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 #3

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

General comments:

The study by Gonzalez-Dugo et al. presents an interesting analysis of long-term ET and drought indicators over an Oak savanna region in Spain. The study implemented a surface energy balance model (i.e. SEBS) together with MODIS products and ERA meteorological data to obtain monthly and annual water stress indicators for a 17-year period. The manuscript demonstrated a sound remote sensing-based methodology and is valuable to better understand the long-term effects of droughts over an important and complex region such as the Spanish dehesa, which may be also relevant for other similar savanna-like ecosystems. The analysis of the monthly and annual time-series demonstrated an important dataset that helps to better characterize and understand drought events (and their effects) in these water-limited ecosystems. The results and conclusions were well described and articulated.

However, I have some comments related to certain details of the model set-up, which were missing or not clearly elaborated in the methodology section. Since the study presents a workflow to obtain long-term water stress indicators, more information on how the input datasets were pre-processed is needed (e.g. retrievals of inputs, resam- pling of datasets at different temporal and spatial resolution) so this workflow can be reproduced for other studies/applications. Additionally, it was not very clear how the authors tackled the issue of having different vegetation covers (i.e. trees and grasses) and if the model inputs/structure reflected this added uncertainty in these types of land-scapes. The retrieval of certain inputs, especially important ones like LAI and canopy height, should be more clearly described. In addition, the study should more clearly show the particularities of the dehesa system and how the methods presented here are more sound for monitoring dehesa (and similar) ecosystems compared to other ET products such as, for example, the MODIS ET product.

The study is concise and relatively well written. However, the authors should review certain sentences and try to write with more direct language in certain situations (see the specific comments below for examples).

Overall, I would recommend accepting this manuscript after revising and addressing the comments specified below.

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.

Specific comments:

L44-45: Here, the authors briefly mention the complex canopy structure of the agro-system and how it causes an added difficulty to assess and monitor droughts. However, a few more details on the particularities of dehesa/savanna ecosystems is needed in the introduction and, more concretely, why these ecosystems demonstrate greater uncertainty when using modeling methods, such as surface energy balance models especially compared to landscapes with more homogeneous canopy covers and structures. This would further justify the study, which provides a methodology that monitors ET and drought for an ecosystem that tends to be poorly represented by land-atmospheric models, usually causing for greater uncertainties.

Similarly to other savanna ecosystems, the different components of dehesa structure: sparse tall vegetation, large areas of grasses, shrubs, and bare soil, contribute differently to the turbulent exchange and radiative transfer, hindering its modeling, especially when compared with more homogeneous landscapes. In addition, these vegetation layers differ in phenology, physiology and function: while the trees are evergreen and have access to sources of water all year, the herbaceous layer only taps water from the first cm of soil and dries up during summer. The combined different functioning and characteristics of the system components affects the exchange of sensible and latent heat flux, resulting in a high spatial and temporal flux variability difficult to account for in model parametrization and algorithms. This structure appears to play an important role in savannas' resilience, making the system an efficient convector of sensible heat and keeping the canopy surface temperature inside the adequate range for survival (Baldocchi et al., 2004). A brief explanation of this will be added to the introduction of the paper.

References:

Baldocchi, Dennis D. and Xu, Liukang and Kiang, Nancy. (2004) How plant functional-type, weather, seasonal drought, and soil physical properties alter water and energy fluxes of an oak-grass savanna and an annual grassland Agricultural and Forest Meteorology, 123: 13-39. doi: https://doi.org/10.1016/j.agrformet.2003.11.006

L74: Why was SEBS used compared to other models? A small justification is needed for the use of SEBS. What advantages does it present compared to other models? Why not other thermal-based SEB models such as e.g. METRIC, SEBAL, TSEB etc or optical-based PM/PT methods as used in the MODIS ET product. Or even the use of products from geostationary satellites such as LSA-SAF ET.

