This reviewer strongly supports the type of research that the authors present in their paper: characterize field conditions on soils where certain management types have been applied for a substantial period. The authors also present a professional paper in terms of methods used, particularly the method to measure retention curves and by applying multiple experiments and proper statistical analyses.

Unfortunately, when they conclude in the end that: "*the study contributes to higher sustainability of mediterranean agrosystems*", they are off the mark. Sustainable development is defined as having economic, social and environmental dimensions as expressed by the UN Sustainable development Goals (SDG) and the associated European Green Deal. Scientific papers published in 2022 that suggest a link with sustainable development cannot ignore these developments in the scientific, policy and public arena's. The title of the paper is also highly misleading: "*improvement of cultivated soil*" cannot be based only on: "*water retention curves*". Numerous published papers describe a systems analysis based on the interaction between soils-water-atmosphere-plants that is needed to assess effects of soil management.

Sustainable development implies for agriculture at least: production of healthy food (SDG2&3), protection of ground- and surfacewater quality (SDG6), carbon capture and reduction of greenhouse-gas emission (SDG13) and increasing biodiversity and combatting land degradation (SDG 15).

Firstly, thank you very much for your thorough review and your helpful comments and suggestions that significantly helped to improve our manuscript.

We agree with the reviewer that our findings are too narrow to be a comprehensive contribution to the sustainability of Mediterranean agrosystems considering all the aspect that sustainability development really involved (SDG), as clearly remarks the reviewer. In fact, our main objective, as well pointed out by the reviewer, is the evaluation of an innovative -and pioneer in our region (Navarre)- soil and crops management strategy on topsoil properties. Indeed, to our knowledge, there is no other agricultural field in the whole region of Navarre where soil and crop management as proposed herein (OPM) is practiced and even less for almost two decades, with the exception of our small OPM test area were a pioneer farmer works for almost 20 years. This represents in our view, the uniqueness of our OPM study. This relevance of our work would be better and clearly pointed out in the text, especially in the Introduction and in section 2.1. (Study site [former 'zone'] and treatments).

Our evaluation is carried out through the analysis of soil water retention (curves) and also soil structural stability, as they can be considered indicators of the two most relevant limitations for sustainable crop production in the area, namely water deficit (usually expressed as annual drought) and soil loss. In short, we characterize two "phenoforms" of a typical Mediterranean soil ("genoform"). Ultimately, this is just a first step towards a more complete evaluation of the proposed agrosystem.

We believe that the actual scope of our work could be unambiguously presented as follows. First, deleting the questioned statement that "the study contributes to higher sustainability of Mediterranean agrosystems". Second, reformulating the title following the reviewer suggestion: *Effects of innovative long-term soil management on topsoil properties of a Mediterranean soil based on detailed water retention curves.*

Moreover, our work can be improved by assessing other indicators related to the SDG as indicated below.

I would recommend that the authors frame their results in a SDG context:

1. It should not be too difficult to report crop yields. (SDG2&3).

As this work was conducted in farmers' plots, yields were not explicitly measured as it is usually done in experimental fields, but some basic data are available from the farmers managing the fields (see Table 1 below). From the data provided by the farmers we cannot infer assure that the OPM treatment was better than the CM in terms of yield. Generally, there is usually no difference between the two treatments because the limiting factor is mostly water. Even so, the data suggest that farmers are not going to see a decrease in the yields of their plots by implementing sustainable practices, i.e. OPM. This is an interesting finding since normally no-till often results in reduction in crop yields of ca. 10 % (Or et al., 2021).

Table 1. Average crop yield (2016-2021) of OPM and conventional agricultural fields under conventional tillage (CM), as reported by farmers.

	Yields (t/ha)	
Crop	**OPM**	**CM**
Wheat	6.8 - 9.3	5.5 - 7.0
Barley	5.8 - 8.0	5.0 - 6.5
Rapessed	2.0 - 4.0	2.0 - 3.0
Legumes	2.2 - 3.5	1.7 - 2.5

2. Water tables may be deep and not polluted; this could be mentioned showing that groundwater quality is not really an issue here.(SDG6).

