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Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

31st Mar 23

Dear Mr Reiff,

Your manuscript titled "Elevated soil carbon storage, soil quality and biodiversity under permaculture." has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to

- address these criticisms (that is, either to incorporate the suggestions or provide a compelling argument why the point made by the reviewer is not valid, or relevant to the editorial threshold as outlined below)

AND

- meet our five editorial thresholds as outlined below,

then we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

* Editorial threshold 1: Provide compelling new insights into the effect of permaculture on soil carbon, nutrients and biodiversity;

* Editorial threshold 2: Provide comprehensive sampling and site details and demonstrate that your experimental design and control are robust;

* Editorial threshold 3: Conduct appropriate statistical analysis to demonstrate that your claims are robustly supported by your data;

* Editorial threshold 4: Report and consider the influence of land use history on your findings;

* Editorial threshold 5: Clearly communicate the new contribution that your findings offer to our understanding of permaculture and agroecology and clearly contextualise this advance against a comprehensive discussion of the related literature.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at

Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments and our editorial thresholds with a list of your changes to the manuscript text (which should be in a separate document to any cover letter) and any completed checklist:

[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Kate Buckeridge, PhD
Editorial Board Member
Communications Earth & Environment
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Clare Davis, PhD
Senior Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements (Download the link to your computer as a PDF.)

For your information, you can find some guidance regarding format requirements summarized on the following checklist:(<https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf>) and formatting guide (

style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors report data collected on 9 Permaculture plots and control plots under conventional landuse situated in the vicinity. They showed that soil as well as plant parameters showed improved quality under Permaculture. Although the study may have been carried out carefully I cannot recommend this paper for publication. All parameters reported by the authors are strongly dependent on the soil type. Firstly, no valid statistical analyses were carried out as the choice of control plots is poor. No randomized block design was used in this study. The fact that the control plots are situated no more than 3 km from Permaculture plots is not sufficient to prove similarity. Very simple t-tests were used, which do not take into account the fact that the initial properties of both treatments may be different. Moreover, landuse history strongly affects soil carbon storage and other soil characteristics – however, it is unknown in both situations. Moreover, it is already well-known that agroecological practices, which is based on similar principles as Permaculture lead to the similar improvements as shown by the authors. The hypothesis tested by the authors is too general (i.e. effect of permaculture). Therefore, I recommend rejection of this paper due to poor scientific quality and lack of novelty.

Moreover some minor suggestions to improve the introduction :

L ; 38 : replace 'However' by "On the other hand"

L. 39-41 : comes somewhat out of the context. Readers, who do not know the 4 per 1000 initiative will not understand – improve argument or delete

L.45 : This argument needs to be supported by a scientific reference

48-49 : I cannot follow the argument with the phosphorus – increased P availability through SOC sequestration ? If this is true than it should be true not only for P but for all other mineral nutrients.

L.50 : replace 'contemporary' by 'recent'

Reviewer #2 (Remarks to the Author):

General comments:

The manuscript presents an interesting comparative study focusing on differences in biodiversity and soil quality when comparing permaculture and control plots/reference sites.

Although results are interesting and scientific studies on permaculture are scarce, the general approach is quite simplistic, and the whole manuscript remains very broad and general. The introduction could better substantiate the research gap and objective, the methods could be more detailed, the results could have included an analysis on the effect of land use type, and the discussion could have more depth and nuances.

Please find my specific comments below:

Title:

I think the title could be more attractive and clearer. I would change to something like: Permaculture

practices in Central Europe increase carbon storage, soil quality and biodiversity

Abstract:

The way values are reported are not consistent (sometimes in percentage, sometimes rounded to the number of times e.g. “3 times higher”). Besides, the values are based on mean comparisons, but there is no mention to statistical significance or measures of dispersion.

Introduction:

Line 39: The authors mention that we should maintain food security. However, food security is already an issue for many people.

Line 40: Why is the 4 per 1000 initiative so important? I would say there are other targets/goals that are more relevant and holistic (not only looking at soil carbon).

Lines 35-58: The first paragraph reads a bit too broad and superficial. Maybe including some examples and more concrete information would make the paragraph stronger. By the end of it, it is still not clear what is the main research gap tackled by the study.

Lines 59-80: Still not clear what exactly is the research gap addressed by the study. Concepts of Permaculture and Agroecology are not linked to any kind of concrete examples of experiences and practices, which makes the text a bit vague.

Lines 81-89: Here the research gap is finally explained, but further information what is known and what is not known based on literature is lacking.

Lines 92-92: Not clear what is meant by “had to pay for itself”.

Line 96-97: I understand what is meant after reading the other sections, but the following sentence could be better phrased: “equalling previous land use for seven of the permaculture plots, was sampled.”

Results:

Overall results are reported in a clear and structured way.

However, it is not clear why the comparison with sites from the German soil inventory was only made for certain variables.

Besides it is not clear what results are reported after the mean (standard error? Standard deviation? Confidence interval?)

And finally, I wonder if there is any pattern considering the variation within permaculture farms. In that sense, results could have been much more detailed, showing how the different indicators vary across land use types. Lumping all the results together gives a very general perspective, but understanding the role of land use (and not only permaculture vs. control) could bring more specific insights.

Discussion:

The discussion is clear and concise. However, it reads quite broad and general. There is a lack of depth, especially regarding the comparison with results from other studies. There are many studies that show the impact of similar practices on soil quality and biodiversity, but this is barely mentioned in the current discussion. I also miss a more nuanced discussion on levels of adoption. For instance, in the case of agroforestry, how many trees per hectare would be needed/enough? I also miss a more detailed discussion on the mechanistic effect of the practices on the assessed indicators.

And finally, I would suggest to include some sentences on the limitations of the current study and recommendations for future studies. For instance, the current study has a simplistic approach comparing two groups (permaculture vs. control) whereas the understanding of mechanistic effects,

causal relationships and levels of adoption is still missing.

Material and Methods:

Some sentences could be better phrased or miss information. For instance: "Minimum area for land use elements to be considered as field for sampling was 400 m² to fit the mapping procedure of 24,58." (line 352).

I feel there is information missing:

- Was soil type considered in the analysis? It is mentioned that control fields were 3km apart from permaculture plots, but soils can be quite heterogeneous and change with this distance range. Besides, it would be interesting to know more about what kind of soils the authors are talking about.
- Although authors mentioned they conducted interviews in the Introduction, this is not well explained in the methods section.
- Only three sampling points were taken per plot. I found this number quite low, especially because there is no mention to composite samples.
- It would be good to see the plots used to check the assumption in the Appendix.
- I also found that the assumption check description could be improved. For instance, It is not clear what data was used to check the assumption (e.g. residuals? Variables themselves? Difference between observations?)

Since the citations are given in numbers, some of the sentences read odd (e.g. line 386)

Finally, I think (more and more) it is considered good practice to be transparent and open about data. Therefore, I would highly recommend the authors to provide their data and R script available on-line in a repository. The link to this repository should be mentioned in the main manuscript.

Reviewer #3 (Remarks to the Author):

The study compares soil and biodiversity properties of 9 pairs of cropping fields across Germany and Luxembourg. At each location, one field is managed 'according to the permaculture framework and one field is managed under the 'regionally predominant land-use type'. The results show very clear differences according between permaculture and control plots with permaculture plots showing strong increases in SOC, macro- and micronutrient concentrations, earthworm abundance and diversity, plant diversity and reduced bulk soil density. The results suggest that permaculture can, besides producing agricultural yields, can strongly increase ecosystem service provisioning.

While permaculture is a regularly discussed framework in the public and among practitioners, there is a surprising dearth of scientific studies on the subject. Increasing the sustainability of agricultural production is, however, a hotly debated topic in the scientific literature. Providing data on the effects of permaculture management on soil and biodiversity properties is therefore timely and needed. As far as I know, this is the first study comparing such properties across a range of permaculture fields and the results are impressive.

