

## ***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**

Received and published: 15 July 2020

#### 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

C1

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, resampling 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 landscapes. 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.

#### 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,

C2

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.

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.

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,  $f_c$ )

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 make this clearer and more specific.

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

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

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

C3

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.

L186: MBE acronym was not defined.

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

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?

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?

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

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

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 there are slightly more negative anomalies (particularly for Sta. Clo) from 2013/2014 onwards.

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

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'.

C4

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

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/ET<sub>0</sub> map or in 2004/05, 2008/09, and 2011/12.

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

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.

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2020-190>, 2020.