

Referee 1

I appreciated the opportunity to review this manuscript. The authors present a well written, easy to read synthesis that outlines the knowns and knowledge gaps around a complex topic. I commend them for finding ways to simply visualize a number of concepts -- different agricultural practices related to sustainable agriculture and their impacts on various components of water regulation. I would encourage the authors to consider a few additions to clarify their methods and results.

The statement in the methods (lines 109-110) is quite brief in regards to how data was extracted from the meta-analyses. If I am following correctly it appears the authors visually from graphs or quantitatively from tables if effect sizes were significantly above zero, below zero or were not different from zero, and then counted the n=value from these studies. It is not entirely clear to me how effect size is counted. Is this one per study (i.e. one effect size per MA?) or the total in the studies that were combined, and therefore 1 effect size = 1 field site embedded within one meta-analysis? I believe it is the latter. I encourage the authors to clarify.

The number that was reported at line 108 was the total number of overall (average) effects sizes (i.e. the number of overall effects sizes for all combinations of management practices and target variables reported among all MA's included in our analysis. We have clarified this. We have also added a complete new sub-section describing how the information from the meta-analyses was summarized.

I also encourage the authors to consider if the language "increase, decrease, no effect" is the same as "positive, neutral, negative". The latter have a connotation that is assumed by the reader, while the former are (in my opinion) more descriptive of the actual effects. For example, a decrease in runoff, while "negative" in nature, could be perceived as a positive effect of a management practice.

Yes, we did consider this aspect. The direction of the effect sizes reported in the paper are purely statistical and therefore have no connotation of value. We decided to report the effects in a statistical sense, because in some instances it's not clear whether an effect would be beneficial or detrimental.

It was already noted briefly in the caption to figure 5 that the effects are purely statistical, but we have also made this clear in the text in the revised version of the paper.

Although I found the manuscript generally easy to read and follow, I believe that the short intro and methods section are incongruent with the very long results & discussion. I encourage the authors to review and look for opportunities to be more concise. For example the discussion of tillage is quite lengthy, and although it is interesting to cover some elements of yield impacts, tradeoffs, etc. I am not sure that lengthy discussion as to the paper. And further, although the introduction discusses/focuses on conservation agriculture, the practices outlined in the paper are far broader in scope. I encourage the authors to consider a slight reframing of why specific practices were selected to focus on in this analysis. I do appreciate that the selection was broad, as it allows for a quick visualization/comparison of what properties and practices are more widely studied, but again think this decision needs more clarification.

Yes, we agree. We have now shortened the results and discussion section, in particular by removing a short paragraph on grazed systems and a much lengthier paragraph on carbon sequestration in soils which is more appropriate and relevant to climate change mitigation rather than adaptation.

In the revised version, we make it clear that our synthesis is not exclusively focused on conservation agriculture. We mention the terms climate-smart and regenerative agriculture in the introduction.

I disagree with the authors final assessment in line 534 that continuous living cover reductions in SWS/recharge will outweigh increases due to carbon sequestration. While there may be concern of water limitations in drier climates, I do not believe the evidence presented supports this perception as there are few studies that look directly at this effect (judging by the gaps in evaporation and water content associated with the cropping systems practices). The benefits discussed in terms of soil physical improvements, plus benefits from carbon, with the uncertainties of rainfall variability and increases in precipitation in many places, even semiarid regions, make this assertion one that deserves more attention.

Yes, we agree that it was too speculative. We removed this statement.

There is a small typo on line 112 - believe the first word in the sentence should be "We..."

We have corrected this

Referee 2

The topic of this review is interesting and covers a necessary research line. Authors have made a huge work collecting data from literature. The list of topics and cases is really huge, and the length of the manuscript is large. However, and after reading the manuscript, I have the feeling that I have not read anything new, or the new content has not been highlighted enough. Everything is known and expected. It is true that Figure 5 is valuable and could be appreciated by potential readers, but I cannot find the new content that any publication has to include.

