General comments:

The purpose of the manuscript is a simple comparison of the influence of two management systems, conventional cropping and direct-drilling rain-fed cropping in the physical properties of a soil in the north of the Spain. The selected physical properties are the S index of Dexter (2004) and the size distribution of the stable aggregates. Neither the objectives nor the methods represent a new contribution to the fields of Agronomy or Soil Science.

Nevertheless, the relevance of reduced tillage systems for the Mediterranean countries, and the long duration of the field trials, deserve an opportunity for the authors after a thorough revision of the manuscript. Some recent contributions as, for instance, Or et al. (2021) could be inspiring for such a revision.

First of all, thank you very much for the helpful comments and suggestions you have made about our manuscript.

By re-reading our own manuscript after the reviewer comments, we realize that we did not properly clarify certain important points necessary to better understand the relevance that we sincerely believe our work has.

We evaluate more than just a direct-drilling but an innovative soil and crop management system (OPM) in Navarre (Mediterranean conditions). Moreover, there is to our knowledge, no agricultural field in all 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.

As the reviewer is well aware, the crop performance under no-till is strongly dependent on the crop type and climate (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); in fact, 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 0 below). From the data provided by these farmers we cannot assure that the OPM treatment was better than the CM in terms of yield. In fact, there is usually no difference between the two treatments because the limiting factor is mostly water. Even so, the data suggest that farmers will not suffer a decrease in the yields of their fields by implementing sustainable practices, i.e. OPM.

Table 0. 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 and in section 2.1. (Study site [former 'zone'] and treatments).

On the other hand, another key issue in 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 suggestions 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).

The Introduction section does not contain a comprehensive, updated perspective of the conservation agriculture and the quality of the soil. Consequently, the objectives, (lines 77-82) are very imprecise.

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)".

The Material and methods section is rather incomplete, with some inaccuracies that will be commented later. The climate properties of the study zone are missing. The description of the soil is limited the mention of the subgroup in the Soil Taxonomy scheme, the textural class of the upper soil horizon and Tables 1 and A1. No explanation is given in the text of the methods followed for the determination of the data of Table1. However, the details of the soil water retention in subsection 2.2 and of the aggregate size fractionation in subsection 2.4 are excessive including Figure 1.

The reviewer is very 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 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. This can be summarized in a paragraph that would be included in the new version, as follows:

"The physical-chemical analysis of the soils shown in Table 1 was done using standard methods. In particular, soil pH was analyzed in a 1:2.5 soil:water solution as in Hendershot and Lalande (1993), organic C content by wet combustion as in Tiessen and Moir, (1993), carbonates were determined in a modified Bernard's calcimeter following Pansu and Gautheyrou (2003a), and the electrical conductivity in a soil:water solution similar to that for pH analysys (Pansu and Gautheyrou, 2003b). The soil texture was determined by the pipette method. All analyses were conducted on air-dried samples ground to 2 mm. Finally, the bulk density was determined using the Hyprop device (see below) from undisturbed samples extracted from the first 5 cm of the soil profile.

Table 1. Physical-chemical properties of the topsoil (0-30 cm) in OPM and CM treatments and the textural characterization of both treatments. Mean ± standard deviation of the mean (n = 3). Statistically significant differences ($p < 0.05$) are in bold."

Treatment	**Optimized (OPM)**	**Conventional (CM)**
Bulk density (0-5 cm) ($g \cdot cm^{-3}$)	1.26 ± 0.05	1.26 ± 0.15
pH	8.00 ± 0.05	8.01 ± 0.01
Organic C (%)	**1.80 ± 0.10**	**1.51 ± 0.14**
CE ($\mu S \cdot cm^{-1}$)	**483 ± 5.66**	**795 ± 4.24**
Carbonates (%)	**31.6 ± 0.09**	**32.5 ± 0.14**
Sand (Coarse) (%)	5.05 ± 0.08	5.79 ± 0.33
Sand (Fine) (%)	30.9 ± 1.00	31.7 ± 1.25
Silt (%)	47.2 ± 1.23	43.7 ± 0.93
Clay (%)	**16.9 ± 0.46**	**18.5 ± 0.46**
Texture (USDA)	***Loam***	***Loam***

On the other hand, as indicated by the reviewer, a brief description of the climatic properties of the study area will be included in the Material and Methods section:

“This is an area with a dry temperate Mediterranean climate, according to Papadakis (1967). The mean annual precipitation is 550 mm year^{-1}, and the Thornthwaite mean annual evapotranspiration is 711 mm year^{-1} (Gobierno de Navarra Meteorología y Climatología de Navarra, 2022).”