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 has 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 as the reviewer mentioned in the previous point. 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 that the one of interest for this application. Many operative solutions presented in that paper were used also 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/

L116: It says 'The green canopy cover and leaf area index (L) were calculated using the following equations (Choudhury et al., 1994)' however equation 8 or 9 do not detail how leaf area index was computed (only fractional cover, fc)

Fc is calculated using eq.8 and L is derived from fc using eq. 9. However, to clarify the procedure we will modify eq. 9 to provide a more direct computation of L.

L125-129: It is not very clear how the canopy height was estimated. Is the canopy height assumed to be 8m, as such only accounting for tree and neglecting the grass/pasture or is it an integrated/effective value based on NDVI? If not ignoring the grass, how is the grass canopy height estimated? What is the relationship between NDVI and canopy height? I suggest to re-write this paragraph to makes this clearer and more specific.

Yes, we will rewrite the paragraph to clarify computation of canopy height and justify the decisions made to simplify the structure of the system. This simplification is based on the homogeneity in composition of the tree stratum of the dehesa, dominated by mature Quercus ilex sp., and on the very high variability of herbaceous species with low heights of the grassland canopy. To compute hc a constant height of 8 m has been assigned to oak trees, which is multiplied by its ground coverage in each pixel. The oaks fc 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 that of 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 hc. 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.

L131-132: Leaf area index was previously defined as L in L116 but here uses the acronym LAI. Should be consistent throughout the manuscript.

Yes, it will be corrected

L151-153: Review sentence with more direct language. E.g. 'The good correspondence between the model input was verified [. . .]'

It will be changed to: "In both cases, the good correspondence between the model input and the ground measurements was verified (data not shown)."

Section 2: Some more clarification is needed in the methodology section on how the model inputs and parameters were set up and evaluated. Perhaps also a table that states all the inputs and parameters used in SEBS with their values/method would help clarify this. This information is scattered in the text but should be directly and clearly stated in the methods. Were the input datasets filtered for cloud cover/quality? Looking at Table 1, the different datasets used have different temporal and spatial resolutions (additionally in the text it says MODIS LAI product was used but it is not shown in Table 1). So how were these datasets homogenized? Which resampling algorithm was used? Was everything averaged for the month? Was only daytime meteorological data used or also nighttime? All this information should be stated so that the presented method is reproducible. In addition, the model evaluation method, and criteria (e.g, RMSE, R2 etc) should be explicitly stated in this section.

The methodological section 2.1 will be reformulated to include the missing information about the application of the model described in the referee's comment. It will be divided in three subsections: 2.1.1 SEBS model description; 2.1.2 Model parametrization and dataset preparation; and 2.1.3. Model evaluation. 2.1.2 will include a new version of table 1, a detailed description of the parameters, and the datasets used in the model, including the explanation of datasets resampling and homogenization.

L186: MBE acronym was not defined.

MBE stands for Mean Bias Error; it will be defined in the text.

L202-204: review sentence 'A few of the years [..] an increase in run-off'

It will be changed to: "Very wet years, and those with average rainfall but intense precipitation events producing an increase in run-off, did not follow this pattern."

L218: Here it is mentioned that drought was evaluated at the annual scale but how was it aggregated? As an annual average or cumulative over the year?

The annual value was an average of monthly anomalies. We will add this information to the text.

L222-223: why is the drought event of 2016/2017 considered mild, if it reaches similar levels as the years 2004/2005 and 2011/12, which were considered the most severe droughts (Fig.4)? Is there a cutoff/threshold?

Yes, we will 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). When analyzing the events 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. In terms of intensity, only the drought event of 2004/2005 can be considered severe (max negative anomaly = -1.7) and 2016/2017 is classified as moderate with the maximum negative anomaly equal to -1.29.

L225-228: Review sentence 'Figure 5 aggregates [..] scarcity on the system'. Sentence is too long, maybe cut in two with more direct language.