As a review, it is inevitable that our paper summarizes what may be already quite well known for many readers, to a greater or lesser extent. Nevertheless, we feel that our paper provides an in-depth and comprehensive synthesis that is currently lacking. Indeed, our strong focus on water regulation functions is in itself novel, as we pointed out in the introduction (at lines 70-76 in the revised version).

Our analysis, which is summarized in figure 5, has identified several areas of consensus as well as a number of important knowledge gaps related to the effects of management practices on the water regulation functions of soil. However we agree with the referee that we did not do a good enough job in highlighting these knowledge gaps and have therefore completely re-written this part of the text. We also agree with the referee that we did not outline potentially promising areas of future research arising from the knowledge gaps that we have identified. In the revised version of the paper, we now describe promising avenues of future research arising from our synthesis, briefly in the abstract and more fully in the final section which is now entitled "Conclusions and outlook".

Several aspects should be addressed, namely:

- Authors have said that the aim of this review is to evaluate "the impacts of soil and crop management practices on soil properties and processes and the various ecosystem services and functions delivered by soil" and "these analyses with respect to the water regulation functions that are relevant for climate change adaptation in Europe". However, no information is provided any the potential scenarios of climate change in Europe. For sure, adaptation will be different depending on the magnitude of climate change. I don't think that this review is focused on adaptation to climate change.

It would be impossible to provide potential scenarios for climate change in any great detail for the whole of Europe. However, we have indicated in general terms the kind of future climate scenario for Europe that we are addressing in our paper, namely a climate characterized by longer and more severe droughts as well as more frequent intense rainfall events.

This was mentioned in the first sentence of the abstract and in the first two sentences of the paper itself. However, we have added some more text on these aspects in the first paragraph of the paper as well as in the conclusions and outlook section.

- Abstract. Authors have made a good job summarizing all sections of the manuscript. However, I miss the novel aspect of this review. In my opinion, the usefulness of a review of published meta-analysis is the emergence of new aspects that cannot be observed by the individualized analysis of each study. Therefore, I encourage authors to include in the abstract the actual contribution of this review: Where is the novelty?

Please see our response to the first comment from referee 2.

- Section 2. Authors have included three sub-sections, namely: Literature search, Quality assessment, and Redundancy, but there is no information about the statistical analysis of the extracted information. Before showing the results, authors have to explain in detail, in a new sub-section, what they did with the data and information that they extracted from the selected literature. In my opinion, in a review study, there are 3 main aspects: I) what is the gap that you want to fill in; II) data mining; and III) methods and techniques to analyse the extracted information. Please, improve your manuscript taking into account these three aspects.

We don't actually do any statistical analysis of the information extracted from the meta-analyses (this is not really possible because only half of them included the raw data, as we mentioned at line 114)

Instead, we synthesized the results of published meta-analyses in a qualitative way, in order to highlight areas of agreement and consensus as well as gaps in knowledge and synergies/trade-offs. This has been clarified in the revised version. We did this by including a new sub-section where we explain how we summarized and displayed the information from the individual meta-analyses.

We suspect that one reason for this misunderstanding concerning our methodology may have been that the heading of section 2.1 ("Literature search") was inadequate. We have changed the heading to "Literature search and extraction of information".

- L.154: Please, provide arguments (e.g. references) to support the comment that macroporosity should be strongly correlated with soil water infiltration.

We certainly could cite papers to support this comment, but this sentence has now been deleted from the discussion in the paper to save space.

Editors comments

1- A clear indication of the new results provided by analysis made by the authors, in the abstract but also in in the other sections of the article, focusing the dicussion on the new insights.

We have done this (see response to the first comment from referee 2).

2- Implications under changing climate conditions, albeit qualitative given the nature of the analysis (see next point), should be improved in the discussion of results.

We have done so. We added text on this topic in the first paragraph of the introduction as well as the first paragraph of the conclusions and outlook section

3- A clarification on the kind of analysis been performed, since it is of qualitative nature. To prevent confusion I suggest that add "qualitative" in the title before the word "synthesis".

We have done so. We added a new sub-section to better explain our analyses and also modified the title of the paper as you suggested.