

Interactive comment on “Long-term water stress and drought monitoring of Mediterranean oak savanna vegetation using thermal remote sensing” by María P. González-Dugo et al.

Anonymous Referee #1

Reviewer comments are typed in black colour, whereas the responses are typed in blue colour.

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General Comments The manuscript present an interesting study using a long-term dataset to characterize the impact of water stress on the dehesa region of Spain. Overall, study was well designed, the paper is well written, and the results and conclusions are fully supported. however there are a few aspects of the study that need some clarification. The concerns, along with handful of minor grammar and typographical errors, are noted below.

We thank the reviewer for the constructive comments. We have considered all of them, the suggested changes and clarifications are detailed here and will be introduced in the revised manuscript.

Specific Comments

1. Line 13: The sentence beginning "Drought is a ..." might be expressed more clearly as : "Drought is a devastating natural hazard that is difficult to define, detect and quantify."

The sentence will be changed

2. Line 13: The sentence beginning "Global meteorological data ..." is oddly constructed. It might be more clearly expressed as" The increased availability of both meteorological and remotely sensed data provides an opportunity to develop new methods to identify drought conditions and characterize how it changes over space and time."

The sentence will be changed

3. Line 26: The sentence beginning "During the drier ..." is unclear and needs revision.

The sentence will be changed to: “During the drier events, the changes in the grasslands and oak trees ground cover allowed a separate analysis of the strategies adopted by the two strata to cope with water stress”.

4. Line 34: The sentence beginning "Drought is a ..." could be expressed more clearly if constructed as: "Drought, which is both a devastating natural hazard and globally widespread, has complex consequences across spatiotemporal scales and sectors."

The sentence will be changed to the proposed construction.

5. Line 43: Replace "slow-onset nature" with "slow onset".

It will be replaced

6. Line 48: Indicators of what?

Indicators of drought, it will be clarified in the manuscript.

7. Line 53: The sentence beginning "LST and VIs" reads oddly. The authors seem to be saying that by combining information about the surface temperature and vegetation, remote sensing-based models can provide accurate estimates of ET. But, rather than stating that explicitly, the coach it in terms of vegetation indices etc.

The sentence will be changed to 'LST and VIs have been used to provide ET estimations over agriculture ...

8. Line 115: This paragraph is a bit unclear. The authors state the parameterization of green vegetation fraction and height are unique for the dehesa. Are the authors back calculating the leaf area index (L) using equations 8 & 9? If so, why? Also, there is no discussion of canopy height and how it's calculation is modified to better represent the dehesa.

Yes, we obtained F_c using eq. 8 and L is derived from F_c using eq.9. To clarify the procedure, we will modify eq. 9 to provide a more direct computation of L.

The computation of the canopy height is described in the manuscript, but the paragraph is unclear, and it will be modified. Considering that the tree stratum of the dehesa is quite homogeneous in composition, dominated by mature *Quercus ilex* sp., and that grassland canopy has a very high variability of low height herbaceous species, the ecosystem structure has been simplified to compute h_c in the following way: A constant height of 8 m has been assigned to oak trees, which is multiplied by its ground coverage in each pixel. Oaks f_c is computed annually using summer NDVI in eq. 8. During the summer the grasslands are dry, and the only photosynthetically active vegetation contributing to the NDVI signal are the oak trees. The grassland height is low (< 1 m), affecting the effective canopy height of each pixel less than the trees, and it is also difficult to compute based on monthly vegetation indices given the high species variability. For this reason, the grassland height has been discarded and only the contribution of trees was considered to compute h_c . We are aware that this is a simplification of a complex system that will contribute to the error of modelled fluxes. However, it was an operative solution considering the scale of this study.

9. Line 172: It would be helpful if the authors included a histogram and an estimate of the distribution skewness for ET and relative ET. From the description given here it appears quite small.

In the figure below (Fig1-commentR1), we present the histograms (one for each month) of both variables. For both variables most months presented an approximately symmetric distribution, with skewness between -0.5 and 0.5, three of them were moderately skewed and only one month (for ET) and two months (for ET/ETo) were slightly above one. We will elaborate this point in the manuscript. However, given the limited number of available points, these graphs only provide preliminary information and more data is required to confirm this point. For this reason, we prefer to include these graphs as supplementary information and not as part of the paper.

