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 #2

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

General Comment

This paper deals with the modeling of drought in a oak savanna in Spain, where trees and pasture coexists, using ET estimates from thermal remote sensing data. I found the paper generally well written and well organized. The goal is clear, and the results sufficiently elaborate. However, I have three main concerns regarding the adopted methodology:

We really appreciate the time dedicated by the reviewer to read this manuscript and all the suggestions and comments that have been provided. We have considered all the comments, and the suggested changes and clarifications will be introduced in the revised manuscript.

1) the SEBS model is well-known in the remote sensing community for "instantaneous" application at the satellite overpass time (eventually followed by upscaling procedures to daily/monthly scale). Here the model is used on monthly data, but the authors fail to clarify how the model was adapted for the change in time scale (more details in the specific comment P8, L4).

The methodological section did not provide sufficient detail on how the different time step data were aggregated to SEBS inputs for the calculation. We will add a new section to the revised manuscript dealing with "Model parametrization and dataset preparation" to clarify this issue. The monthly ET calculation using SEBS was demonstrated by Chen et al. (2014). The structure of the model was not changed regardless of whether it was used for instantaneous, daily or monthly ET calculations. The difference in its implementation was only due to the input datasets. For monthly ET calculation, monthly mean LST, air temperature, wind speed, downward shortwave radiation, downward longwave radiation etc were used. The accuracy of monthly LST, a key variable in SEB models, was evaluated by Chen et al. 2017, supporting its applicability for climate studies and numerical model evaluation.

References:

Chen X, Z Su, Y Ma, J Cleverly, M Liddell. (2017) An accurate estimate of monthly mean land surface temperatures from MODIS clear-sky retrievals, , Journal of hydrometeorology 18 (10), 2827-2847

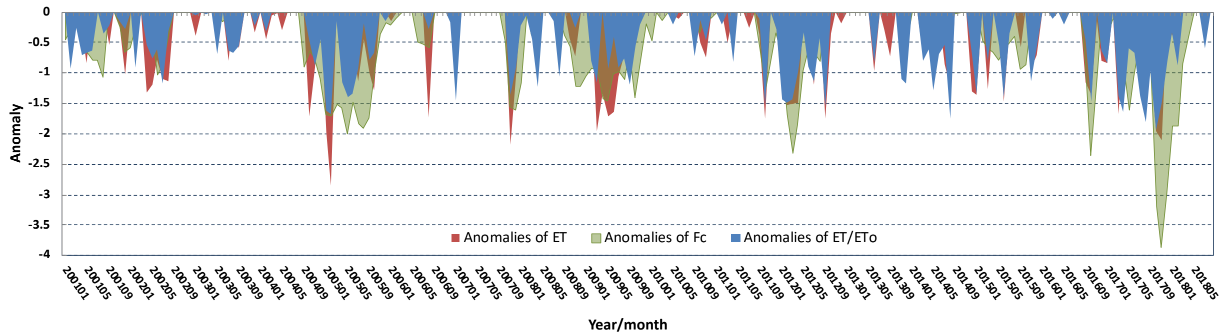
Chen X, Z Su, Y Ma, S Liu, Q Yu, Z Xu. (2014), Development of a 10-year (2001-2010) 0.1 data set of land-surface energy balance for mainland China. Atmos. Chem. Phys., 14, 13097–13117. 2014. www.atmos-chem-phys.net/14/13097/2014/

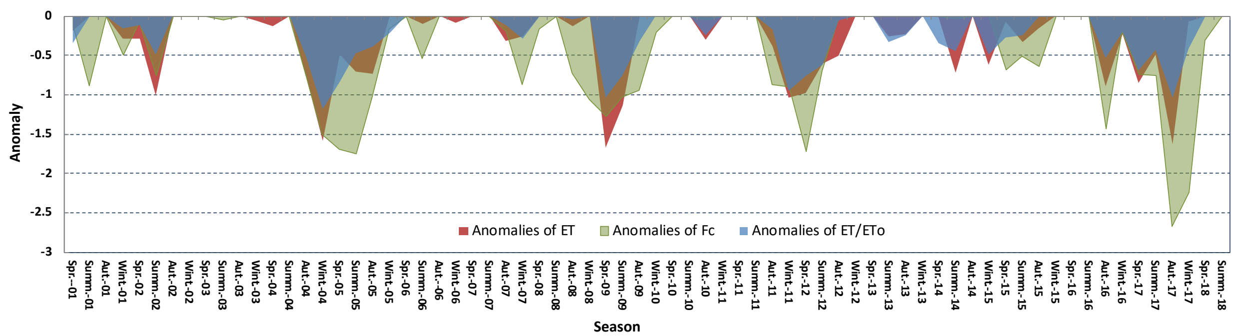
2) The authors decided to use anomalies of the ratio ET/ETo as drought indicator. However, they do not provided neither evidences that this index perform better than others (e.g. even the simple ET), nor justification on why this index was used for the ecosystem under analysis (is it better suited for oak savanna than others?). Indeed, part of the study shift the focus on fc, because ET is not able to separate the behavior of trees and pastures. This analysis, even if interesting, is out of place give the declared goal of the study.