We appreciate this observation in particular, because groundwater quality is in fact an issue as the area is within a zone classified as vulnerable to nitrate contamination of the watertable as defined by the EU Nitrates Directive (Council Directive 91/676/EEC) (Council of the European Union, 2008). With this, and considering our results of a lower proportion of draining macropores under the optimized management strategy considered, we could infer that its adoption can also be an asset in terms of groundwater protection. However, we cannot be conclusive on this issue without a better understanding of the soil water dynamics.

3. Carbon capture is not evident as the % C is about the same for OPM and CM. This is interesting because OPM management is supposed to lead to higher % C and their results indicate this may apply to soils in humid regions ,as reported in literature, but not in arid climates. Effect of high temperatures? But.. data only are restricted to 30 cm depth. (SDG13).

The reviewer is right and generally, OPM management is usually associated with an increase in C storage compared to CM. Nevertheless, it has been seen that when tillage is suppressed and crop yields are maintained under OPM and CM, as in our study, C content does not increase (Virto et al., 2012). In addition, several studies show a gradual increase in SOC stocks under no-tillage, albeit differences between tillage systems become detectable only after several decades after conversion (Or et al., 2021; Angers and Eriksen-Hamel, 2008; Haddaway et al., 2017). This issue would be clarified in the discussion.

4. Even though the carbon content of both treatments is about the same, the biological soil properties seem to differ. Biodiversity in OPM is higher. This result is interesting because information on soil biology is often missing in other publications and could be part of the main text here and not be presented in an appendix. (SDG15).

Since our evaluation is mainly based on soil porosity analysis, we had included the information concerning the biological properties of the soil in an annex. But the reviewer's comment is very right. Thus, we would include a new section "Organic C storage and soil microbial diversity", just passing the information regarding biological issues from the supplementary material to the main text.

5. As bulk densities of the two treatments are about the same for 0-5 cm dept and for the 0-30 cm depth (how was the latter measured, we read only the method for 0-5 cm? Effect of high tempera) there seems to be no negative effect on soil structure by CM management which is usually assumed to take place. Big problem here is that analyses were only made to 30 cm depth and a plowpan may well form by CM management but usually occurs at 30 cm depth and deeper. Indeed, as the authors mention on line 320: deeper soil layers are needed. For a correct analysis soils should be analysed for the total rooting depth. (SDG15).

In fact, the bulk density values correspond only to the first 5 cm (0-5 cm). However, based on our field observations these values would be extrapolated up to 30 cm depth. At greater depths, a higher compaction was observed, especially in the soil under conventional tillage (CM). Nevertheless, the rest of the indicators were analyzed at 0-30 cm, as suggested by the reviewer and proposed for example by FAO for the organic C stock. All these would be clarified in the text (material and methods) and in Table 1 of our manuscript.

As already mentioned in the conclusions similar experiments at deeper soil layers are needed.

Also, in relation to SDG15, we have now considered in more detail the potential of the OPM system for erosion control, and will discuss it in the new version of the manuscript, by adding up-to-date information in this view:

"Changes in soil erosion rates depend among others on climatic conditions, land use patterns, farmers' decisions and, agri-environmental policies (Panagos et al., 2021; Mosavi et al., 2020; Grillakis et al., 2020; Eekhout and De Vente, 2020; Paroissien et al., 2015)

Recent work has shown that studies associated with soil erosion are essential, both for agricultural conservation practices and to subsidize environmental planning, where economic practices must be calculated under conservationist principles (Panagos et al., 2021; Cruz et al., 2019; Mosavi et al., 2020; Plambeck, 2020). In particular, selective application of cover crops in soil erosion hotspots, combined with limited soil disturbance measures, has been recently recommended as an effective measure for partially or totally mitigate the effect of climate change on soil losses in Europe (Panagos et al., 2021)."

Some additional notes:

1. **Line 45:** to study soil pores, morphological analyses are most useful, if only because different types of macropores can be distinguished: e.g. channels of roots or animals or cracks. Deriving pore sizes form moisture retention data is an indirect, approximate method. Later (lines 260, 266) micromorphology is mentioned. That should also be done upfront.