I have, however, a range of questions on the methodology, specifically, the experimental design and sampling scheme. The description of criteria by which the permaculture and control plots were

selected is insufficient and it remains unclear to me, how many samples were taken in which pattern at each site.

The authors report certain criteria that the farms/or plots had to meet, including 'at least two different types of land use (e.g. grazing and fruit trees) had to be integrated at the agroecological production.' However, later it is stated that at each farm, 'one field of each land use type (vegetables, arable crops, tree crops, grassland, grazed land) was randomly selected to be sampled.' So, how many fields were sampled per farm? Five (one of each land use type?) or only two? Or did two of five land-use types have to be present, and only one was randomly selected?

The methods state that at each sampling plot, 3 soil samples were collected. How many sampling plots were there per farm? Was the type of land-use sampled different at each permaculture farm? If so, the information on the land-use type of the sampled plots should be included in the manuscript. Right now, the entire sampling procedure remains cryptic to me.

Looking at the aerial Photograph in Fig. 1, permaculture plots are obviously very heterogeneous in nature. If soil samples were taken in 10m distance from each other and you have, for example, a plot with what I interpret as raised beds or Hugelbeete in Fig. 1, the exact spot of taking a soil sample can make a big difference. Did you sample in the raised beds or also on the paths in-between? When calculating the Carbon sequestration potential, did you account for the heterogeneity across a certain area in such a permaculture plot or did you extrapolate the measured values across the entire plot? I feel like much more detail on the plots and the respective sampling scheme is needed to properly evaluate the study.

Much of this uncertainty is probably owed to the complex and heterogeneous nature of permaculture systems and the somewhat loose and open definition in general. I consider this an important study that presents important data on the benefits that permaculture approaches can provide. However, a major revision, especially on the methods description is needed for a full evaluation.

Detailed comments

Line 30: For environmental sustainability, the yield potential also plays a major role. Even though you may not have looked at productivity in this study, it would be interesting to include and discuss some existing information on this in the discussion.

Line 132: Which P fraction did you assess- total or plant available P? Please specify. A table providing an overview of the soil characteristics at all sites investigated would also be good to include somewhere.

Fig. 4: It would be good to also include P-data, since it is mentioned across all the other parameters shown in the text.

Lines 194-197: What are the area units the species richness numbers are based on? Please provide full units together with the numbers. This also applies to Figs. 6 and 7.

Line 198: in permaculture plots, in controls or in general?

Line 254-256: the carbon sequestration potential should also consider how the high organic C contents are achieved. If this is done by adding biomass from other fields that are, in turn, depleted in SOC over time, overall C sequestration might be much lower. At least something to discuss.

Line 331-333: As mentioned earlier, it would be worthwhile touching on this in the discussion.

Line 353-354: How was the regionally predominant land-use type determined? Looking at Table 1, these were mostly agricultural plots. Did you specifically focus on agriculture plots or did you consider all potential land-use types? Please provide more information on the selection procedure.

Reviewer #1 (Remarks to the Author):

The authors report data collected on 9 Permaculture plots and control plots under conventional landuse situated in the vicinity. They showed that soil as well as plant parameters showed improved quality under Permaculture. Although the study may have been carried out carefully I cannot recommend this paper for publication. All parameters reported by the authors are strongly dependent on the soil type.

Firstly, no valid statistical analyses were carried out as the choice of control plots is poor.

No ramomized block design was used in this study. The fact that the control plots are situated no more than 3 km from Permaculture plots is not sufficient to prove similarity. Very simple t-tests were used, which do not take into account the fact that the initial properties of both treatments may be different. Moreover, landuse history strongly affects soil carbon storage and other soil characteristics – however, it is unknown in both situations.

Moreover, it is already well-known that agroecological practices, which is based on similar principles as Permaculture lead to the similar improvements as shown by the authors. The hypothesis tested by the authors is too general (i.e. effect of permaculture). Therefore, I recommend rejection of this paper due to poor scientific quality and lack of novelty.

- Thank you very much for your valuable comments and suggestions! We completely revised statistical analysis using up to date generalized linear mixed models and included soil texture class and pH value as random factors to account for possible differences in soil characteristics between permaculture plots and control fields. However, this is an evaluation of real world case studies with rather complex system design. Choice of control plots was very difficult. Therefore we chose control plots matching (where possible) the land use history of permaculture plots and selected methods of investigation to allow for comparison with large published datasets. We included land use history information for both permaculture plots and control fields and discussed its possible implications for our findings. We added an extensive discussion on findings on agroecological practices that were also used on permaculture plots. Hereby, we elaborate the new insights our study provides on the advantages of the integration of various practices into a whole, complex agroecological production system. We really appreciate your recommendations as including them strongly improved our manuscript!

Moreover some minor suggestions to improve the introduction :

L ; 38 : replace ‘However’ by ‘‘On the other hand’

- done

l. 39-41 : comes somewhat out of the context. Readers, who do not know the 4 per 1000 initiative will not understand – improve argument or delete

- done

l.45 : This argument needs to be supported by a scientific reference

- done

48-49 : I cannot follow the argument with the phosphorus – increased P availability through SOC sequestration ? If this is true than it should be true not only for P but for all other mineral nutrients.

- improved. Sure it should be true for other mineral nutrients, but P is most important due to its finite storages.

l.50 : replace ‘contemporary’ by ‘recent’

- done

Reviewer #2 (Remarks to the Author):

General comments:

The manuscript presents an interesting comparative study focusing on differences in biodiversity and soil quality when comparing permaculture and control plots/reference sites.

Although results are interesting and scientific studies on permaculture are scarce, the general approach is quite simplistic, and the whole manuscript remains very broad and general. The introduction could better substantiate the research gap and objective, the methods could be more detailed, the results could have included an analysis on the effect of land use type, and the discussion could have more depth and nuances.

- Thank you very much for your valuable comments and suggestions! As you will see, we included almost all of your recommendations which strongly improved our manuscript.

Please find my specific comments below:

Title:

I think the title could be more attractive and clearer. I would change to something like: Permaculture practices in Central Europe increase carbon storage, soil quality and biodiversity

- done

Abstract:

The way values are reported are not consistent (sometimes in percentage, sometimes rounded to the number of times e.g. “3 times higher”). Besides, the values are based on mean comparisons, but there is no mention to statistical significance or measures of dispersion.

- We changed all values to percentage and all mentioned differences are significant.

Introduction:

Line 39: The authors mention that we should maintain food security. However, food security is already an issue for many people.

- We included this argument, because it is an issue for many people.

Line 40: Why is the 4 per 1000 initiative is so important? I would say there are other targets/goals that are more relevant and holistic (not only looking at soil carbon).

- We think that it is an important initiative and guideline for climate change mitigation in agriculture. While there are more holistic targets like the sustainable development goals (also mentioned in the manuscript), the 4 per 1000 initiative has a specific focus on soil carbon in agriculture and more important a clearly measurable target that we could address.

Lines 35-58: The first paragraph reads a bit too broad and superficial. Maybe including some examples and more concrete information would make the paragraph stronger. By the end of it, it is still not clear what is the main research gap tackled by the study.

Lines 59-80: Still not clear what exactly is the research gap addressed by the study. Concepts of Permaculture and Agroecology are not linked to any kind of concrete examples of experiences and practices, which makes the text a bit vague.

- From our point of view it is the purpose of an introduction to deduce a concrete research question from a previously build-up broad theoretical basis. Concrete examples of permaculture are described by this study and we now included an evaluation on important individual practices in the discussion.

Lines 81-89: Here the research gap is finally explained, but further information what is known and what is not known based on literature is lacking.

- We improved the argument.

Lines 92-92: Not clear what is meant by “had to pay for itself”.

