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Reply on RC2

Guillaume Blanchy et al.

Author comment on "Soil and crop management practices and the water regulation functions of soils: a qualitative synthesis of meta-analyses relevant to European agriculture" by Guillaume Blanchy et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-270-AC2, 2022

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. In this respect, our analysis, which is summarized in figure 5, has clearly identified strong areas of consensus, which should help to highlight research questions which may no longer require any further work!

Our synthesis has also highlighted 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, particularly in the abstract. In the revised version, we will re-write the second paragraph of the abstract to address this important omission.

We also agree with the referee that we did not outline potentially promising areas of future research that emerge from the knowledge gaps that we identified. In the conclusions section of the revised version of the paper, we will describe promising avenues of future research arising from our synthesis.

This will include recommendations to:

1. focus less on tillage effects per se and place more emphasis on the role and impact of traffic compaction in contrasting soil management systems (e.g. no-till vs. conventional till)

2. manipulate precipitation inputs (via irrigation and drought shelters) at existing LTFE's to investigate the consequences of soil/crop management systems for water regulation under climate change

3. calibrate soil-crop simulation models against experimental data from LTFE's and then use them to extrapolate for future projected climates

We will re-name this final section "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 is mentioned in the first sentence of the abstract (line 3) and in the first two sentences of the paper itself (lines 25-28).

We think the review is quite strongly focused on the potential for management practices to assist in climate change adaptation. To support this, we cite text from the paper below that specifically relates to differences in climate among the primary studies included in the meta-analyses:

Lines 204-207: "An increased proportion of perennial crops in the rotation and the presence of ground cover between the rows of perennial crops (e.g. in vineyards) increase soil infiltration and reduce surface runoff ….. These positive effects seem broadly similar regardless of climate (Xiong et al., 2018; Liu et al., 2021)".

Lines 209-213: "Meta-analyses have shown that high grazing intensities result in significantly poorer soil physical quality, in terms of larger bulk densities (Byrnes et al., 2018) and reduced infiltration rates (deLonge and Basche, 2018; Basche and deLonge, 2019) as a result of compaction by animal trampling. These impacts of intensive grazing are similar irrespective of soil texture or climate, although they appear to be slightly larger in wetter climates (Byrnes et al., 2018; deLonge and Basche, 2018)".

Lines 215-217: "for a combined dataset of 36 studies comprising both experimental and modelling studies, Meyer et al. (2019) found that cover crops reduced recharge by 27 mm/year on average with no apparent effects of climate, soil type or cropping system".

Lines 229-233. "Shackleford et al. (2019) reported an average 7% reduction in cash crop yields for systems employing non-legume cover crops in dry Mediterranean climate conditions. Similarly, in a recent meta-analysis on cover crops grown in climates with less than 500 mm annual rainfall, Blanco-Canqui et al. (2022) found that cover crops decreased main crop yields in 38% of cases, with no effects found in 56% of cases and increased yields in 6% of cases".

Lines 268-270: "Mondal et al. (2020) found no significant differences in stocks of soil organic carbon between NT and CT systems, while variations in response could not be attributed to either climate or soil type".

Lines 271-277: "Sun et al. (2020) demonstrated significant effects of climate on the changes in organic carbon stocks observed under NT systems. In their global analysis, they found that soil C sequestration was enhanced in warmer and drier regions, while soils under no-till in colder and wetter climates were just as likely to lose soil C as gain C. These findings are supported by the regional-scale

studies of Meurer et al. (2018) for boreo-temperate climates and Gonzalez-Sanchez et al. (2012) and Aguilera et al. (2013) for Mediterranean climates, although for vineyards, Payen et al. (2021) found larger topsoil C sequestration in temperate climates than hot and dry climates".

Lines 326-333: "Pittlekow et al. (2015) identified several reasons for variations in the yield response to no-till. Crop type was the most important, with no significant yield losses found under NT for oilseed, cotton and legume crops, while the yields of cereals and root crops were on average ca. 5% and 20% smaller respectively. In accordance with the results of the meta-analyses on stocks of soil organic

carbon discussed earlier, Pittlekow et al. (2015) and Sun et al. (2020) also found climate to be a significant factor, with no significant yield losses for no-till systems under rain-fed conditions in dry climates. In contrast, Peixoto et al. (2020) showed that occasional tillage increased crop yields compared with NT in dry regions and in soils with limited water retention capacity and availability, presumably by alleviating soil compaction and improving rooting".

- 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?

We agree. 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 was described in the paper, but we will try to further clarify our approach in the revised version.

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 will change the heading to "Literature search and information extraction".

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

Yes, we will cite these three review articles to support this statement:

Beven, K., Germann, P. 1982. Macropores and water flow in soils. Water

Resources Research, 18, 1311-1325.

Lee, K., Foster, R. 1991. Soil fauna and soil structure. Aust. J. Soil Res., 29, 745-775.

Jarvis, N. 2007. A review of non-equilibrium water flow and solute transport in soil macropores: principles, controlling factors and consequences for water quality. European Journal of Soil Science, 58, 523-546.