The reviewer is right, micromorphological analysis, as a (more) direct method of pore space characterization, should be mentioned in the paragraph starting on line 45. However, the use of thin sections limits the study of pores larger than the thickness of the film. This is usually 20 microns (Virto et al., 2013).

2. **Line 190:** no significant compaction? Could be deeper in the CM soil, see comment above.

This issue was treated and clarified above.

Line 319: infiltration at the surface is mentioned in the context of the pore analysis. But infiltration rates can be measured and this is very important for the Mediterranean environment, certainly when considering climate change where showers will become more intense. CM has more macropores, so the authors suggest that infiltration rates would be higher than in OPM with fewer macropores. But the CM soil is less stable so crusts may form rapidly, the more so since there are no cover crops and crop residues are removed. So just linking the occurrence of physically derived macropores to infiltration rates is unrealistic without measurements of such rates. In fact, the reasoning should be reversed: measure infiltration rates and then explain differences by looking at pore patterns.

This is true; it is rather speculative to directly relate infiltration to soil macroporosity. We will limit ourselves to the description of porosity, while indicating that it would be necessary to have infiltration measurements, ideally with controlled suction (e.g.,disc infiltrometer).

4. The authors conclude that more water is stored in the OPM treatment and this should be favorable for plant growth. But this is a statement based on static moisture retention measurements while storage is determined by in- and outflow from a certain soil volume, a dynamic process as is the moisture supply to plants. Numerous dynamic simulation models are available to quantify this process, that needs hydraulic conductivities of the soil. The authors seem to be unaware of modern soil physical theory.

The reviewer is right; we cannot be conclusive in this issue with our results. We could rephrase that sentence by saying that our results suggest a higher water storage capacity in OPM. However, to properly quantify water storage capacity inflow and outflow from the soil should be estimated. This issue would be clarified in the text; more precisely in the discussion (line 201) and in the conclusion (line 306).

5. As the soil index by Dexter does not provide any valuable results (lines 226, 317) it should receive much less attention and might as well be omitted.

We agree with the reviewer, we have decided that the Dexter index deserves less attention and we will not be mentioned in the objectives:

"The objective of this study was to assess the continuous application, throughout 18 years, of an innovative soil and crop management –in comparison with conventional management– for the improvement of the soil physical condition, and the optimization of the soil water balance, in rainfed cereal agrosystems in semi-arid land. This evaluation will be carried out through the analysis of detailed SWRCs and soil structure, i.e., the size-distribution of stable macro- and microaggregates and their relation to organic C storage."

It should be noted that the main strength of our work is the use of detailed SWRCs that allows a better and more reliable adjustment of any index based, precisely, on the different shape of these curves. In most of the published works in which the S-index was applied, the corresponding SWRCs were constructed from a small number (less than 10) of water content-suction values, obtained from sandbox and/or pressure plates apparatus. To evaluate to what extent the degree of detail of the SWRCs is relevant we have recalculated the van Genuchten' parameters and the S index considering only 9 water content-suction values –the most frequently used, according to the literature–. Not surprisingly, the values obtained differed markedly –with no apparent bias– from those reached using the full dataset (about 100 evenly distributed points).

Taking advantage of our continuous SWRCs and following the reviewer's #5 suggestion we have determined the water retention energy index (WRa) (Armindo and Wendroth, 2016) obtained from numerical integration including all the points of the SWRC. Needless to say, that the accuracy of this index is highly conditioned by the degree of detail of the SWRCs. Moreover, this index presents an adequate sensitivity for smaller-scale, high-precision applications and for capturing the dynamic evolution of the soil physical state (Armindo and Wendroth, 2016) (see more details in the answers to reviewer #5).

A possible new title might be: *Effects of innovative soil management on topsoil properties of a Mediterrenean soil*. This could focus on several interesting results identified above. Again, I would recommend they would frame their story in an SDG context.

As mentioned above, a new title is proposed based on the reviewer suggestion: *Effects of innovative long-term soil management on topsoil properties of a Mediterranean soil based on detailed water retention curves.*

The authors should realize that OPM is, in fact, a form of regenerative agriculture, studied in the USA. I recommend that they check with the National Soil Health Institute website. info@soilhealthinstitute.org.

Many thanks for your recommendation.

J.Bouma.

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