- Permaculture plots did not rely economically on other incomes of the farm. We changed this to „economically self-sufficient“

Line 96-97: I understand what is meant after reading the other sections, but the following sentence could be better phrased: “equaling previous land use for seven of the permaculture plots, was sampled.”

- We deleted this argument for the introduction, better and more detailed explanations are found in the methods and results sections.

Results:

Overall results are reported in a clear and structured way.

However, it is not clear why the comparison with sites from the German soil inventory was only made for certain variables.

- Micronutrient concentrations were measured with a different approach by the German soil inventory, so they are not comparable with our results.

Besides it is not clear what results are reported after the mean (standard error? Standard deviation? Confidence interval?

- This information is now included at the beginning of the results section (in addition to the methods).

And finally, I wonder if there is any pattern considering the variation within permaculture farms. In that sense, results could have been much more detailed, showing how the different indicators vary across land use types. Lumping all the results together gives a very general perspective, but understanding the role of land use (and not only permaculture vs. control) could bring more specific insights.

- A statistical analysis on the effect of land-use type would be interesting. However, we do not have enough replications for testing land-use types separately. E.g. we had only 2 arable sites on permaculture plots. Additionally, variation within land-use types of permaculture plots was so high, that it would be difficult to count it as same treatment. E.g. grazing by chicken without trees vs. Grazing by sheep with trees. But we appreciate your suggestion and included a discussion on the variation within permaculture plots.

Discussion:

The discussion is clear and concise. However, it reads quite broad and general. There is a lack of depth, especially regarding the comparison with results from other studies. There are many studies that show the impact of similar practices on soil quality and biodiversity, but this is barely mentioned in the current discussion. I also miss a more nuanced discussion on levels of adoption. For instance, in the case of agroforestry, how many trees per hectare would be needed/enough? I also miss a more detailed discussion on the mechanistic effect of the practices on the assessed indicators.

- We included a discussion on most important agroecological practices, on their adoption and on effects of individual practices. However, there are complications in the isolation of single effects, since permaculture is always a combination of numerous practices.

And finally, I would suggest to include some sentences on the limitations of the current study and recommendations for future studies. For instance, the current study has a simplistic approach comparing two groups (permaculture vs. control) whereas the understanding of mechanistic effects, causal relationships and levels of adoption is still missing.

- We see the small sample size of our study as main limitation and included this in the conclusion. A greater sample size might also allow for an evaluation of effects and differences among and within permaculture systems.

Material and Methods:

Some sentences could be better phrased or miss information. For instance: “Minimum area for land use elements to be considered as field for sampling was 400 m² to fit the mapping procedure of 24,58.”(line 352).

- improved

I feel there is information missing:

Was soil type considered in the analysis? It is mentioned that control fields were 3km apart from permaculture plots, but soils can be quite heterogeneous and change withing this distance range. Besides, it would be interesting to know more about what kind of soils the authors are talking about.

- We repeated statistical analysis utilizing generalized linear mixed models and included soil texture class and pH value as random factors for evaluation of soil parameters to account for possible differences between pairs of permaculture plots and control fields.

Although authors mentioned they conducted interviews in the Introduction, this is not well explained in the methods section.

- We added this information.

Only three sampling points were taken per plot. I found this number quite low, especially because there is no mention to composite samples.

- Our financial resources allowed us only to take and analyze the present amount of samples. While more samples are always better, the clarity of results shows that the sampling intensity was sufficient. We did not use composite samples to be able to directly relate bulk density to carbon content for calculation of carbon storage.

It would be good to see the plots used to check the assumption in the Appendix.

- We repeated statistical analysis with generalized linear mixed models utilizing the Dharma package for model diagnostics. Our data is now openly available, so interested readers will be able to reproduce our results and check the plots. Including model diagnostic plots would go beyond the scope of this manuscript.

I also found that the assumption check description could be improved. For instance, It is not clear what data was used to check the assumption (e.g. residuals? Variables themselves? Difference between observations?)

- see above

Since the citations are given in numbers, some of the sentences read odd (e.g. line 386)

- We found some of these cases and corrected them.

Finally, I think (more and more) it is considered good practice to be transparent and open about data. Therefore, I would highly recommend the authors to provide their data and R script available on-line in a repository. The link to this repository should be mentioned in the main manuscript.

- As mentioned above, the data is already openly available. With the information in this manuscript statistical analysis is quite straightforward, so we see no necessity to provide an R script.

Reviewer #3 (Remarks to the Author):

The study compares soil and biodiversity properties of 9 pairs of cropping fields across Germany and Luxembourg. At each location, one field is managed 'according to the permaculture framework and one field is managed under the 'regionally predominant land-use type'. The results show very clear differences according between permaculture and control plots with permaculture plots showing strong increases in SOC, macro- and micronutrient concentrations, earthworm abundance and diversity, plant diversity and reduced bulk soil density. The results suggest that permaculture can, besides producing agricultural yields, can strongly increase ecosystem service provisioning.

While permaculture is a regularly discussed framework in the public and among practitioners, there is a surprising dearth of scientific studies on the subject. Increasing the sustainability of agricultural production is, however, a hotly debated topic in the scientific literature. Providing data on the effects of permaculture management on soil and biodiversity properties is therefore timely and needed. As far as I know, this is the first study comparing such properties across a range of permaculture fields and the results are impressive.

- Thank you very much for your valuable comments and suggestions! As you will see, we included almost all of your recommendations to improve the quality of our manuscript.

I have, however, a range of questions on the methodology, specifically, the experimental design and sampling scheme. The description of criteria by which the permaculture and control plots were selected is insufficient and it remains unclear to me, how many samples were taken in which pattern at each site.

- We included all farms that we could find that fitted our criteria. Due to the Covid19 pandemic we were limited to Germany in 2020 and 2021. We included this information.

The authors report certain criteria that the farms/or plots had to meet, including 'at least two different types of land use (e.g. grazing and fruit trees) had to be integrated at the agroecological production.' However, later it is stated that at each farm, 'one field of each land use type (vegetables, arable crops, tree crops, grassland, grazed land) was randomly selected to be sampled.'

So, how many fields were sampled per farm? Five (one of each land use type?) or only two? Or did two of five land-use types have to be present, and only one was randomly selected?

The methods state that at each sampling plot, 3 soil samples were collected. How many sampling plots were there per farm? Was the type of land-use sampled different at each permaculture farm? If so, the information on the land-use type of the sampled plots should be included in the manuscript. Right now, the entire sampling procedure remains cryptic to me.

- We improved the description. Land-use types of each permaculture plot are listed in the table. This number multiplied by 3 gives the number of soil samples for each permaculture plot. Not all land-use types were present at each farm.

Looking at the aerial Photograph in Fig. 1, permaculture plots are obviously very heterogeneous in nature. If soil samples were taken in 10m distance from each other and you have, for example, a plot with what I interpret as raised beds or Hugelbeete in Fig. 1, the exact spot of taking a soil sample can make a big difference. Did you sample in the raised beds or also on the paths in-between?

-In case of raised beds or Hugelkultur one sample each was taken from the centre of the bed, the border to a footpath separating beds and the middle in between. We included this information in the methods section.

When calculating the Carbon sequestration potential, did you account for the heterogeneity across a certain area in such a permaculture plot or did you extrapolate the measured values across the entire plot? I feel like much more detail on the plots and the respective sampling scheme is needed to properly evaluate the study.

- We did not account for heterogeneity across a broader area. Carbon storage values do only reflect measured values of soil samples from agricultural production areas. We clarified that in the results section.

Much of this uncertainty is probably owed to the complex and heterogeneous nature of permaculture systems and the somewhat loose and open definition in general. I consider this an important study that presents important data on the benefits that permaculture approaches can provide. However, a major revision, especially on the methods description is needed for a full evaluation.