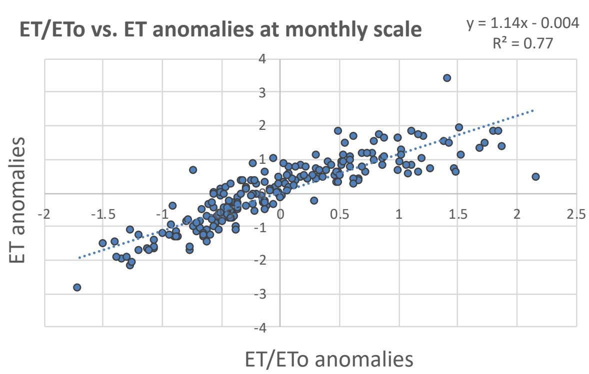
The reasons for the use of evapotranspiration anomalies to assess agricultural drought and a remote sensing-based surface energy balance model to estimate ET are provided in the introduction. However, as the reviewer indicates, the selection of the ratio of actual to potential ET was not explained in the manuscript. The reason why ET is normalised by ET_o is to separate the ET signal component responding to soil moisture from variations due to the radiation load. Therefore, this reduces the variability in ET due to seasonal variations in available energy. Anderson et al., (2011) showed that anomalies in ET/ET_o were more strongly correlated with other drought indices (including the US Drought Monitor, PDSI, PDMI, PHDI, SPI) than were anomalies in ET for most US climatic divisions, showing strong agreements in the southwest of the country, with a similar climate to the study area.

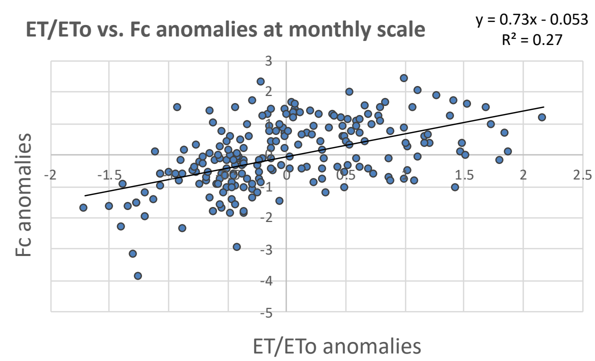
However, following the reviewer's recommendation, a comparison between both series of anomalies (including also anomalies of Fc) has been performed (see figures below). The result showed that, for the conditions of the study, the anomalies of ET and ET/ET_o performed similarly to characterize drought periods, presenting a high correlation (R^2=0.76 at monthly scale and R^2=0.82 at seasonal scale). It suggests that ET anomalies could be an option to monitor drought in dehesa areas. Nevertheless, the computation of ET_o does not require additional variables than those already used by the energy balance models, with a quite straightforward computation. Once actual ET is estimated, the computation of ET/ET_o takes very little effort and adds some confidence to the focus on the soil moisture signal. The graph of comparison of monthly anomalies will be added as a new figure to the paper and these results will be discussed in the text, including some comments to the rest of figures, presented as separate supplementary information. The justification of the selection of the ratio ET/ET_o will be also included in the revised manuscript.

