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Dear Dr Materechera,

We are very pleased by the good feedback of our manuscript. We have carefully responded in detail to the questions and concerns raised by the two anonymous reviewers in their corresponding response letters. Their insightful recommendations have been fully taken into account in the new revised manuscript.

The responses to each of the comments made by the two reviewers are detailed point by point below.

Sincerely,

Alaitz Aldaz-Lusarreta and co-authors.

Report #1

Future studies should take a dynamic approach to soil water regimes applying widely available dynamic simulation models of the soil-water-atmosphere-plant system. Also soil sampling should extend over the entire rooting zone and not be limited to the topsoil.

Thank you very much for your helpful suggestions for future studies. In the conclusions of the revised manuscript, the need to take a dynamic approach to soil water regimes by using mathematical models is stated. As the need to expand the sampling area to the entire rooting zone was already indicated in the previous version of the manuscript, we have included the need for dynamic approaches in the same paragraphs, which will read as follows in the new revised version:

"Further analysis at deeper soil layers –at least to the rooting depth– are necessary for a more complete assessment of the proposed optimized management. Moreover, to better understand changes in the soil hydrology, it is necessary to carry out experiments to determine infiltration rates, preferably under controlled suction. Finally, future studies should take a dynamic approach to soil water regimes by taking advantage of the widely available dynamic simulation models of the soil-water-atmosphere-plant system."

Report #2

The authors have made a great job revising the first version of the manuscript, which has been improved, and I appreciate their effort.

Many thanks to the reviewer for the good feedback on our manuscript and for these new insightful comments.

Nevertheless, there are still some aspects that demand a further revision:

1. The description of the experimental site is better than the previous one. However, the use of more widely accepted climate type such as Koppen-Geiger, (e.g. Peel et al. 2007), could be more helpful for the readers than the Papadakis scheme mentioned in line 91.
   I would not recommend the use of the Thornthwaite model for the estimation of the potential evapotranspiration, line 92, since this model infer the net radiation from the air temperature what causes important deviations of the measured values, in particular in semiarid areas (e.g. McMahon et al. 2013).

As suggested by the reviewer, in the revised manuscript the climate type and potential evapotranspiration are defined using the Koppen-Geiger and FAO Penman-Monteith models, respectively (lines 91-93):

"This is an area with a Csb type of climate according to the Koppen-Geiger classification (Gobierno de Navarra Meteorología y Climatología de Navarra, 2022; Peel et al., 2007). The mean annual reference evapotranspiration according to the FAO Penman-Monteith method is 1107 mm·year^{-1}. For crops in the rotation, the mean annual crop evapotranspiration is 326 mm·year^{-1}."

2. The choice of the Water retention energy index of Armindo and Wendroth is correct. Nevertheless, the selection of the values of integration limits in equation 4 is rather questionable. The estimation of the moisture content at the state of field capacity, the upper limit of the integral, must be justified. The authors state that the moisture content at field capacity correspond to the value at the inflection point of the soil water retention curve in lines 213-214. Why? There are several proposals for the estimation of this moisture content but almost all of them have been justified. Therefore, the authors must explain their decision, considering not the limitations of measuring method, (lines 204-206), but the physical meaning of the limit in the water retention energy concept.

   Similarly, the moisture content at the permanent wilting point needs another justification. As Czyz and Dexter (2012) indicated, and experimental data confirm (e.g. Wiecheteck et al. 2020), the conventional permanent wilting limit, was defined by the constraints of the usual measuring methods, not by the plant response. Again, the authors must properly justify the selection of both limits of the water retention energy method.

We thank the reviewer for these detailed observations of our use of this index, as it has allowed to observe and improve some aspects in this part of the manuscript:

- Regarding the determination of the moisture content at field capacity, it corresponds in really to the value at the inflection point of the hydraulic conductivity vs soil water content curve and not to the inflection point of the SWRC, as ambiguously stated in the original manuscript. We thank the reviewer for finding this misleading ambiguity. We have corrected the error in the revised manuscript (line 220).

- The reviewer concern about the conventional wilting limit is relevant since it is true that the wilting point lacks a universal physiological –in terms of plant response– basis. We decided to use the classical concept of permanent wilting point at a suction of 1500 kPa as it is widely used in the literature, and then to facilitate comparisons with previous and future publications. However, as stated by Wiecheteck et al.'s (2020) when comparing the classical permanent wilting limit with the biological wilting of wheat and barley suggest that wilting depends on soil texture, with an occurrence of wilting at lower suction (i.e., wetter soil conditions) for sandy soils than for clay soils. This clarification was incorporated into the manuscript (line 160). In any case, a more in-depth discussion on the determination of more realistic values for this parameter is beyond the scope of this paper

3. The ‘accumulated frequency’ of pore size in line 314, caption, and ordinate-axis legend of Figure 3, is what in statistical terms is mentioned as probability distribution function of pore size.

Many thanks for the clarification. We corrected it in the revised manuscript (see Fig. 3 caption and ordinate-axis legend as well as line 318).

4. If the plot of the probability distribution function of Figure 3 uses a logarithmic abscissae-axis, why not change the suction-axis of Figure 2 from arithmetic to logarithmic?

In Figure 2, the limited range of suction values –from 0 to ~110 kPa– does not justify the use of a logarithmic scale, compared to the wide range of values in the abscissase-axis. Then, we prefer to keep the arithmetic scale in this axis in the figure.

5. I keep thinking that figure 1 is not necessary in the manuscript. A simple reference to the original one if Olivera et al. (2019) could be sufficient.

The reviewer is indeed right, we now realize that Figure 1 is rather unnecessary: we will therefore eliminate Figure 1 in the revised manuscript.

References

Gobierno de Navarra Meteorología y Climatología de Navarra: http://meteo.navarra.es/climatologia/selfichaclima.cfm?IDEstacion=81&tipo=MAN, last access: 30 August 2022.

Peel, M. C., Finlayson, B. L., and McMahon, T. A.: Updated world map of the Köppen-Geiger climate classification, Hydrol. Earth Syst. Sci., 11, 1633–1644, https://doi.org/10.5194/hess-11-1633-2007, 2007.

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