The manuscript is dedicated to the estimation of the influences of a cover crop treatment on some physical properties of the soil in a 10-year field experiment. The research subject is very interesting to assess the convenience of a soil and water conservation practice in a semiarid area.
Thank you very much for this comment.

Overall quality
Nevertheless, the manuscript contains some problems that require a thorough revision. The authors, instead of exploring their field data, apparently prefer to extract some information of the soil water retention curve and of the saturated hydraulic conductivity through inverse methods fitting a hydrological model coupled to those data.
I think that the quality of the raw data is always greater than the use of the output of any model, in particular if the this model systematically predicts a lower ground cover or greater biomass than the corresponding observed values, as Figure 2 indicates.
We also agree with the statement that raw data are always more interesting than simulated ones. Yet, the interpretation of the raw field data were already presented in previous publications dealing with measured soil properties after 10 years (García-González et al. 2018) and crop growth (Gabriel and Quemada 2011). In the present paper, we want to go a step further as compared to the results presented in the paper of Garcia-Gonzalez and try to find a method able to provide information about changing hydraulic parameters directly from in-situ soil water content measurements, and not from direct measurements of these parameters. Implementing inverse modelling based approaches as done in the present study offers many advantages as compared to parameter determinations based on direct methods, in particular for inferring hydraulic properties at larger depths for which direct methods are hard to implement.

There are other problems with the interpretation of soil physical parameters as indicated below.

Specific comments
The results shown in Figure 2 can give an 'acceptable' fit, (line, L, 12, page, P, 5), but the data points of the figure are either below, case of ground cover, or above, case of biomass in the two fist plots of the figure. This trend could affect the results.
We agree with the reviewer. To consider this effect, we considered a trend for each crop parameter. We also add some comments on this in the revised manuscript.

The time variation of the optimized values of the soil physical parameters shown in Figure 3, can be related to some environmental conditions. As indicated in a previous review this was the case of the van Genuchten soil water retention equation parameter, in the surface layer, 0-20 cm, for the barley plots. This parameter normalizes the matric component of soil water potential,Ψ, in brief matric potential. Using the information of figures 3 and 4, I have plotted in figure 1, below, the optimized value of the parameter α against the annual rainfall, P. The decreasing trend is evident. Why the parameter can change with the annual rainfall? In principle the parameters of the soil water retention equation do not depend on the rainfall, but the observed relationship of the α parameter with the annual rainfall is more evident than its relationship with the time. The explanation of the manuscript, 'large particle transport (clay transport)' within the soil (L 25-26, P10) is doubtful. Could it be an artefact of the optimization method?
The reviewer is strongly acknowledged for this excellent remark. Indeed, there is a correlation between rainfall and α, but there are also correlations with other variables such as with evaporation (R^2=0.79) or barley biomass (R^2=0.57). An artefact of the optimisation model cannot be excluded, but in this case other parameters should also be impacted (or at least more depths). This has not been observed systematically. We therefore attribute this to a real physical process. It should be noted that the model is

no very sensitive to α, so that uncertainty will be larger as compared to the determination of other parameters such as n.
We have included new explanations on this in the manuscript.

The alpha parameter is linked to the other parameters m and n, in the original equation of van Genuchten (1980), relating the saturation degree of water in the soil, S, for any water content, θ, with the residual, and saturated water content, θ_r , and θ_S, respectively, with the matric potential. Van Genuchten (1980) suggested a relation between the m and n parameters to get a closed form equation in the relation between the hydraulic conductivity of the soil and the degree of saturation. Kosugi[1] found a further relationship between the parameters α, m and n for the inflection point of the water retention curve, which, with the help of equation 3, characterizes the value of the matric potential at this point, Ψ_{IP}.
The equation 4 can explain the observation of L 29-32 P 10 of the manuscript.
We agree with this and we have included some information on this in a new sentence

Why the water balance analysis of the section 3.3 has not been based on the raw data, as, for instance, Palese et al (2014) did? The results of the Figure 4 induce several questions not answered in the text.
As said before, many of the raw SWC data have been a discussed in previous publications, and the main goal of the manuscript is to assess the medium-term effect of cover crops on soil hydraulic properties with a new non-destructive technology.