Detailed comments

Line 30: For environmental sustainability, the yield potential also plays a major role. Even though you may not have looked at productivity in this study, it would be interesting to include and discuss some existing information on this in the discussion.

- We agree that a discussion on productivity of permaculture might be interesting. However, there is no scientific information on land productivity of permaculture yet. Therefore an extensive discussion on productivity of individual practices would be necessary and we think this would go beyond the scope of this manuscript.

Line 132: Which P fraction did you assess- total or plant available P? Please specify. A table providing an overview of the soil characteristics at all sites investigated would also be good to include somewhere.

- We openly provide the data and we repeated statistical analysis utilizing generalized linear mixed models and included soil texture class and pH value as random factors for evaluation of soil parameters. As mentioned in the methods section (soil analysis) we measured CAL-extractable phosphate. We also mentioned in the results section, that we measured plant-extractable soil nutrient concentrations except for nitrogen.

Fig. 4: It would be good to also include P-data, since it is mentioned across all the other parameters shown in the text.

-done

Lines 194-197: What are the area units the species richness numbers are based on? Please provide full units together with the numbers. This also applies to Figs. 6 and 7.

-We included this information in the figure description. This can also be found in the methods section.

Line 198: in permaculture plots, in controls or in general?

- clarified

Line 254-256: the carbon sequestration potential should also consider how the high organic C contents are achieved. If this is done by adding biomass from other fields that are, in turn, depleted in SOC over time, overall C sequestration might be much lower. At least something to discuss.

- True, we added that point!

Line 331-333: As mentioned earlier, it would be worthwhile touching on this in the discussion.

- see above

Line 353-354: How was the regionally predominant land-use type determined? Looking at Table 1, these were mostly agricultural plots. Did you specifically focus on agriculture plots or did you consider all potential land-use types? Please provide more information on the selection procedure.

-We compared to predominant agricultural land-use. We changed the wording to locally, to better fit our selection procedure. Locally predominant agricultural land use type was determined based on farmers interviews and supported by evaluation of aerial images five kilometers around the permaculture plot. We improved the description in the methods section.

14th Aug 23

Dear Mr Reiff,

Your revised manuscript titled "Permaculture in Central Europe increases carbon storage, soil quality and biodiversity." has now been seen by 2 reviewers of the original reviewers, whose comments are appended below. Reviewer #1 was unavailable to comment on this version. In the light of their advice we regret to inform you that we cannot publish your manuscript in Communications Earth & Environment.

You will see that Reviewer #2 continues to raise substantive concerns. Taking these points together with our editorial considerations, we are unable to conclude that your revised manuscript has addressed the previous concerns regarding the statistical handling of your data. As such, we are not convinced that your manuscript represents a sufficiently novel and compelling advance over the body of related work in the literature. At this stage we consider whether a manuscript is progressing sufficiently and, in this case, we feel it is best that we decline publication of this study in Communications Earth & Environment to avoid a protracted review process and delays.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss our decision in more detail.

Although we cannot offer to publish your manuscript, we believe the editors at our sister journal, Scientific Reports will find it interesting and recommend you transfer it there. To transfer your manuscript, please use our <https://mts-commsenv.nature.com/cgi-bin/main.plex?el=A1DE2BHK3B3JeB4X4A9ftdQcN2BcQMIwiVvDWFec3wZ> manuscript transfer portal. You will not have to re-supply manuscript metadata and files, unless you wish to make modifications, but please note that this link can only be used once and remains active until used. For more information, please see our http://www.nature.com/authors/author_resources/transfer_manuscripts.html?WT.mc_id=E MI_NPG_1511_AUTHORTRANSF&WT.ec_id=AUTHOR manuscript transfer FAQ page.

We are sorry that we cannot be more positive on this occasion and thank you for the opportunity to consider your work.

Best regards,

Clare Davis, PhD
Senior Editor
Communications Earth & Environment

www.nature.com/commsenv/
@CommsEarth

Reviewers' comments:

Reviewer #2 (Remarks to the Author):

I thank the authors for their response to my comments.

Although I can see the authors put effort to improve the manuscript and data collection covers a wide variety of indicators, I still feel there are major issues with the paper:

- The assessed permaculture systems are very different in terms of practices adopted, size, land use type, soil type and previous land use. Yet, all these differences are barely considered in the analysis, which makes the results too general and hard to grasp.

- There is little explanation/discussion on the mechanistic effects that could explain a positive effect of permaculture practices on system sustainability. Therefore, the findings remain quite superficial.

- The statistical analysis was changed to add soil pH and texture as control (random) factors. However, I still find the statistical analysis confusing and incomplete. For instance, the authors mention that they conducted post hoc comparisons, but it is not clear what analysis was used previous to the post-hoc test (e.g. ANOVA or Kruskal-Wallis?). Or just the generalized linear mixed model was used with management as a dummy variable? If that's the case, it does not make sense to me, since in the GLM model, you can add random factors but not in the post hoc test.

- The description of statistical analysis could also be improved, for instance, the authors only mention that "election of distribution family was based on data structure, model diagnostics and AIC value." However, authors do not explain how decisions were made, for instance, they could mention that if residuals were normally distributed, an ANOVA followed by a Tukey post-hoc test were conducted.

- I see some contradictory results that are not well explained in the manuscript. For instance, although the authors mention that earthworm species richness was 77 % higher on permaculture plots (line 31), the boxplot shown in Figure 4b (line 228) shows not significant differences in earthworm species richness across the four different treatments. Although in table 1 the effect of management (permaculture vs. conventional vs. organic?) on earthworm species richness is significant, the pairwise comparisons shown in table 2 are not significant.

- For me it is not clear why model-predicted means were reported instead of the sample mean (which would be more informative in my opinion).

- There are too many figures and tables, which is sometimes repetitive. For instance, the content displayed in Figures 1,2,3 and 4 largely overlaps with Table 2.

- The discussion remains too broad and general. Although a whole new section ("Comparison with individual practices") was added, the information is not very well connected and confronted with the findings of the study.

Reviewer #3 (Remarks to the Author):

The authors have sufficiently addressed my previous comments and the study has much improved.

Experimental setup and sampling procedures are now explained much clearer and support the scientific merit of the study. Given the impressive results and a dearth of scientific assessments of the effects of permaculture practices on soil and environmental parameters, I consider this a valuable study worth publishing.

I still have some minor remarks and comments. The Conclusion section should include some outlook and important open questions to be addressed in future research. For example, it will be necessary to quantify nutrient fluxes (e.g. N₂O, CO₂) emissions from soil, as well as fluxes of organic matter. It needs to be investigated how the increased soil carbon contents are achieved- where does the organic matter come from? This will be necessary to provide a precise quantification of C sequestration potential of permaculture. What is the yield potential of permaculture systems and how does it compare to conventional agriculture?

Detailed comments:

lines 64-65: 'design and manage' is being used repetitively here. Please reword.

Figure 1: I don't understand panel d). What does the x-axis exactly indicate? Why do certain colors/ locations appear twice? Please explain properly.

Line 501: The terminology is in parts confusing. You refer to the permaculture farms as 'plot' and then speak of 'fields' of different land-use types within each 'plot'. Later you refer to the fields as 'sampling plot'. Why not speak of permaculture 'farm' and the of 'plots' within each farm? It would be a more intuitive wording.

I thank the authors for their response to my comments.