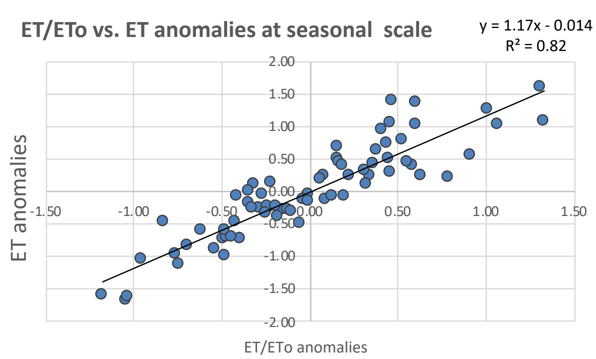
The explanation for the use of Fc and its connection to the goal of the paper is included in the following comment.

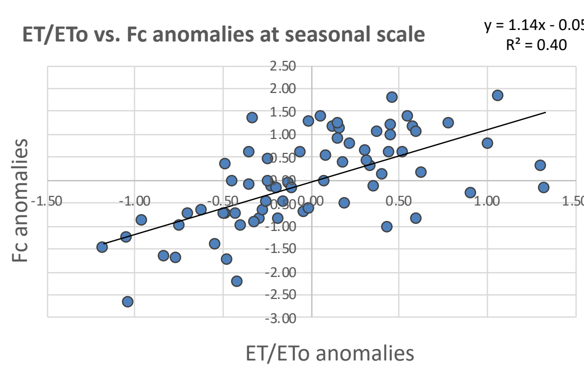












3) The authors used vegetation coverage and wheat productions as proxy of the drought impacts, without providing any justification for this choice. The first quantity is actually one of the input of

SEBS, but is also weirdly used also for “validation”, whereas the second is not necessarily related to drought impacts in a drought-resistant agropastoral system (see their words in P3, L6 of the manuscript).

The vegetation condition and the failure of crops are known consequences of a declining soil moisture and both have been used previously as indicators of drought (Liu and Kogan, 1996; FAO, 1983). Both variables, together with general numbers of hydroelectricity production, were the only available data that can provide a complementary view on drought impact in addition to evapotranspiration anomalies. As the reviewer points out, the green canopy cover is one of the inputs of SEBS and it is not used in the manuscript to validate the series of ET/ETo anomalies. However, the explicit analysis of its evolution sheds some light on the interpretation of these anomalies. In the case of wheat production, this rainfed winter cereal is the main agricultural use of dehesa areas. It is periodically sown in many pasture fields of this ecosystem. Its growth cycle is similar to that of the natural grasslands, with both of them escaping drought and coping with the long summer dry season by completing its life cycle before serious soil and plant water deficits develop. Given that no irrigation is provided, the impact of moisture deficits over its yield can be considered an indirect indicator of the impact of drought on all dehesa herbaceous vegetation.

An explanation justifying the use of both proxies will be included in the methodological section of the manuscript.

References:

Liu, W.T., Kogan, F.N., 1996. Monitoring regional drought using the vegetation condition index. Int. J. Remote Sens. 17, 2761–2782.

Food and Agriculture Organization, 1983. Guidelines: Land evaluation for Rainfed Agriculture. FAO Soils Bulletin 52, Rome.

In view of these considerations, I suggest the authors to revisit the manuscript to clarify these points before considering for publication. Some additional specific comments are also reported below, which I hope would be useful for improving the overall quality of the manuscript.

Specific comments

Title: I would replace the world “monitoring” with something else, since in my opinion monitoring implies something done in near-real time.

We will replace the term *monitoring* by *assessment* in the title: “Long-term water stress and drought assessment of Mediterranean oak savanna vegetation using thermal remote sensing” and in some references along the text.

P2, L1: RMSD > xxx, and R^2 < xxxx for all...

To provide a general idea of the global performance, we prefer to show average values rather than absolute ones for RMSD and R^2. We will modify this sentence of the abstract to clarify it.

P2, L2-3: The details for each site are not needed in the abstract, especially after the previous sentence.

We are sorry but we are not sure to which "details for each site in the abstract" the reviewer's comment refers to. We don't provide details for each site separately there. There is a general comment "for both sites", which we consider relevant for the abstract.

P2, L8: "with the first one being. . .". I suggest to move this to a new sentence.

It will be changed.

P5, L4: Here I miss something that better links the previous description of the dehesa with the adopted modeling framework. In particular, why ET modeled by SEBS has been used? Is it a good option to capture the specificities of this environment (e.g. other options, such as dual source approaches, agri-forest modeling)?