The sentence will be changed to: "Figure 5 aggregates, for the total dehesa area, the evolution of the relative ET anomalies, together with the exchanges of energy between the surface and the atmosphere, the green canopy cover, and the production of rainfed wheat. The last two variables were selected as indicators of the impact of water scarcity on the system."

L263-264: Make sentence more direct 'The duration [. . .] these periods'.

This sentence will be changed to use more direct language but also to include a definition of drought intensity attending to a previous comment.

Section 3.2: It would maybe be interesting to do a trend analysis to investigate if drought events are becoming more frequent/severe? Probably the time series is not large enough... but it does seem that the there are slightly more negative anomalies (particularly for Sta. Clo) from 2013/2014 onwards.

This is an interesting analysis that we would like to perform when a longer dataset is available. The current database, as the reviewer mentioned, is not large enough and it could provide misleading information.

L293: More direct language, e.g. 'The SEBS model was used [..]'.

The sentence of L293 will be changed and the whole text will be reviewed to use a more direct language.

L317-19: Review sentence. More direct language, e.g. 'The approach proved useful [..] defining and identifying areas of interest for future studies at finer resolutions'.

The sentence will be changed to: The approach proved useful for providing insight into the characteristics of drought events over this ecosystem, and for defining and identifying areas of interest for future studies at finer resolutions.

Table 1: In table caption, it says from 2000-2015 but the study time period is 2001-2018 right?

Yes, it was a mistake, it's 2001-2018 and will be corrected in the manuscript

Figure 6: The dehesa area of interest should be made more explicit and clearer in the map and legend. Also, little spatial analysis was provided in the text. For example, there seems to be important differences and patterns in the northern tip compared to the rest of the area of interest, most clearly seen in the average ET/ET0 map or in 2004/05, 2008/09, and 2011/12.

We will modify Figure 6 to make the area of interest more explicit in the map and the legend and we will add a few sentences briefly dealing with the spatial analysis. However, we have not performed a detailed analysis and only general comments can be made.

Figure 7a: There is no legend for the dashed green line.

The explanation has been added to the caption of Figure 7.

All figures: There should be self-explanatory captions in all figures so that the reader can understand the figure without looking at the main text.

The figure captions will be corrected to:

Figure 1: Distribution of oak savanna area in the Iberian Peninsula. Location of Sta.Clo (Santa Clotilde) and ES-LMa (Las Majadas) validation sites and pictures of both eddy covariance flux towers.

Figure 2. Comparison of monthly energy fluxes of latent heat (LE), sensible heat (H), net radiation (Rn) and soil heat flux (G) estimated using the SEBS model at a monthly scale and observed fluxes at each oak savanna site: ES-LMa (LA) for the years 2009-2011 and Sta.Clo (SC) for the years 2015-2017.

Figure 3. Evolution of annual rainfall, ET, ETo and ET/ETo at ES-LMa site (a) and Sta.Clo site (b), and annual run-off at Sta.Clo watershed from the hydrological years 2001/02 to 2017/2018.

Figure 4. Annual anomalies of relative evapotranspiration at ES-LMa and Sta.Clo experimental sites estimated using the SEBS model from 2001/02 to 2017/18.

Figure 5. Evolution from 2001/02 to 2017/18 of annual anomalies of relative evapotranspiration, energy balance components, air and surface temperature, vegetation ground fraction cover and rainfed wheat yield, aggregated for the whole oak savanna area of the Iberian Peninsula.

Figure 6. Spatial distribution of annual anomalies of relative evapotranspiration for the oak savanna area of the Iberian Peninsula from 2001/02 to 2017/18, the average ET/ETo for the period and its standard deviation (STD)

Figure 7. (a) Monthly evolution of evapotranspiration anomalies (blue line), with negative values indicating drier than normal conditions (depicted in red), and green canopy cover (green line) of the oak savanna area of the Iberian Peninsula. The dashed green lines connect the annual maximum

and minimum values of fc; (b) Monthly evolution of and rainfall, ETo and ET in the same region and time interval.