Although I can see the authors put effort to improve the manuscript and data collection covers a wide variety of indicators, I still feel there are major issues with the paper:

- The assessed permaculture systems are very different in terms of practices adopted, size, land use type, soil type and previous land use. Yet, all these differences are barely considered in the analysis, which makes the results too general and hard to grasp.
- There is little explanation/discussion on the mechanistic effects that could explain a positive effect of permaculture practices on system sustainability. Therefore, the findings remain quite superficial.
- The statistical analysis was changed to add soil pH and texture as control (random) factors. However, I still find the statistical analysis confusing and incomplete. For instance, the authors mention that they conducted post hoc comparisons, but it is not clear what analysis was used previous to the post-hoc test (e.g. ANOVA or Kruskal-Wallis?). Or just the generalized linear mixed model was used with management as a dummy variable? If that's the case, it does not make sense to me, since in the GLM model, you can add random factors but not in the post hoc test.
- The description of statistical analysis could also be improved, for instance, the authors only mention that "election of distribution family was based on data structure, model diagnostics and AIC value." However, authors do not explain how decisions were made, for instance, they could mention that if residuals were normally distributed, an ANOVA followed by a Tukey post-hoc test were conducted.
- I see some contradictory results that are not well explained in the manuscript. For instance, although the authors mention that earthworm species richness was 77 % higher on permaculture plots (line 31), the boxplot shown in Figure 4b (line 228) shows not significant differences in earthworm species richness across the four different treatments. Although in table 1 the effect of management (permaculture vs. conventional vs. organic?) on earthworm species richness is significant, the pairwise comparisons shown in table 2 are not significant.
- For me it is not clear why model-predicted means were reported instead of the sample mean (which would be more informative in my opinion).
- There are too many figures and tables, which is sometimes repetitive. For instance, the content displayed in Figures 1,2,3 and 4 largely overlaps with Table 2.
- The discussion remains too broad and general. Although a whole new section ("Comparison with individual practices") was added, the information is not very well connected and confronted with the findings of the study.

Reviewer #1 (Remarks to the Author):

The authors report data collected on 9 Permaculture plots and control plots under conventional landuse situated in the vicinity. They showed that soil as well as plant parameters showed improved quality under Permaculture. Although the study may have been carried out carefully I cannot recommend this paper for publication. All parameters reported by the authors are strongly dependent on the soil type.

Firstly, no valid statistical analyses were carried out as the choice of control plots is poor.

No randomized block design was used in this study. The fact that the control plots are situated no more than 3 km from Permaculture plots is not sufficient to prove similarity. Very simple t-tests were used, which do not take into account the fact that the initial properties of both treatments may be different. Moreover, landuse history strongly affects soil carbon storage and other soil characteristics – however, it is unknown in both situations.

Moreover, it is already well-known that agroecological practices, which is based on similar principles as Permaculture lead to the similar improvements as shown by the authors. The hypothesis tested by the authors is too general (i.e. effect of permaculture). Therefore, I recommend rejection of this paper due to poor scientific quality and lack of novelty.

- Thank you very much for your valuable comments and suggestions! We completely revised statistical analysis using up to date generalized linear mixed models and included soil texture class and pH value as random factors to account for possible differences in soil characteristics between permaculture plots and control fields. However, this is an evaluation of real world case studies with rather complex system design. Choice of control plots was very difficult. Therefore we chose control plots matching (where possible) the land use history of permaculture plots and selected methods of investigation to allow for comparison with large published datasets. We included land use history information for both permaculture plots and control fields and discussed its possible implications for our findings. We added an extensive discussion on findings on agroecological practices that were also used on permaculture plots. Hereby, we elaborate the new insights our study provides on the advantages of the integration of various practices into a whole, complex agroecological production system. We really appreciate your recommendations as including them strongly improved our manuscript!

Moreover some minor suggestions to improve the introduction :

L ; 38 : replace ‘However’ by ‘‘On the other hand’

- done

l. 39-41 : comes somewhat out of the context. Readers, who do not know the 4 per 1000 initiative will not understand – improve argument or delete

- done

l.45 : This argument needs to be supported by a scientific reference

- done

48-49 : I cannot follow the argument with the phosphorus – increased P availability through SOC sequestration ? If this is true than it should be true not only for P but for all other mineral nutrients.

- improved. Sure it should be true for other mineral nutrients, but P is most important due to its finite storages.

l.50 : replace ‘contemporary’ by ‘recent’

- done

Reviewer #2 (Remarks to the Author):

General comments:

The manuscript presents an interesting comparative study focusing on differences in biodiversity and soil quality when comparing permaculture and control plots/reference sites.

Although results are interesting and scientific studies on permaculture are scarce, the general approach is quite simplistic, and the whole manuscript remains very broad and general. The introduction could better substantiate the research gap and objective, the methods could be more detailed, the results could have included an analysis on the effect of land use type, and the discussion could have more depth and nuances.

- Thank you very much for your valuable comments and suggestions! As you will see, we included almost all of your recommendations which strongly improved our manuscript.

Please find my specific comments below:

Title:

I think the title could be more attractive and clearer. I would change to something like: Permaculture practices in Central Europe increase carbon storage, soil quality and biodiversity

- done

Abstract:

The way values are reported are not consistent (sometimes in percentage, sometimes rounded to the number of times e.g. “3 times higher”). Besides, the values are based on mean comparisons, but there is no mention to statistical significance or measures of dispersion.

- We changed all values to percentage and all mentioned differences are significant.

Introduction:

Line 39: The authors mention that we should maintain food security. However, food security is already an issue for many people.

- We included this argument, because it is an issue for many people.

Line 40: Why is the 4 per 1000 initiative is so important? I would say there are other targets/goals that are more relevant and holistic (not only looking at soil carbon).

- We think that it is an important initiative and guideline for climate change mitigation in agriculture. While there are more holistic targets like the sustainable development goals (also mentioned in the manuscript), the 4 per 1000 initiative has a specific focus on soil carbon in agriculture and more important a clearly measurable target that we could address.

Lines 35-58: The first paragraph reads a bit too broad and superficial. Maybe including some examples and more concrete information would make the paragraph stronger. By the end of it, it is still not clear what is the main research gap tackled by the study.

Lines 59-80: Still not clear what exactly is the research gap addressed by the study. Concepts of Permaculture and Agroecology are not linked to any kind of concrete examples of experiences and practices, which makes the text a bit vague.

- From our point of view it is the purpose of an introduction to deduce a concrete research question from a previously build-up broad theoretical basis. Concrete examples of permaculture are described by this study and we now included an evaluation on important individual practices in the discussion.

Lines 81-89: Here the research gap is finally explained, but further information what is known and what is not known based on literature is lacking.

- We improved the argument.

Lines 92-92: Not clear what is meant by “had to pay for itself”.

- Permaculture plots did not rely economically on other incomes of the farm. We changed this to „economically self-sufficient“

Line 96-97: I understand what is meant after reading the other sections, but the following sentence could be better phrased: “equaling previous land use for seven of the permaculture plots, was sampled.”

- We deleted this argument for the introduction, better and more detailed explanations are found in the methods and results sections.

Results:

Overall results are reported in a clear and structured way.

However, it is not clear why the comparison with sites from the German soil inventory was only made for certain variables.

- Micronutrient concentrations were measured with a different approach by the German soil inventory, so they are not comparable with our results.

Besides it is not clear what results are reported after the mean (standard error? Standard deviation? Confidence interval?

- This information is now included at the beginning of the results section (in addition to the methods).

And finally, I wonder if there is any pattern considering the variation within permaculture farms. In that sense, results could have been much more detailed, showing how the different indicators vary across land use types. Lumping all the results together gives a very general perspective, but understanding the role of land use (and not only permaculture vs. control) could bring more specific insights.

- A statistical analysis on the effect of land-use type would be interesting. However, we do not have enough replications for testing land-use types separately. E.g. we had only 2 arable sites on permaculture plots. Additionally, variation within land-use types of permaculture plots was so high, that it would be difficult to count it as same treatment. E.g. grazing by chicken without trees vs. Grazing by sheep with trees. But we appreciate your suggestion and included a discussion on the variation within permaculture plots.