We have not performed a comparison of different models' performance over this ecosystem. Several inter-comparison studies have evaluated different modelling schemes and no single one had been found consistently best across all biomes (Ershadi et al., 2013). The SEBS model has been selected here because it presents a good compromise between the detailed parameterization of the turbulent heat fluxes for different states of the land surface on the one hand, and the input requirements, kept to a feasible minimum and without requirements for local calibration, on the other. Thus, it is a good candidate to produce global fluxes (Chen et al. 2019, Timmermans et al., 2013) and this work may contribute to improve the model parametrization for this type of ecosystems, usually poorly represented in land-atmospheric models. There was another practical reason in that the model had been previously applied with good results by Chen et al., (2014), at a similar spatiotemporal scale. Many operative solutions presented in that paper were also used here, simplifying the implementation of the model.

References:

Chen X., Z. Su, Y. Ma: Remote sensing of global monthly evapotranspiration with an energy balance (EB) model. The International Archives of the Photogrammetry, Remote Sensing and Spatial Information Sciences, Volume XLII-2/W13, 2019 ISPRS Geospatial Week, Enschede, The Netherlands. https://doi.org/10.5194/isprs-archives-XLII-2-W13-1729-2019. 2019

Chen X, Z Su, Y Ma, S Liu, Q Yu, Z Xu. (2014), Development of a 10-year (2001-2010) 0.1 data set of land-surface energy balance for mainland China. Atmos. Chem. Phys., 14, 13097–13117. 2014. www.atmos-chem-phys.net/14/13097/2014/

Ershadi A., M.F. McCabe, J.P. Evans, N.W. Chaney, E.F. Wood: Multi-site evaluation of terrestrial evaporation models using FLUXNET data. Ag. Forest Meteorol. 187: 46–61 http://dx.doi.org/10.1016/j.agrformet.2013.11.008

Timmermans J., Z. Su, C. van der Tol, A. Verhoef, and W. Verhoef: Quantifying the uncertainty in estimates of surface–atmosphere fluxes through joint evaluation of the SEBS and SCOPE models.

Hydrol. Earth Syst. Sci., 17, 1561–1573, 2013. doi:10.5194/hess-17-1561-2013 www.hydrol-earth-syst-sci.net/17/1561/2013/

P6, L3: I would suggest to write the eq. as LE = Rn - . . .. since you already introduced concept of LE as residual.

It will be changed.

P6, Eqs. (4) and (5). The second eq. is redundant.

Eq.5 will be removed

P7, Rqs. (6) and (7). These two equations are confusing. In LEwet is computed via (6), then Hwet needs to be defined in another way, or vice versa. Please clarify.

Eq. (6) will be similarly removed, Eq. 16, in Su (2002), will be added for the calculation of H_wet.

Su Z.: The surface energy balance system (SEBS) for estimation of turbulent heat fluxes. Hydrol. Earth Sys. Sci., 6(1), 85– 99, 2002.

P7, L3. The way the limits are used needs a better clarification.

P7, L4. if Hwet is derived from Eq. (7), LEwet needs to be defined by an eq. that is not (6) (e.g. Penman-Monteith as stated afterward).

P7, L5. ". . . a set of assumptions. . .". Please provide a brief description of these assumptions.

Answer to the three questions above regarding the limits: The use of the limits in SEBS are fully described in Su (2002). We will rewrite this part of the SEBS model description to clarify it.

P7, L7. The role of canopy height is not clear at this point for a reader that is not familiar with the model. Please briefly introduce where and how hc plays a role. Also, the authors introduced a "revised version of the model. . . new bare soil resistance" (P5, L18), but the role of this new parameterization is not clear since there are no mention of resistance in the model description.

The canopy height is needed for calculating the momentum roughness length and thus, important for the sensible heat calculation. A short explanation will be added on how resistance is calculated, where the role of soil resistance appears.

P8, L4. The SEBS model has been designed for "instantaneous" application at the time of LST acquisition. As a consequence, more details needs to be provided on how the authors adapted the model to work on monthly LST. I think that the idea is to use monthly LST as a "artificial" instantaneous LST for a theoretical average day, but some questions that needs to be addressed are: - how did you ensure consistency between the mosaicked monthly LST and 6h ECMWF meteo forcing? - How 16 days NDVI was used jointly with monthly LST? - How daily upscale was performed? - How monthly upscale was performed?

