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Comment on egusphere-2022-131

Anonymous Referee #6

Referee comment on "Effects of innovative long-term soil and crop management on topsoil properties of a Mediterranean soil based on detailed water retention curves" by Alaitz Aldaz-Lusarreta et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-131-RC6, 2022

This article is an experimental study on the effect of conservation agricultural practices. Thus, an optimal treatment (OPM) is compared to a conventional treatment with a 15 cm ploughing. From my point of view the strong points of this study are: 1) the choice of treatments with the analysis of the condition obtained after 18 years of direct seeding. The documentation of long-term effects is a very valuable contribution. 2) the use of a structural stability test that I find relevant for this type of treatment. Around these strong points the study brings clear results, namely significant effects on macroporosity and structural stability.

However, the study also suffers from weak points. The experimental design remains minimalist (6 samples 3 per treatment). It is not clear whether the samples were taken on one field or whether 3 plots were observed with one sampling point per field. In the latter case, the lack of replication is a problem. Indeed, it is very likely that different fields, even if managed according to the same strategy, probably have different short and long term cropping histories. For soil characterisation, we remain somewhat in the midstream. Indeed, the functional consequences can only be partially addressed. On the SWRC, the water storage capacity, which likely is one of the most interesting property, cannot be addressed, as the measurements at the wilting point are not reported. Macroporosity is of interest for infiltration. Hydraulic conductivity measurements at saturation and/or near saturation would have allowed to interpret the clear differences in porosity and their consequence on the water flows. Previous studies have shown that the absence of tillage leads to a restructuring of the pore space which could lead to a continuity of macropores that might favour infiltraton. The link between structural stability and erosion risk, as mentioned in the conclusion, is also an interesting point that could be further investigated. To what extent can the difference in structural stabilities, as observed, play on the erosion risk.

On the form the article could be improved.

1) The apriori statement that no tillage is optimal is questionable. The study should contribute to knowledge that could be used to support such an assertion.

2) The state of the art in the introduction is very succinct. There is a lot of existing work that deals with the impact of conservation agriculture on soil properties. Some elements are given in the analysis of the results. A more extensive state of the art would allow to better situate the results. The subject is already well studied, but it is still important to document new cases (as that of the presented study) due to the complexity of the subject. Such a state-of-the-art analysis could highlight the originality of results obtained in the study.

3) The text is not always easy to read and follow, especially in the methodological and results analysis sections.

Specific points

L105 : when organic fertilizer was applied in OPM?

Table 1 : Soil granulometry would be a very valuable to interpret the results. This cannot be replaced by the fractions obtained for the structural stability.

L140 : how the inflexion determined (second derivative = 0?).

Section 2.4 : as this method is the less common, it would interesting to introduce first the type of information that can be characterized and then give some key to understand the results. Can the stability properties be related to erosion risk?

L195-198 it is a bit confusing to highlight that below field capacity the soil water capacity is comparable between treatments. This would mean that the water availability of the water storage (below field capacity) would not be affected by the treatments. It would be useful to analyse the moisture variations from field capacity to the driest points (idealy , the wilting point) that may lead to significant differences

Table 2 and 3 : the moisture at inflexion point differed a lot between treatments and analytic SWRC curve. Can you comment this.

Section 3.3 : how the porosity spectrum established?

L266-268 : rephrase

L276-279 : not clear

L280-283 : rephrase

L305-L308 a bit speculative without going further in the SWRC between Field capacity and the dry point the difference in water content is comparable

L319-321 : No data for infiltration. Results are over interpreted

L322-L327 : establish clearly the criteria that support the judgement. For instance, macroporosity can be associated to potentially good infiltration which is in favour to CM. For me there is a bias in favour to OPM that start from the introduction and goes toward the conclusion. (I am not against OPM but in evaluating agricultural systems we must stay as objective as possible).

In conclusion : in its present form the paper cannot be published. I suggest major revision including, if possible, more results (texture, infiltration, wilting points…) and/or a clear articulation between the state of art and the findings.