Discussion:

The discussion is clear and concise. However, it reads quite broad and general. There is a lack of depth, especially regarding the comparison with results from other studies. There are many studies that show the impact of similar practices on soil quality and biodiversity, but this is barely mentioned in the current discussion. I also miss a more nuanced discussion on levels of adoption. For instance, in the case of agroforestry, how many trees per hectare would be needed/enough? I also miss a more detailed discussion on the mechanistic effect of the practices on the assessed indicators.

- We included a discussion on most important agroecological practices, on their adoption and on effects of individual practices. However, there are complications in the isolation of single effects, since permaculture is always a combination of numerous practices.

And finally, I would suggest to include some sentences on the limitations of the current study and recommendations for future studies. For instance, the current study has a simplistic approach comparing two groups (permaculture vs. control) whereas the understanding of mechanistic effects, causal relationships and levels of adoption is still missing.

- We see the small sample size of our study as main limitation and included this in the conclusion. A greater sample size might also allow for an evaluation of effects and differences among and within permaculture systems.

Material and Methods:

Some sentences could be better phrased or miss information. For instance: “Minimum area for land use elements to be considered as field for sampling was 400 m² to fit the mapping procedure of 24,58.”(line 352).

- improved

I feel there is information missing:

Was soil type considered in the analysis? It is mentioned that control fields were 3km apart from permaculture plots, but soils can be quite heterogeneous and change withing this distance range. Besides, it would be interesting to know more about what kind of soils the authors are talking about.

- We repeated statistical analysis utilizing generalized linear mixed models and included soil texture class and pH value as random factors for evaluation of soil parameters to account for possible differences between pairs of permaculture plots and control fields.

Although authors mentioned they conducted interviews in the Introduction, this is not well explained in the methods section.

- We added this information.

Only three sampling points were taken per plot. I found this number quite low, especially because there is no mention to composite samples.

- Our financial resources allowed us only to take and analyze the present amount of samples. While more samples are always better, the clarity of results shows that the sampling intensity was sufficient. We did not use composite samples to be able to directly relate bulk density to carbon content for calculation of carbon storage.

It would be good to see the plots used to check the assumption in the Appendix.

- We repeated statistical analysis with generalized linear mixed models utilizing the Dharma package for model diagnostics. Our data is now openly available, so interested readers will be able to reproduce our results and check the plots. Including model diagnostic plots would go beyond the scope of this manuscript.

I also found that the assumption check description could be improved. For instance, It is not clear what data was used to check the assumption (e.g. residuals? Variables themselves? Difference between observations?)

- see above

Since the citations are given in numbers, some of the sentences read odd (e.g. line 386)

- We found some of these cases and corrected them.

Finally, I think (more and more) it is considered good practice to be transparent and open about data. Therefore, I would highly recommend the authors to provide their data and R script available on-line in a repository. The link to this repository should be mentioned in the main manuscript.

- As mentioned above, the data is already openly available. With the information in this manuscript statistical analysis is quite straightforward, so we see no necessity to provide an R script.

26th Oct 23

Dear Mr Reiff,

Thank you for your appeal.

You can access your appealed manuscript through the following link:
[link redacted]

Sincerely,

Editor
Communications Earth & Environment
The Macmillan Building
Crinan Street
London N1 9XW

15th Dec 23

Dear Mr Reiff,

Your manuscript titled "Permaculture in Central Europe increases carbon storage, soil quality and biodiversity." has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

EDITORIAL REQUESTS:

Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

*****Please take care to match our formatting and policy requirements. We will check revised manuscript and return manuscripts that do not comply. Such requests will lead to delays. *****

Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

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In order to accept your paper, we require the files listed at the end of the Editorial Requests Table; the list of required files is also available at <https://www.nature.com/documents/commsj-file-checklist.pdf>.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Clare Davis, PhD
Senior Editor
Communications Earth & Environment

www.nature.com/commsenv/
@CommsEarth

REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

This is the third time I revise the manuscript and I can see the authors have put a lot of effort to improve it. Overall, it is an interesting paper, showing the potential of permaculture for the development of more sustainable agricultural systems.

The statistical analysis has improved and is now clearer and more coherent.

Most of my comments were addressed although it is sometimes hard to find the changes in the manuscript since the authors did not mention line numbers in their response. For some of the responses I also find the additions incomplete. For instance, although I appreciate the new paragraphs added about Variability and land use history of permaculture plots, the text is missing references. Besides, necessary next steps are missing (e.g. assessing the influence of specific variables such as complexity level on the success of permaculture fields).

Reviewer #3 (Remarks to the Author):

I still consider this an important study on an important, yet understudied issue. The manuscript again improved substantially with respect to description of the experimental procedures and analyses. Moreover, the additional discussion on potential mechanisms behind the findings adds an important level of depths. Additional tables 3 and 4 greatly help to understand the experimental setup.

However, I still have some concerns and points that require further clarification, especially with respect to the statistical analysis, terminology and the presentation of the results. See below my specific comments.

The authors describe that at each sampling plot, three samples in 10m distance were collected. While at permaculture plots, several sampling (1-6) plots were sampled, it is unclear how control plots were handled. Were they only sampled at one location on three sampling points in 10m distance? If this is the case, this means that the setup is spatially very unbalanced. Data for a very confined area in the control plots is compared to a potentially much bigger area in the permaculture plots. Is this the case and was this considered in the analyses?

The terminology is confusing. You speak of “permaculture plots” as entire farms or part of farms. Then you select fields of differing land-use types within one plot and call them “sampling plot”. The repeated use of the word “plot” for different hierarchical levels in the design should be avoided. Please adapt and harmonize the terminology throughout the MS. I suggest using different wording for the different levels, e.g.: 9 “Locations”, each with “Permaculture site” and “Control site” and then “sampling plots” within each site.

In line 461 you speak of “two different types of land-use” on the same area. Then, in line 465, you speak of “fields of each land-use type”. To my understanding you are using the term land-use type for two different aspects here. I suggest calling the former “land-use practices” instead. Then in line 469 you use “land use elements”. Please harmonize and properly define terminology.

To properly understand the results section, some aspects have to be introduced before the results. The comparison with literature data appears only in the results section (L. 126) It should be introduced beforehand. Same with the comparison between conventional and organic. It first appears in Table 2 (referred to in line 122) but is not previously explained in the text.

Detailed comments:

L. 42: “Agriculture is responsible” seems a bit one-sided. I suggest “... is an important contributor to the crossing...”

L.76: You nicely explain and delimit the terms agroecology and permaculture but I would refrain from using yet another specific term, especially if it is that loosely defined as regenerative agriculture.

L. 91 “ diversified farming systems” is yet another term that is not defined. Since agroecology is the broadest concept that includes regenerative and diversified farming systems, I would stick to the comparison to the concept of agroecology.

L. 124: I would first present the data before stating a conclusion.

L. 128: quickly introduce what C storage entails in comparison to C content.

L. 167-169 More explanation needed in caption for panel d): What is regressed against what? What does the time indicate? Why are there no yellow points? Control and permaculture plots are indistinguishable in this plot, this should be adapted. Referring to all Figures: Location 9 has grey dots just as the literature data. This is confusing, please choose different colors.

L. 225 and Fig. 4: Why is no area with trees reported for control plots? Please explain somewhere.

L- 298... might have otherwise been stored in soils elsewhere

Conclusion: I am still missing some lines on the potential productivity of permaculture compared to other agricultural practices. Is there data available? This should be stressed also with reference to the SDGs mentioned in the conclusion.

L. 461-463: The purpose of this point is unclear. Does this refer to the interactive aspect of permaculture? Please add some information to improve clarity

Reviewer #2 (Remarks to the Author on COMMSENV-23-0080A):

I thank the authors for their response to my comments.

Although I can see the authors put effort to improve the manuscript and data collection covers a wide variety of indicators, I still feel there are major issues with the paper:

-The assessed permaculture systems are very different in terms of practices adopted, size, land use type, soil type and previous land use. Yet, all these differences are barely considered in the analysis, which makes the results too general and hard to grasp.

