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Hydrology and Earth System Sciences
Discussions

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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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:

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

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to clarify how the model was adapted for the change in time scale (more details in the specific comment P8, L4).

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.

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 agro-pastoral system (see their words in P3, L6 of the manuscript).

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.

P2, L1: RMSD > xxx, and R2 < xxxx for all. . .

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

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

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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)?

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

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

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

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 eq. (6) (e.g. Penman-Monteith as stated afterward).

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

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.

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?

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.)?

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).

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?

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.

P11, L12. It would be better to have the results disaggregated fro 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.

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?

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.

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.

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.

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?

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.

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

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

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.

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