This issue was mostly addressed in the first point of the general comments. It was clarified that no model upscale was performed. LST and all the meteo forcing used to run the SEBS model in this study were monthly mean values. Monthly mean meteo forcing were directly provided by ECMWF (available for download in its website). Monthly mean LST was processed following the work of Chen et al. (2017) referenced above. Monthly NDVI was derived from 16 days NDVI by selecting the maximum values in each month. All this information will be added to a new methodological section dealing with dataset preparation.

P9, L1. Some details on the balance closure would be helpful. Was closure forced, and with which method? How were the data cumulated at monthly scale (I'm assuming some unavoidable missing data during the acquisition, any constrain on minimum data, etc.)?

The closure of the balance was forced using the residual method. For ES_LMa the processing of the data (gap filling, monthly aggregation) corresponded to the procedure standardized by Fluxnet (described here: https://fluxnet.org/data/fluxnet2015-dataset/data-processing/). In the case of Sta.Clo, the comparison period was selected attending to the quality of the data and some month were discarded due to missing information. A new paragraph with the details on data selection and processing will be included in the manuscript.

P10, L13. It would be interesting to have a couple of words on the reason behind the use of ET/ETo rather than ET itself for the computation of anomalies. In my experience, there are many cases where ET anomalies are a better proxy of drought that ET/ETo ones. Ideally, the authors should add a test showing that ET/ETo outperform ET alone (especially with the latter being a more conservative approach, which does not need any additional quantity).

We will add to the revised version an explanation for the reasons behind the use of ET/ETo rather than ET itself for the computation of anomalies. In addition, we have compared both anomalies at monthly and seasonal scales, part of this analysis will be presented in the manuscript with a new figure and the rest as supplementary information.

P10, L15. I have some issue with the use of fc as proxy of drought impacts, especially when fc is also one of the input of SEBS. If fc is a good proxy of drought impact, why we should use a complex model such as SEBS (which uses fc as input) to derive a quantity (ET) which performance is evaluated against fc. Why don't we use directly fc (or rather fc anomalies) at this point?

We have justified above the way of using fc in the paper. Regarding the evaluation of Fc anomalies, a new analysis has been performed to compare its performance to drought assessment with ET and ET/ETo anomalies (see figures of the general comment 2). From these figures, both at monthly and seasonal scales, it can be derived that the drought events identified using the three variables would have been the same, but with different intensities and duration. The main differences can be found during the cold winter months when the vegetation is largely dormant. In these cases, the anomalies of Fc, similarly to the performance of other indices based on vegetation as the Vegetation Condition Index (VCI) (Heim, 2002) have a limited utility. The results are more comparable and could be more useful during the growing season.

Heim, R. R., 2002: A Review of Twentieth-Century Drought Indices Used in the United States. *Bull. Amer. Meteor. Soc.*, **83**, 1149–1166, https://doi.org/10.1175/1520-0477-83.8.1149.

P10, L18. Similarly, I miss the connection between the impact of drought on the dehesa (a predominantly oak savanna) and wheat production. I know that having an independent estimate of drought impacts is tricky, but if the focus of the paper is specifically for the dehesa, you should justify better why wheat production is a good proxy of the drought impact on a likely drought-resistant, adapted oak savanna. The use of this quantity risks to lost the specificity of the work that you introduced earlier.

We have justified the use of wheat production as a component of dehesa and attending to its similar growth cycle to natural grasslands. The impact of moisture deficits over its yield can be considered an indirect indicator of the impact of drought on dehesa herbaceous vegetation. This point will be clarified in the methodology of the revised manuscript.

P11, L12. It would be better to have the results disaggregated for seasons, in order to better highlight the impact of this seasonality in the error. This would help discussing the results, since drought may be mostly concentrate in some seasons. Also, since your goal is to use ET/ETo anomalies as proxy for drought, it would be much better to have in addition a validation of both ET/ETo values and z values against ground data. Even if the length of the time series is quite short, it is important to show that the model is able to capture the year-to-year fluctuations, since this is what you want to reproduce. Often, ET estimates are "well" modeled only because the area has a strong yearly cycle.

Of the different temporal scales to show the results, we have selected the most extreme available (year and month). The seasonal information can be derived from Figure 7 for ET, ET0, P and fc and the identified dry period. The validation of ET estimated, as shown in Figure 2, is performed on monthly data.