We agree that assessed systems are very different and regret, that we were not able to make a more detailed analysis due to a small sample size. However, permaculture is a very new and innovative approach and farms utilizing it are still rare, at least in Europe. We ask to understand, that this is the first extensive study on this approach worldwide implemented by real-world commercial farms. Therefore, we never expected more than first general results. But we agree with your demand on a more detailed analysis, which definitely has to follow a first general study like this one. Thus, we added this as research question in the conclusion.

-There is little explanation/discussion on the mechanistic effects that could explain a positive effect of permaculture practices on system sustainability. Therefore, the findings remain quite superficial. We outlined in detail in the introduction (line 91 ff) as well as in the discussion (line 396 ff), that there are many findings on isolated practices that could explain the positive effects of permaculture to some extent. Also the mechanistic effects of practices (e.g. agroforestry) are well documented. However, we think that it would go beyond the scope of this manuscript to review the effects of each individual practice in detail. Still, we included an extra paragraph on these effects in the discussion (line 396 ff) upon your request. We also outlined in the manuscript, that stronger effects of permaculture might be attributed to synergies between individual practices (line 430 ff). We are aware that the complex nature of permaculture inherits a somehow overwhelming variety of combinations of practices. Therefore we ask to admit that we were forced to make compromises in order to prevent exceeding the scope of this first general study on permaculture systems.

-The statistical analysis was changed to add soil pH and texture as control (random) factors. However, I still find the statistical analysis confusing and incomplete. For instance, the authors mention that they conducted post hoc comparisons, but it is not clear what analysis was used previous to the post-hoc test (e.g. ANOVA or Kruskal-Wallis?). Or just the generalized linear mixed model was used with management as a dummy variable? If that's the case, it does not make sense to me, since in the GLM model, you can add random factors but not in the post hoc test. We conducted a generalized linear mixed model for each dependent variable. We tested fixed predictor variables (management in almost all cases) for significance, with a Type II Wald χ^2 -test using the Anova function of the 'car' package. For models with a significant fixed predictor variable we conducted a post hoc test with Tukey correction utilizing the „emmeans“ package, because this one is able to do a post-hoc test on mixed models with random factors. We apologize that the description was not detailed enough and strongly expanded it in the manuscript.

-The description of statistical analysis could also be improved, for instance, the authors only mention that “election of distribution family was based on data structure, model diagnostics and AIC value.” However, authors do not explain how decisions were made, for instance, they could mention that if residuals were normally distributed, an ANOVA followed by a Tukey post-hoc test were conducted.

We are grateful for this remark, as we admit that the description we provided was not detailed enough. At first, for some variables possible distribution families were limited by the data structure.

This was e.g. the case for share of area with trees as this variable in percentage is limited to a space between 0 and 1 (or 100%) and therefore the „beta“ distribution families. Next we conducted several glm's for each variable with varying distribution families beginning with a gaussian or normal distribution and then ran model diagnostics. These included a test on the distribution of the residuals. If more than one distribution families produced glm's with acceptable diagnostics we selected the model according to the Akaike Information Criterion, an index for the goodness of fit of a model. We also clarified this in detail the manuscript.

-I see some contradictory results that are not well explained in the manuscript. For instance, although the authors mention that earthworm species richness was 77 % higher on permaculture plots (line 31), the boxplot shown in Figure 4b (line 228) shows not significant differences in earthworm species richness across the four different treatments. Although in table 1 the effect of management (permaculture vs. conventional vs. organic?) on earthworm species richness is significant, the pairwise comparisons shown in table 2 are not significant.

Our text says that earthworm species richness was by trend 77% higher. We used this wording to indicate (by scientific convention) that the corresponding p-value is lower than 10 % but larger than 5 %. The exact p-value is given in the respective figure (line 222).

It may be surprising but is correct, that post-hoc comparison p-values (in this case $p=0.073$) are higher due to the Tukey correction and therefore might lack significance previously found for the variable in general (in this case $p=0.026$).

-For me it is not clear why model-predicted means were reported instead of the sample mean (which would be more informative in my opinion).

Sample means (reported in the first version of the manuscript) are similar to model-predicted means. However, we want to show effects of permaculture without influence of any random effects. For example, soil organic carbon levels are generally higher for soils with finer soil texture. Therefore sample means might show a higher or lower difference between permaculture and control, just because of finer or coarser soils on control fields. To bypass this bias in the presentation of results it is advisable to report model-predicted means instead.

-There are too many figures and tables, which is sometimes repetitive. For instance, the content displayed in Figures 1,2,3 and 4 largely overlaps with Table 2.

Table 2 provides exact p-values and test statistics of post hoc comparisons, while the figures only provide significance levels, but also means, confidence intervals and individual data points. However, we see the argument of the reviewer and will attach this table as supplementary information.

-The discussion remains too broad and general. Although a whole new section (“Comparison with individual practices”) was added, the information is not very well connected and confronted with the findings of the study.

For each variable, at first we repeated effect size (in %) of permaculture found in this study, before discussing literature effect sizes (in %) of individual practices (line 396 ff), to allow a direct comparison. We were not able to find a better approach to connect existing evidence on individual practices to our findings. However, we are open to suggestions.

Reviewer #3 (Remarks to the Author on COMMSENV-23-0080A):

The authors have sufficiently addressed my previous comments and the study has much improved.

Experimental setup and sampling procedures are now explained much clearer and support the scientific merit of the study. Given the impressive results and a dearth of scientific assessments of the effects of permaculture practices on soil and environmental parameters, I consider this a valuable study worth publishing.

I still have some minor remarks and comments. The Conclusion section should include some outlook and important open questions to be addressed in future research. For example, it will be necessary to quantify nutrient fluxes (e.g. N₂O, CO₂) emissions from soil, as well as fluxes of organic matter. It needs to be investigated how the increased soil carbon contents are achieved- where does the organic matter come from? This will be necessary to provide a precise quantification of C sequestration potential of permaculture. What is the yield potential of permaculture systems and how does it compare to conventional agriculture?

Thank you very much for these suggestions! We agree and included a paragraph on research questions in the end of the conclusions.

Detailed comments:

lines 64-65: 'design and manage' is being used repetitively here. Please reword.

-done

Figure 1: I don't understand panel d). What does the x-axis exactly indicate? Why do certain colors/ locations appear twice? Please explain properly.

For organic carbon storage a model was fitted with age as predictor variable and location and soil texture class as random factors to estimate carbon sequestration. To set today as baseline, age of permaculture plots was set to zero and age of paired control plots was set to negative age of corresponding permaculture plot. This calculation was done only for six permaculture plots, where previous land use equalled land use of control fields. Further this calculation is based on the assumption that carbon level was originally sufficiently equal on plot pairs and did not change on control fields.

Therefore the x-axis indicates the age of the permaculture plot, although in this figure today datapoints belong to the permaculture plot and "older" datapoints to the respective control field of the same location (indicated by the same color).

Thanks for this remark. We improved the caption of the figure and explained in detail in the Statistics part of the Methods section.

Line 501: The terminology is in parts confusing. You refer to the permaculture farms as 'plot' and then speak of 'fields' of different land-use types within each 'plot'. Later you refer to the fields as 'sampling plot'. Why not speak of permaculture 'farm' and the of 'plots' within each farm? It would be a more intuitive wording.

We are also grateful for this remark! Our wording was indeed somehow misleading. We did not want to use the term "permaculture farm" as in some cases only part of a farm was permaculture, so not the whole farm was evaluated. Therefore we used the term "permaculture plot". We now changed this term to "permaculture site" for clarification.

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## **Reviewer #2 (Remarks to the Author on COMMSENV-23-0080B-Z):**

This is the third time I revise the manuscript and I can see the authors have put a lot of effort to improve it. Overall, it is an interesting paper, showing the potential of permaculture for the development of more sustainable agricultural systems.