The Results and discussion section is incomplete as well. The authors have chosen the van Genuchten soil water retention equation, but, in addition to the absence of the fitted values of its parameters in the text, one misses some consideration of other alternatives as, for instance, the multimodal equations, which according to Jensen et al. (2019), can reflect better the effects of management systems in the soil properties. I have expected some changes in properties like the bulk density, and the presence of some surface crusts, but, apparently, they were not found. The Figure 3 could not give an adequate information on the size distribution of the pores since it is estimated from the soil water retention curve through equation (2). The discussion of the results is fragmentary.

Following the reviewer suggestions we plotted our experimental results as differential functions [dϴ/d(log h) vs log h(h] seeking for a multimodal behavior: all the curves analyzed were of the uni-modal type. But it should be noted that suction values do not exceed 150 kPa and according to Dexter et al. (2008) (cf. their Fig. 3) and Jensen et al. (2019) (cf. their Fig. 2) findings the second peak defining a bimodal behavior seems to appear at suction around 1000 kPa. Then, we tried again incorporating to the dataset the water content-suction measurements at 1500 kPa (obtained using pressure plates) with the same result, i.e. unimodal behavior. But this could be an artifact of the dataset since there is a wide experimental gap between 150 kPa and 1500 kPa, i.e. no measurements in between.

Despite this, we tried the double-exponential equation for soil water retention proposed by Dexter et al. (2008) (Eq.1) with our experimental results:

$$\theta = C + A_1 e^{\left(-\frac{h}{h_1}\right)} + A_2 e^{\left(-\frac{h}{h_2}\right)} \quad (1)$$

Where *ϴ* is the gravimetric water content; *C* is the residual water content (asymptote of the equation); the amount of matrix and structural pore space are proportional to A_1 and A_2, respectively. The values of h_1 and h_2 are the characteristic pore water suctions at which the matrix and structural pore spaces empty, respectively (Dexter et al., 2008).

Table 2 shows the values of the fitted parameters of equation (1) using our experimental dataset. Unexpected at first sight, the structural pore space would have been reduced by 55 % as a result of no-tillage (OPM) (cf. A_2 values, Table 2), while the matrix pore space values remain rather constant in both treatments (see A_1 values in Table 2). This could be explained by an increase in soil fragments rather than soil aggregates in the CM treatment in comparison with OPM treatment. Soil aggregates and fragments may look similar but are formed by different processes and have different properties (Or et al., 2021): soil fragments form by mechanical forces of tillage; they tend to be mechanically weak and coalesce upon wetting with macroposity collapsing within a single season. Instead, soil aggregation is stimulated by biological activity with biopolymers and hyphae that stabilize and bind soil particles.

Table 2. Average values of the fitted parameters of the double-exponential water retention equation by Dexter et al. (2008) obtained with the experimental dataset.

Treatment	Parameters of the Dex model					
	C	**A_2**	**h_2**	**A_1**	**h_1**	**RMSE**
	$m^3 \cdot m^{-3}$	$m^3 \cdot m^{-3}$	hPa	$m^3 \cdot m^{-3}$	hPa	$m^3 \cdot m^{-3}$
OPM	0.25	0.06	29.94	0.11	865.08	0.005
CM	0.2	0.10	25.16	0.11	737.43	0.003

All this is consistent with our previous results as described next. The fast desorption rate at low suction observed in CM compared with OPM (cf. Fig 2 in our manuscript) could be the result of macropores due to large soil fragments induced by tillage. However, more stable (biologically-formed) macroaggregates were observed in the OPM treatment compared with CM (cf. Fig 4 in our manuscript).