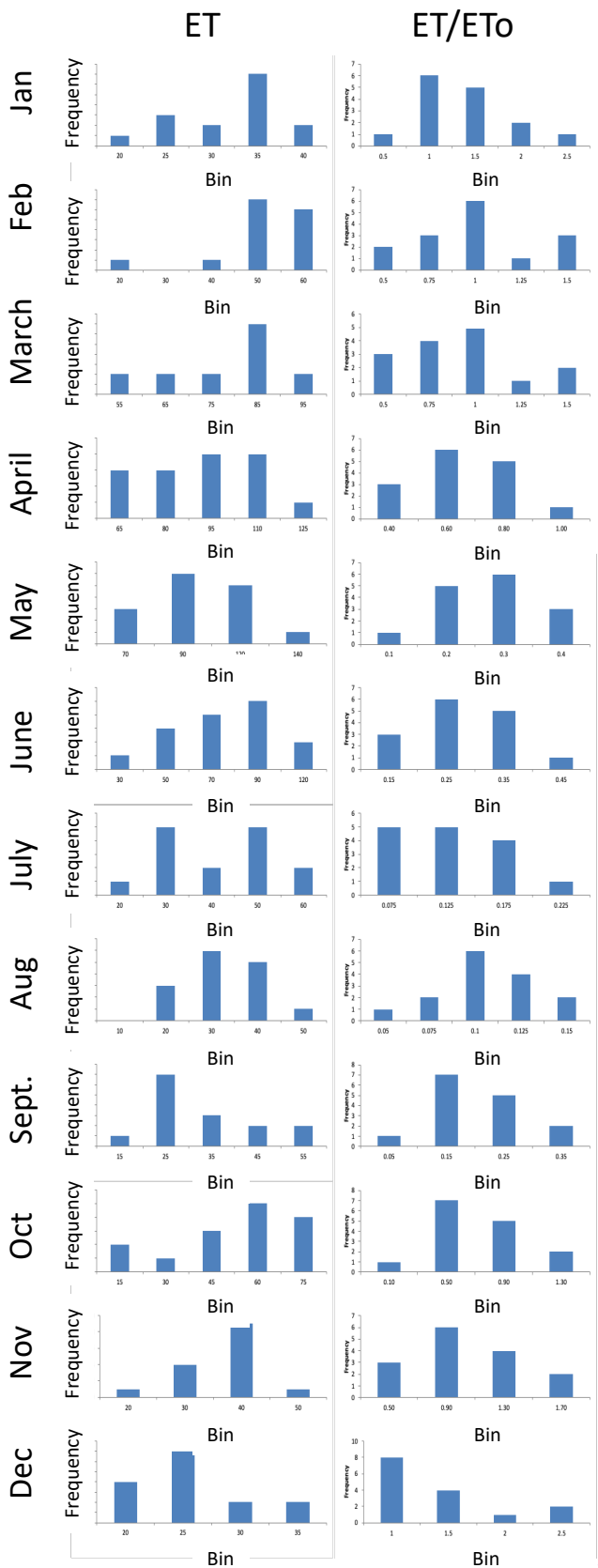


Fig1-commentsR1

10. Line 182: Replace "presented a general good agreement" with "generally showed good agreement".

It will be replaced

11. Line 184: Why the greater discrepancy for the turbulent fluxes compared to the C2 non-turbulent fluxes? Is this linked to imperfect closure for the flux measurements? Errors in partitioning the available energy between H and LE?

