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.

First of all, thank you very much for your consideration and positive reaction. Your comments and suggestions will undoubtedly help to improve the manuscript.

Indeed, the area of study including both CM and OPM tested points is relatively small (3 ha). It is important to note that there is to our knowledge, no other agricultural field in the whole region of Navarre where soil and crop management as proposed herein is practiced, and even less for almost two decades, with the exception of our relatively small area where OPM was introduced by a pioneer farmer nearly 20 years ago. The uniqueness of our OPM area and then the 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).

There were 3 composite samples for each treatment (OPM and CM). Each of them consisted of three sub-samples randomly taken within the studied area.

Regarding the vague soil information, the reviewer is right, some of the soil information -clay, silt and sand contents- was originally included in the supplementary material of the manuscript. However, since this is a relevant information, it would be included in Table 1 along with the methods followed for the data determination. In addition, at the suggestion of reviewer #4, a statistical analysis of the parameters in Table 1 will be performed and those showing significant differences ($p < 0.05$) between treatments will be marked in bold.

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.

Our analysis is focused on the suction range between 10 kPa (field capacity) and ca. 100 kPa, i.e., what would be the *available* –for use by plants/crops– water storage capacity. Besides, 150 kPa is the maximum operating suction value of the Hyprop device. However, gravimetric moisture content (ϴ) at 1500 kPa was also measured using a pressure plate: 10.4 % and 10.0 % for OPM and CM, respectively.

We agree that the better soil stability that the soil under OPM shows should play an important role in soil erosion mitigation. Although our findings do not allow us to go beyond this basic statement, we have developed this idea in the discussion of the future new version, under the light of recent literature on soil erosion control in Europe, as follows:

"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)."

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.

The reviewer is right to avoid misinterpretation the title should be reformulated: *Effects of innovative long-term soil and crop management on topsoil properties of a Mediterranean soil based on detailed water retention curves.*

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.

Following the reviewer's advice, we have made a revision and have drafted the following paragraph to be included in the introduction:

"Conservation agriculture (CA), and other soil management strategies implying a reduction of tillage have been reported to reduce soil degradation in different agroecological situations (Verhulst et al., 2010; Sartori et al., 2022), and in some cases are designed for this purpose (Virto et al., 2015).

The reasons reported for its adoption in Europe are several. In Northern Europe soil erosion control, soil crusting in loamy soils and the need to increase soil organic C storage, as well as soil trafficability are widely cited as reasons for CA implementation (Lahmar et al., 2007). In the Mediterranean countries, soil water storage and water-use efficiency can be added to this list of reasons (De Turdonnet et al., 2007). However, different studies show that the effectiveness of CA in solving these problems can be site-dependent (Costantini et al., 2020; Chenu et al., 2019). In fact, the most widely reported benefits of CA in Southwestern Europe in relation to erosion are the increased soil infiltrability and/or the protective effect of crop residues on the soil surface (Gómez et al., 2009; Espejo-Pérez et al., 2013; Virto et al., 2015), although this seems to be related to the type of soil and to the presence and activity of earthworms. In Spain, the soil water-retention capacity has been observed to be greater in semi-arid land under no-tillage (Fernández-Ugalde et al., 2009; Bescansa et al., 2006).

Other positive effects of CA on soil quality observed in semi-arid rainfed agricultural systems in Spain are related to soil organic C and nutrients storage (Ordóñez Fernández et al., 2007)".

We believe it is appropriate to add the following comments about our soil and crop management proposal (OPM). As the reviewer is well aware, the crop performance under no-till is strongly dependent on the crop type and climate (Pires et al., 2013; Or et al., 2021) and also soil type. Then, no-till may not be suitable for all conditions (Pittelkow et al., 2015). In fact, conventional tillage -in carefully managed agricultural soils- may be imposed when no-tillage would lead to chronic and unacceptable yield losses (Or et al., 2021). Indeed, no-till often results in reduction in crop yields of ca. 10 % in some areas (Or et al., 2021). Although we do not have precise data on crop yields in both treatments (CM vs OPM) some rough data are available from the farmers managing the fields (see Table 1 below). From the data provided by the farmers, we can see that our proposed soil and crop management (OPM) is not inferior to the CM in terms of crop yields and then it seems to be indeed suitable for our local (Mediterranean) conditions.

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

The uniqueness of our pioneer OPM area, and then the relevance of our work should be better and clearly pointed out in the text, especially in the Introduction.

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

We would do our best to improve the readability of the MS after incorporating all the modifications and suggestions of all the reviewers.

Specific points

L105: when organic fertilizer was applied in OPM?

The text will be reformulated to include more information about organic fertilization:

"In both treatments, mineral fertilization consisted of phosphorus addition before seeding (120-150 kg·ha^{-1} of triple superphosphate 0-46-0) and nitrogen supply of 180 kg N·ha^{-1} (split and distributed into two cover dressings at 60 kg N·ha^{-1} and 120 kg N·ha^{-1} in January and March, respectively) as urea. Organic fertilization was not used in any of the study treatments until 2021, in which an organic amendment was applied to the soil without disturbing the surface in the OPM treatment. After harvest, pig slurry was applied with an average concentration of 2.5 kg N·m^{-3}, by means of a tanker equipped with a system of hanging pipes that deposit the product a few centimeters above the ground and at a time close to a forecasted rainfall event. The application rate was 60 m^{3}·ha^{-1} of slurry. These rates are within the legal limits established by legislation for groundwater protection against pollution caused by nitrates from agricultural sources (EU Directive 91/676 (Council of the European Union, 2008)), as the area is within a vulnerable watershed according to this Directive."

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.

The reviewer is right, granulometry –originally included in supplementary material– was added to Table 1.

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

Yes, the inflection point is the second derivative of the function

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?

In fact, the technique used is well documented in previous publications cited in the text (mainly Oliveira et al., 2019), so we prefer not to modify this section and to refer the reader to those publications for further details.

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

As commented above we focused our analysis in the available water storage which is between around 10-100 kPa. However, the gravimetric water content at wilting point in both treatments – measured using a pressure plate– is roughly the same: 10.4 % and 10.0 % for the OPM and CM, respectively. We cannot go further in this analysis since there is no measurements points between 100-150 kPa and 1500 kPa (wilting point).

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

In fact, the variation in moisture content corresponding to the infection point obtained with the van Genuchten equation (Table 2) is only around 2 %. This is true that this difference is higher for the sigmoidal equation adjusted to the experimental data (Table 3) because this approach presented a much higher dispersion both in S values and in the corresponding moisture content as pointed out in the text.

Section 3.3: how the porosity spectrum established?

As mentioned in the material and method the soil pores size-distribution was estimated from the equivalent radius obtained from the suction values of SWRCs by Jurin's law (1718) (equation 2).

L266-268: rephrase

L276-279: not clear

L280-283: rephrase

Sorry, the three sentences will be written more clearly.

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

As commented above we focused our analysis in the available water storage which is between around 10-100 kPa.

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

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).

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).

We agree, we will rephrase this paragraph as follows:

“In summary, our results suggest that this type of system that includes, among other techniques, the suppression of tillage, crop rotations, and the application of organic amendments, is providing good results from the viewpoint of the physical quality of the soil, and can be recommended for a higher soil sustainability in Mediterranean agrosystems. However, the optimized management analyzed herein is not currently a widespread practice in the region, most likely because the high initial investment and the farmer’s concern that crop yield would be reduced. This work illustrates the need for an adequate assessment and dissemination to overcome these reluctances of farmers and other potential barriers to the adoption of this type of systems.”

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.

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