But all the above should be taken with caution since, as aforementioned, our SWRCs do not show a bi-modal behavior and then the use the Dexter's et al. (2008) equation -and what is inferred therefrom- could be questioned.

Regarding Figure 3, to avoid misinterpretation, it should be clarified in its caption that pore sizes are estimated from equation (2).

<u>Specific comments</u>

Line 28: what is 'the studied depth'?

Thanks for showing this ambiguity: the studied depth is the first 30 cm of the soil profile. This sentence should be reformulated.

Lines 51-53: The definition of the soil water retention is very imprecise. Why soil water retention 'is mainly associated with the porous system' 'at low suctions'?

Sorry, that sentence is indeed ambiguous, it should instead read:

"Soil water retention curve (SWRC) is the relationship between soil water matric potential and soil water content. Since water suction/retention is largely determined by pore sizes SWRCs are a valuable tool to diagnose the physical condition of soils (Dexter, 2004a, b; Pires et al., 2017)."

Line 56: The treatment of the air entry state is, again, imprecise. The term 'so-called' is unnecessary.

We agree, the term "so-called" would be eliminated.

Lines 67-70: This paragraph is unclear.

Sorry, it is indeed unclear, it should be reformulated as follows:

"The inflection point can be determined directly by hand from the SWRC if there are enough accurate measurement points (Dexter, 2004a). Alternatively, it would be more appropriate to fit the SWRC to a mathematical function and then to calculate the slope at the inflection point in terms of the parameters of the function. To do this, one of the best known functions is that proposed by van Genuchten (1980) for which, in turn, pedo-transfer functions are available for estimation of its parameters (Dexter, 2004a)."

Lines 71 and 77: The two sentences are almost repeated.

This is true, thanks. The objective should be rewritten as the reviewer suggests; including also a modification proposed by the reviewer #1, i.e., do not highlight the use of the Dexter's index:

"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."

Lines 88-90: A more complete description of the soils should have been very helpful to understand their behavior. One cannot trust 'the visual inspection' to affirm that the soil is homogeneous. As in the line 28, could 'the study depth' be defined?

As mentioned above, Table 1 giving the main properties of the soil would include more information previously included in the supplement materials.

Regarding the low reliability of a visual inspection, in fact, for visual inspection we actually mean field soil description following standard procedures (i.e., penetration resistance, soil structure, consistence, etc). Anyway, to give less relevance to the in situ soil description this sentence could be reformulated as follows:

"The physical-chemical analysis of soils (Table 1) showed high homogeneity of the soil at the study depth (0-30 cm) regarding the most relevant physical-chemical properties related to moisture retention, except for the content of organic C (which can be related to the change in management). In addition, in situ standard soil description corroborated the homogeneity of the topsoil (0-30 cm)."

Line 105: The term 'coverer' is very odd.

The sentence has been reformulated to avoid the use of the term "coverer", and to provide more accuracy about the fertilization timing:

"In both treatments, mineral fertilization consisted of phosphorus addition before seeding (120-150 $kg{\cdot}ha^{-1}$ of triple superphosphate 0-46-0) and nitrogen supply of 180 $kg\ N{\cdot}ha^{-1}$ (split and distributed into two cover dressings at 60 $kg\ N{\cdot}ha^{-1}$ and 120 $kg\ N{\cdot}ha^{-1}$ in January and March, respectively) as urea."

Table 1: The evaluation method must be indicated in the text and the relevant details like the soil-water ratio for the suspension in the measurement of the pH and of the electrical conductivity.

As mentioned above, Table 1 was completed including this piece of information.

Lines 132-133: To establish the relationship between the gravimetric and volumetric water contents, one only need to know the bulk density of the soil. The sentence is confusing.

The sentence is indeed confusing, sorry. It would be reformulated as follows:

"Gravimetric water content can be expressed as volumetric content since bulk density is known"

Lines 142-145: I could not find equation (1) in the article of Dexter (2004a). In fact, I cannot find the relation of this equation with the van Genuchten (1980) equation.