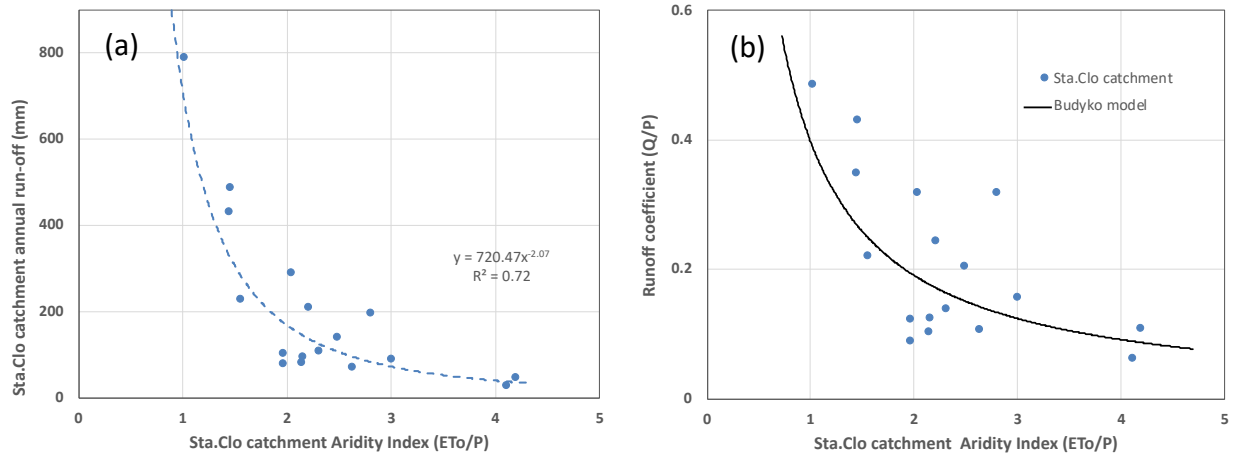
The imperfect energy balance closure is certainly a reason, as well as the discrepancy in the footprints of the different sensors (radiometer, soil heat flux plates, and the instruments for measuring the turbulent fluxes) and that of SEBS estimates. However, the different complexity in the formulation and computation of the radiative and the turbulent fluxes (the net radiation equation is a kind of linear representation, while the equation to estimate the sensible heat flux is highly non-linear), and the factors that influence each component (R_n is influenced by LWD, SWD, albedo and LST; H is influenced by LST, T_a , wind speed, NDVI, f_c and LAI) also influence the final error. A small bias in LST, T_a , and vegetation information can cause a high bias in H (and thereby LE, compute as a residual). The soil heat flux has usually a low RMSD, but generally this comes with a higher relative error, due to the reduced magnitude of this flux.

12. Line 206: The sentence beginning "Very low runoff ..." is redundant and could be omitted.

It will be deleted

13. Line 207: Why isn't the relationship shown? Although it reasonable to suspect these two quantities would be correlated, a "close" relationship is a bit of a surprise. It would be useful to show this relational.

We show below (F2-commentsR1) the relationship between annual run-off measured at the Sta.Clo catchment reservoir and the annual aridity index (Budyko, 1974) estimated for the same catchment on the left, and the same relationship with the run-off (Q) also normalized by precipitation on the right. The shape of these relationships shows how variations in climate, as represented by variations of P and ET_o , impact runoff and could provide a mean to assess the effects of a changing climate on water availability in this watershed. The budyko model represented in (b) was derived using Zhang et al. (2008) eq.9 with an adjusted value for α parameter equal to 0.54. It shows a mean to estimate long term annual run-off values in this catchment. Although these are interesting relationships, useful to complement the drought assessment, it's a little outside of the topic and might disrupt the flow of the results, so we prefer to present it as supplementary material.



F2-commentsR1. (a) Relationship between annual run-off measured at the Sta.Clo catchment reservoir and the annual aridity index (Budyko, 1974) estimated for the same catchment and (b) Relationship between run-off coefficient measured at the Sta.Clo catchment and the annual aridity index.

14. Line 207: Numerous metrics and indices have proposed been proposed over time to quantify aridity. It would be helpful to add a sentence or two to describe this index.

We will include in the text the following definition: "These annual run-off measurements followed a close relationship (Figure 4) with the annual aridity index (Budyko, 1974), estimated at Sta.Clo following Arora (2002), as the ratio between potential evaporation and annual precipitation."

15. Line 222: Do the difference in the anomalies suggest local drought conditions? For example, during 2008/2009 there is a strongly negative value at the ES-LMa site while the value is slightly positive at StaClo. Would this indicate a local drought in the area about ES-LMa?

This is a correct observation. The difference is caused by the big difference in precipitation, as indicated in Fig. 3, precipitation at Sta.Clo (683 mm/a) is about twice that at ES-LMa (338 mm/a).

16. Line 253: it worth point out that the peak in the autumn is much weaker than the one earlier in the year.

Yes, it will be pointed out in the revised manuscript.

17. Line 299: The phrase "and the more ..." also refers to ES-LMa, which was already discussed.

Yes, it will be deleted

18. Figure 5: The word "fraction" is misspelled.

It will be corrected

References:

Arora, V. K.: The use of the aridity index to assess climate change effect on annual runoff. J. Hydrol. 265:164-177. 2002.

Budyko, M.I.: Climate and life, Academic Press, Orlando, FL, 1974.

Zhang L., N. Potter, K. Hickel, Y. Zhang, Q. Shao: Water balance modeling over variable time scales based on the Budyko framework – Model development and testing, J. Hydrol., 360: 117-131. <https://doi.org/10.1016/j.jhydrol.2008.07.021>. 2008