The evolution of the simulated drainage volume of both treatments is roughly parallel, with greater volumes lost from the bare soil treatment than from the barley one. However, during the year 2009-2010, a great difference of drained volume, more than 150 mm, between both treatments was observed: the barley treatment lost less water by drainage than the bare soil treatement. Why? Examining the simulated evapotranspiration plot, one could think that the not drained water was evapotranspirated by the barley crop. If this hypothesis correct?
It is correct, the barley production in 2009/10 season was more than twice the production of the 2008/09 season. Hence, evapotranspiration was much higher than in the fallow treatment.

The manuscript does not inform on the slope of the ground. Was any runoff observed in the plots?
The field experimental site is quite flat and runoff was never observed. We have included a new sentence on this.

Another aspect that deserves some attention is the proportionality between the measured annual rainfall and the simulated evapotranspiration from the barley treatment, very patent in a simple visual inspection of the plots, and more clearly shown in the Figure 2 above. Is there any reason for the apparent proportionality between the annual values of precipitation and simulated evapotranspiration?
In Mediterranean conditions, water is the most limiting factor for barley growth. As far as there is water available, the barley grows at the potential rate and transpires at a rate close to the potential evapotranspiration rate. But if there is not water (rainfall), actual ET is reduced to the amount of water available. The linear proportionality between available water and crop transpiration has often been introduced in crop water stress modelling concepts. We have included a new sentence on this in the manuscript.

With respect to the method of estimation of the evaporation from bare soil, the manuscript indicates that it was computed by the 'multiplication of the reference evapotranspiration by 1' (L18 P5). If I am interpreting this indication correctly it implies that the soil was losing water to the atmosphere without any internal restriction, the stage I of soil water evaporation[2]. Is this interpretation correct? In the affirmative case, why?

No, its not. The definition was not good enough. This module only provides to WAVE a potential evapotranspiration but the model corrects this value according to the actual soil water content for an actual evapotranspiration. Because of that, in the water balance, the evaporation and transpiration are highly correlated to rainfall.

A new sentence has been added in the manuscript to clarify this.

Technical comments

Certain terms in the manuscript are imprecise, as 'water availability' written in L23 P1 and in L2 P11, and not defined until L 8 P11.

It has been corrected.

The field capacity was estimated following Assouline and Or (2014) suggestions in L13 P3, but this estimation seems forgotten in L8 P11, when the value 33 kPa is adopted without any further explanation. Why?

In fact we used Assouline and Or (2014) as reference for defining the concept of field capacity. But as long as now it has been defined in the introduction we can remove this reference.

What was the purpose of the measurement of saturated hydraulic conductivity in the laboratory of indicated in L9 P4?

As we said at the end of the paragraph, 'These results provided a range in which soil hydraulic property values estimated by inverse modelling should be included.'

The manuscript needs a revision to repair some formal defects. For instance, the sentence in L9 P10 is repeated in L10 P10.

It has been corrected

Some sentences reiterate certain terms like 'parameters' in L 29-35 P2.

It has been corrected

The sentence in L 22-23 P 6 is obvious.

With this sentence ('The 10-year weather series considered in this study represents the diversity of weather situations that may occur under these Mediterranean conditions.') we want to express that these 10 year are enough to represent most of the climatic possibilities. We have corrected for a better understanding.

Additional references

1 Kosugi, K. 1994. Three-parameter lognormal distribution model for soil water retention. Water Resour.
Res. 30:891-901.

2 Or, D., Lehmann, P., Shahraeeni, E., Shokri, N. 2013. Advances in soil evaporation physics. A review. Vadose Zone J. doi:10.2136/vzj2012.0163.