The statistical analysis has improved and is now clearer and more coherent.

Most of my comments were addressed although it is sometimes hard to find the changes in the manuscript since the authors did not mention line numbers in their response. For some of the responses I also find the additions incomplete. For instance, although I appreciate the new paragraphs added about Variability and land use history of permaculture plots, the text is missing references. Besides, necessary next steps are missing (e.g. assessing the influence of specific variables such as complexity level on the success of permaculture fields).

Thank you very much for your valuable contributions! We agree, that the paragraph about variability and land use history is scarce in references. This is because we are mainly describing our own observations and conclusions, as there are no existing reference studies on permaculture systems. We list important next steps as research questions in the conclusion including investigations on the level of complexity (line 415ff).

### **Reviewer #3 (Remarks to the Author on COMMSENV-23-0080B-Z) –**

I still consider this an important study on an important, yet understudied issue. The manuscript again improved substantially with respect to description of the experimental procedures and analyses. Moreover, the additional discussion on potential mechanisms behind the findings adds an important level of depths. Additional tables 3 and 4 greatly help to understand the experimental setup.

However, I still have some concerns and points that require further clarification, especially with respect to the statistical analysis, terminology and the presentation of the results. See below my specific comments.

Thank you very much for your valuable feedback and the great effort you have once again put into revising our manuscript. Unfortunately, it appears that you were not provided with the most recent version of the manuscript. Please find the details below.

The authors describe that at each sampling plot, three samples in 10m distance were collected. While at permaculture plots, several sampling (1-6) plots were sampled, it is unclear how control plots were handled. Were they only sampled at one location on three sampling points in 10m distance? If this is the case, this means that the setup is spatially very unbalanced. Data for a very confined area in the control plots is compared to a potentially much bigger area in the permaculture plots. Is this the case and was this considered in the analyses?

Control sampling plots were handled similarly to permaculture sampling plots, with three sampling points in a 10m distance. This approach, to sample one field of each land use type once (regardless of its size), was adopted following Lüscher et al.'s (2016) methodology, allowing for comparison with their broader dataset. Therefore, the data obtained is unbalanced due to varying sample sizes between control fields and permaculture sites. For example, permaculture sites with three different land use types have three times the sample size of the control field, as three plots of the permaculture site were sampled versus one on the control field. However, unbalanced data is not an issue as linear mixed models are capable of handling it. For instance, refer to: Pinheiro, J. C. (2014). Linear mixed effects models for longitudinal data. *Wiley StatsRef: Statistics Reference Online*.

The terminology is confusing. You speak of “permaculture plots” as entire farms or part of farms. Then you select fields of differing land-use types within one plot and call them “sampling plot”.

The repeated use of the word “plot” for different hierarchical levels in the design should be avoided. Please adapt and harmonize the terminology throughout the MS. I suggest using different wording for the different levels, e.g.: 9 “Locations”, each with “Permaculture site” and “Control site” and then “sampling plots” within each site.

In line 461 you speak of “two different types of land-use” on the same area. Then, in line 465, you speak of “fields of each land-use type”. To my understanding you are using the term land-use type for two different aspects here. I suggest calling the former “land-use practices” instead. Then in line 469 you use “land use elements”. Please harmonize and properly define terminology.

The first paragraph refers to an older version of the manuscript. We already adopted the terminology of 9 “locations”, each with “permaculture site” and “control field” and then “sampling plots” within each site/field in the last revised version of the manuscript upon your reasonable request.

We also harmonized the land use terminology, speaking of integrated “land use practices” (as suggested), “land use types” on fields of study sites and “field elements”, when talking about field size. Thanks for this suggestion.

To properly understand the results section, some aspects have to be introduced before the results. The comparison with literature data appears only in the results section (L. 126) It should be introduced beforehand. Same with the comparison between conventional and organic. It first appears in Table 2 (referred to in line 122) but is not previously explained in the text.

Thank you for the suggestion. We have included a brief introduction to these comparisons at the end of the introduction section (line 116ff).

Detailed comments:

L. 42: “Agriculture is responsible” seems a bit one-sided. I suggest “... is an important contributor to the crossing...”

We changed to wording to match that of the referenced study: “... is a major driver...”

L.76: You nicely explain and delimit the terms agroecology and permaculture but I would refrain from using yet another specific term, especially if it is that loosely defined as regenerative agriculture.

L. 91 “ diversified farming systems” is yet another term that is not defined. Since agroecology is the broadest concept that includes regenerative and diversified farming systems, I would stick to the comparison to the concept of agroecology.

We understand your concern. However, we have deliberately used these terms because we believe it is important to show the connection between these concepts rather than looking at them separately.

L. 124: I would first present the data before stating a conclusion.

We agree with you. This introductory sentence for each of the results paragraphs has been removed.

L. 128: quickly introduce what C storage entails in comparison to C content.

We added a short introductory sentence to distinguish these two terms.

L. 167-169 More explanation needed in caption for panel d): What is regressed against what? What does the time indicate? Why are there no yellow points? Control and permaculture plots are indistinguishable in this plot, this should be adapted. Referring to all Figures: Location 9 has grey dots just as the literature data. This is confusing, please choose different colors.

This also refers to an older version of the manuscript, here our answer from the last revision:

“For organic carbon storage a model was fitted with age as predictor variable and location and soil

texture class as random factors to estimate carbon sequestration. To set today as baseline, age of permaculture plots was set to zero and age of paired control plots was set to negative age of corresponding permaculture plot. This calculation was done only for six permaculture plots, where previous land use equalled land use of control fields. Further this calculation is based on the assumption that carbon level was originally sufficiently equal on plot pairs and did not change on control fields.

Therefore the x-axis indicates the age of the permaculture plot, although in this figure today datapoints belong to the permaculture plot and “older” datapoints to the respective control field of the same location (indicated by the same color).

Thanks for this remark. We improved the caption of the figure and explained in detail in the Statistics part of the Methods section.”

Thank you for pointing out the grey dots in our figures. We have now used the Nature Publishing Group colour palette for the locations, which does not include grey.

L. 225 and Fig. 4: Why is no area with trees reported for control plots? Please explain somewhere. Area with trees is a farm-level indicator, which is therefore not comparable with single control fields. Control fields did not include trees in any case. We included these information in the results section as well as in the figure caption.

L- 298... might have otherwise been stored in soils elsewhere  
done

Conclusion: I am still missing some lines on the potential productivity of permaculture compared to other agricultural practices. Is there data available? This should be stressed also with reference to the SDGs mentioned in the conclusion.

There is no data available yet. We included this as a research question as part of the last revision: “What is the crop yield potential of permaculture systems in comparison to predominant industrial agriculture?”

L. 461-463: The purpose of this point is unclear. Does this refer to the interactive aspect of permaculture? Please add some information to improve clarity

This criteria was included to recognize the principle of permaculture on creating synergies through the integration of various land use elements. Thanks, we added this information.

### **Reviewer #3 (Remarks to the Author on COMMSENV-23-0080C) –**

I was now able to review the paper one more time. The manuscript has improved substantially in clarity and sampling procedures and analyses are much easier to understand. I have only two minor comments. It is an interesting and relevant study.

The expressions organic carbon stocks and organic carbon storage are both used throughout the manuscript. It is unclear whether you refer to different measures or whether you use them as synonyms. Either precisely define each expression or consistently chose one of them.

Thank you for this important suggestion. We investigated the amount of organic carbon stored in the soil, there fore we now consistently use the term “carbon stocks”, which is defined in line 121f.

L 296: please provide a reference for the statement “Greater plant diversity increases rhizosphere carbon inputs to the microbial community”

This statement already has a reference at the end of the sentence: 60. Lange, M. et al. Plant diversity increases soil microbial activity and soil carbon storage. Nat. Commun. 6, 6707 (2015)