It is true that the phrase is a bit confusing. It will be clarified in the text which is equation (1) and which is equation (2):

The value of S was calculated in two different ways: i) from a sigmoidal function fitted to experimental data (Eq. 1), and ii) from the adjusted parameters of the van Genuchten (1980) function (Eq. 2) (Dexter, 2004a):

$$y = \frac{a}{1 + e^{-\left(\frac{x - x_0}{b}\right)}} \qquad (1)$$

Where y is the logarithm of suction (hPa), x is the gravimetric moisture ($kg \cdot kg^{-1}$), and a, b, x_0 are parameters of the equation

$$\theta_h = (\theta_{sat} - \theta_{res})[1 + (\alpha h)^n]^{-m} + \theta_{res} \qquad (2)$$

Where h is the soil matric potential (hPa), θ_h ($m^3 \cdot m^{-3}$) is the measured soil water content at matric potential h, θ_{res} is the residual water content ($m^3 \cdot m^{-3}$), θ_{sat} is the saturated water content ($m^3 \cdot m^{-3}$), α (hPa^{-1}), n (-) and $m = 1 - (1/n)$ (-) are the van Genuchten parameters.

Lines 150-154: The equation was not proposed by Jurin (1718) as one can check reading such an article. Jurin described his observations in that contribution, but the equation was later formulated by Young and Laplace. This equation is usually known as the Young-Laplace equation (e.g. Adamson, 1967, § I.10).

Thank you very much for the clarification.

Line 154: Is the (soil water) potential is mentioned, the sign should be negative. The term ‘suction’ is more appropriated here if the symbol ‘h’ has been used for the ‘height’ in the line 152.

The reviewer is right. We will replace the term "potential" with "suction”.

Line 195: The term ‘water capacity’ was already defined, at least, by Arnold Klute in 1952.

Thank you very much for the clarification.

Line 196: How ‘field capacity’ is defined in the text? This term must be precisely defined.

The moisture content corresponding to the inflection point of the hydraulic conductivity vs. moisture content curve -provided by the Hyprop device- would correspond to the field capacity. This should be better defined in the text as the reviewer indicates.

Table 2: as indicated above the soil water potential is negative.

In fact, soil suctions (positive values) are mentioned in Table 2 and not water potential.

Lines 231-233: If the authors are using a structural index, they should not compare the texture but the structure of the soil.

What we are trying to explain in this sentence is that the inflection point of the SWRCs in both treatments would not be affected by the texture since this does not change between treatments. For instance, the S value tends to decrease with increasing clay content (Dexter et al., 2004).

Line 306: The term 'capillary/available water' is misleading.

We agree with the reviewer. The term "capillarity" will be deleted and "available water" will be maintained.

Line 322: I do not think that tillage should be 'suppressed'. The natural consolidation of the soil surface might be alleviated by occasional shallow tillage operations.

The reviewer is right, the term "suppressed" should be replaced by "minimum tillage" since although up to now the OPM plot has not been ploughed, it may be necessary to eventually do that to avoid excessive compaction.

Finally, we agree with the following group of suggestions proposed by the reviewer and will include them in the text:

Specific comments

Line 18: instead of 'unit' it must be written subgroup.

Lines 25 and 26: 'significant differences' must be replaced by a statistic parameter.

Lines 33-38: The paragraph should be rewritten to improve its comprehension.

Lines 45-47: The sentence is very similar to that of the lines 37-38.

Line 85: for the sake of precision write 'subgroup' instead 'type' and mention the Soil Taxonomy.

Line 86: The textural class should be silt loam, according to the particle size information of Table A1, where it was correctly indicated.

Line 129: Use 'matric component of soil water potential' instead of 'hydric potential' to be more precise.

Technical corrections:

Line 54: Writing 'saturated' soil is enough.

Lines 146-147: The use of the acronym 'SUHC' is needless.

Lines 160 and 364: The correct name is Elliott.

Line 280: The Table reference is A1, not 'S1'.

Line 235 and caption of Figure 3: The proper adjective is logarithmic.

Incomplete references: Lines 366, 369-370, 373-374, 381, 387-388, 391, 407, 419.

The reference of lines 441-442 is not mentioned in the text.

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