e.g. lines 20, 26, 563). However, it is important to distinguish carbon sequestration (truly capturing CO2 from air into soil) vs. carbon reallocation (moving carbon from one field to another), see Leifeld et al 2013, PNAS. To assess whether the study design can inform the carbon sequestration potential, it is important to know the organic matter inputs to the field. However, in section 2.1 I do not see systematic information on the type and amount of fertilization and organic matter inputs. The use of organic fertilizer and compost tea for regenerative farming is mentioned for regenerative farming, and	Reference to the type and amount of fertiliser applied to both the conventional control fiend and the blackcurrant fields are explicitly mentioned in the text of section 2.1. Furthermore, no additional organic matter has been added to the fields during the course of the experiment. Additionally, reference to the depth of cultivations (in the control soil) is explicitly stated in section 2.1 In making revisions we have clarified the mechanism of soil carbon stock build. These being 1. Mitigated organic carbon mineralisation due to low soil disturbance and 2. Increased carbon flows into the soil from blackcurrant

stubble re-incorporation for conventional farming. However, it makes the reader wonder on the fertilization applied in the conventional field (no info given), and crop residue management in the regenerative field (no info given). Given the application of organic fertilizer in the regenerative fields, I would say that at least part of the increased SOC contents derive from carbon reallocation rather than sequestration. Moreover, I also miss more information given the tillage operations (e.g. depth), given its importance for SOC transfer across the soil profile.	residues and the roots of the blackcurrant bushes. In both cases these carbon flows are likely higher than in the arable control, where crop biomass is harvested and exported out of the field.
The concept of biochemical recalcitrance of carbon is outdated, except for pyrogenic carbon (Schmidt et al, 2011, Nature), unless argued otherwise. However, this concept is introduced in lines 67-68 without reference, and interpreting the TGA results as currently done (labile vs. recalcitrant SOC) is only supported by one reference without theoretical explanation. It also makes me wonder whether physically protected SOC within aggregates is part of this recalcitrant fraction? I wonder why it is not chosen to use the t50 of TGA that accounts for the continuum of soil carbon stability (Lehmann et al 2015, Nature)? I believe deviating from these common concepts and instead use a simplified 2-pool classification of labile vs recalcitrant SOC needs to be much more substantiated, and based on theoretical stabilization mechanisms of SOC, as is done for the distinction of POM-MAOM by size or density fractionation by Lavallee and Cotrufo.	Further content has been included in the more recent iterations of the manuscript to better explain the concepts of carbon recalcitrance. “I wonder why it is not chosen to use the t50 of TGA that accounts for the continuum of soil carbon stability (Lehmann et al 2015, Nature)?”. We are unclear on what the reviewer is driving at here and what they expect by way of an action. Reference to the Schmidt et al., 2011 paper has been made where relevant in the text. With regards to POM-MOAM the reviewer makes a valid point. We highlight that the conceptual framework of the paper is aligned to this, in so much as our results explicitly define carbon pools and their recalcitrance in non-stable and stable aggregates. however, given the soil fractionation methodology, there is no clear way to define explicitly what would constitute POM vs. MAOM in the samples. A statement has been added to the methodology section sustained by the citation provided by the reviewer has been included to explain this, although we highlight the methodology used is a

	well acknowledged standard. Furthermore, reference has been made in the conclusions regarding opportunities for further research.
Section 2.6: Given the changed tillage regime that potentially alters bulk density, I believe that not the fixed depth method but the equivalent soil mass method should have been used to calculate SOC stocks from SOC content (Von Haden et al 2018, GCB)	This is a key point made in the discussion section of the text, and was indeed the purpose of this methodological decision. Thus, arguing the relevance of the equivalent soil mass method, highlighting that due to changes in soil bulk densities following changes in soil tillage regimes and sample depth. Suggesting this as a superior method for future work based on the results and design of this experiment. No further action required.
Section 2.7: what about meeting the assumptions for these models, have they been checked, and can hence the statistical results be trusted?	These are standardised and robust statistical tests. Pre-processing of data was conducted to confirm normality of data prior to application of statistical analysis. No further action required.
Based on the observations, I recommend to reject this manuscript in its current form for publication. However, I do believe that the experiment and results are super interesting and can be used if the story is not focused on assessing SOC stocks and carbon sequestration, but on assessing the changes in SOC quality and aggregate stability in top soil after a system transformation towards regenerative farming, acknowledging the continuum of SOC stability and providing more quantitative management information relevant for SOC characteristics.	Considering the first round of reviewer comments we restructured the text. This reframed the text to acknowledge changes to the SOC stock and quality and takes the focus away carbon sequestration alone. In further revision in response to the second round of reviewers' comments, we will seek to make this direction clearer. However, some focus on sequestration potential will be retained as we believe this is an important aspect to include.
Next reviewer	
A lot of work has been conducted here, thus many results are presented, which may be detrimental to the clarity of the paper: while explained, the numerous notions (WSA/NWSA, labile/recalcitrant, aggregates, thermal stability in aggregates, bush/alley soils, several	We feel that removal of either the alley or bush regime data into the supplementary, while shortening the text, would serve to create confusion. Outcomes for the bush vs alley regimes are markedly different. To pick one over the other for inclusion in the main text would be misleading.