P12, L1. It is weird to me that you show the yearly-aggregated data before the monthly one. Apart form that, Figure 3 is a good example of my consideration on P10, L13. Just looking at the plot, it seems that ET capture the same events that ETo if Precipitation is used as reference. What is the added value of using ET/ETo rather than ET alone?

We chose to present the results from a coarser temporal scale to provide a more general vision of the evolution of drought years to more detailed monthly results in which we can discuss shorter term variations.

P12, l15 to P13, L5. This whole paragraph seems a little out of topic to me. I suggest to reword to clarify the role in explaining drought in the region, or remove it completely.

This first part of paragraph (in our text lines 207-212) is intended to describe the area of study in terms of aridity and provide some numbers corresponding to the experimental sites, to classify them in relation with other climate areas of the world. Information regarding this analysis will be included as supplementary material, and some clarifications will be added to the text. The second part (lines 212-216) compares the two sites and discusses some aspects of Figure 3, as the relation between ET and ETo at annual scale, that we consider related to the topic of the paper.

P13, L13. Please define a mild drought. Also, it is not clear to me what is the role of this intercomparison between the modeled data over the two sites. Please clarify the aim of this comparison and justify the inclusion of a dedicated figure.

We define drought intensity in terms of maximum negative anomaly of relative ET values reached during the event (thus using the standard deviation as a measure of its departure from the mean). For the analysis of the events that occurred during the study period, the following thresholds were used: severe drought (anomalies <=-1.5); moderate drought (anomalies between -1 and -1.5) and mild drought (anomalies between -1 and 0). These classes are used for both annual and monthly time steps. This info will be added.

The intercomparison between sites complements the information provided on the experimental sites used to validate the model. In addition, we don't present a complete disaggregate analysis and most of the paper is focused on the whole dehesa region. This figure of the experimental sites points out that the general patterns are similar but there exist local differences and provides an estimate of the magnitude of these differences.

Fig. 5. Again, what is the added value of ET/ETo anomalies over ET alone (or, even worse, fc)? If anything, these figures are convincing me even more that a complex modeling framework is not needed, at least at annual scale. I'm sure that there is something more, but this is not discussed and justified by the accompanying text.

This issue is addressed above and also in the manuscript, including a new analysis and a new figure comparing the anomalies of ET/ETo, ET and fc at monthly scale. In the complementary information the analysis is extended to the seasonal scale.

Fig. 6. There is an odd strikingly resemblance between the spatial patterns in the years 2004/2005 and 2011/2012. Can you elaborate a little more on that?

Yes, both maps look quite similar, but they are different. An option for the analysis could be to produce a difference map to analyze similarities and differences.

P15, L2. Is this the average over the whole dehesa? A single point? Other? Please clarify. Also, in case of the average, it would be interesting to see if also the spatial variability (std.dev) shows interesting results.

Yes, it is the average. We will calculate the std.dev and will include it in the paper if it provides interesting results.

P15, L12. What about the intra-annual fluctuactions? Are they similar to ET/ETo z values also at this temporal scale? Any temporal delay?

We don't fully understand this question, we presented the monthly data to analyze the intra-annual fluctuations. The comment has a different number of page/line than the manuscript we have. In most comments, we have identified the reference attending to the content but in this case it's not completely clear.

P15, L19. Similarly to comment P13, L13, duration and intensity of drought needs to be defined in the methodology section.

We define the duration of the drought as the successive number of months with negative anomalies and the intensity as the maximum anomaly in this continuous period. These definitions will be clarified in the methodology.

P16, L11 to P17, L9. These results are interesting but a little out of place in a paper on “drought monitoring using thermal remote sensing”, as you stated in L10 (A more detailed analysis is required...). Above all, this analysis suggests, again, how the adopted modeling framework may not be ideal for the study of this specific biome. Please justify this analysis in the context of the main goal of the study (thermal remote sensing), and against the use of ET/ETo as drought proxy.

The focus of the paper is on the assessment of long-term water stress and drought in dehesa ecosystem, the means used is thermal remote sensing, and fc evolution is also used to interpret anomalies of relative ET. The modelling framework used here is not the only plausible approach to monitor drought in this biome. However, the results have shown that it is well fitted for this system.