years…), make the article uneasy to read. I suggest some data appear only in Supplementary Information (e.g. bush soils detailed in the paper and alley soils only in SI).	Further, where SOC stocks are calculated the proportional area of bush and alley, along with the SOC concentration and soil bulk density, are conflated. Moving either the bush or alley data to the SI will remove a lot of transparency in the reporting. This said, in further revision to the manuscript we will seek to further reduce unnecessary information and where appropriate truncate text and discussion to improve readability and comprehension of the text.
L24: it would be good to reformulate this sentence, as the 'proportion of NWSA : WSA' is not really 'increasing' (only the % of WSA is indeed increasing).	Sentence will be reformulated to reflect this comment.
L71: non verbal sentence, I guess it is rather the end of the previous sentence. Same for L81.	Text and grammar have been corrected.
L88: soil properties regulation	Text and grammar have been corrected.
L198: 'that which passed through the 63 µm sieve after the HMP treatment'	Text and grammar have been corrected.
L222: what is expected between 700 and 1.000°C? (Or – why not stop heating before 1.000°C?)	This was done in accordance with following the methods and temperature windows described in Mao et al., (2021) with only the carbon measured before 700°c, reports suggest that between ~700-1000°C predominantly inorganic carbon sublimes from the sample (Mao et al., 2021). Inclusion of this previously unmentioned fraction now mentioned in the methods section.
L252: any idea why the lowest global SBD is in year 3 treatment and not year 7?	This difference is likely relating more to the underlying soil physiology and soil stoniness than to management practice, (I.e soil texture and aggregate fraction (Figure SI 3)) now highlighted in text.
L262: while it does not fully change the overall observation, I would not say 'generally decrease with time'	Text revised.
L318: there is no year 2 soil, I guess it is 3.	Text revised.
L342-347: So, recalcitrant carbon content increases with time, but not significantly? This paragraph is not totally clear.	Text improved.
L352: if you mention the increase for year 7, you should also mention the one for	Text improved reflecting the higher improvement in the final year.

year 5 as you talk about both years. Same for L359.	
Please check the numbering of your figures before the next revision since Figure 1 exists twice	Figures, Tables and figure legends will be checked throughout to reflect correct numbering position and content in the text.
General: check for punctuation, spelling and grammar. Several non-verbal sentences are still found along the text.	Text and grammar will be reviewed throughout to improve readability and comprehension, and correct mistakes.
move surplus discussion to the supplement to aid readability	In further review of the manuscript we have further reduced unnecessary or superfluous information from the introductory literature review and discussion sections of the paper, and improved the conciseness, readability and comprehension of the text.
Remove the word "associated" from NWSA and WSA in Table S1, to match how it is used throughout the text. Also, consider adding the word "thermally" to labile and recalcitrant carbon to specify the operational definition. In fact, potential caveats of this operational definition and other mechanisms governing "recalcitrance" (as brought up by Reviewer #3 and reviewed in Schmidt et al. Nature 2011) should be mentioned briefly when the method is first introduced (line 134). This is all nicely done in the title and text of section 3.3, but could be added briefly to the introduction and throughout.	Text in the SI table has been amended to exclude associated and include thermally, additionally, reference to thermal lability/recalcitrance has been noted in the introduction and methods text, with a qualifying note of language going forward. Additionally, these have been noted in all relevant figures.
Spell out abbreviations in figure captions to make it easier for figures to be understood on their own.	Figures, Tables and figure legends will be checked throughout to reflect correct numbering position and content in the text.
Line 46 – consider replacing the semi-colon with a comma or rewording into two sentences.	Change made.
Line 59 - combine this sentence with the paragraph below and remove first comma. Also, consider splitting the following paragraph which is quite long.	Change made.
Line 132 – the word respectively is not needed here, since it seems that it refers	Change made.

to the bush and alley soils which are specified in their respective places in the sentence. Check and reword for clarity.	
Line 267 - combine sentence with previous paragraph. In fact, there are many spots like this throughout the text where a sentence or two stand as their own paragraph (e.g., line 362, 393, 440, 443, 447, 452, etc). This disrupts the flow and connections between findings. Please find and rearrange as needed.	All similar paragraphs to that as described have been adjusted throughout the text to better improve flow and place focus on the discussed topic, improving the conciseness, readability and comprehension of the text.