

Interactive comment on “The predictability of reported drought events and impacts in the Ebro Basin using six different remote sensing data sets” by Clara Linés et al.

Anonymous Referee #2

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This study analyses drought indicators' relation and thus usefulness to predict drought impacts for a small case study region in Spain. I find the study a worthy addition to the drought literature, once its contribution has been worked better. In the current manuscript the reader 'gets lost' in the many correlations and individual results a bit and I think, some focus and highlight is needed to better appreciate the work and results. The manuscript also requires clarifications on a number of methodological details and better justification of some parts of the approach in order to assess and value the results.

Major comments:

1) A particular concern I have is related to the use of a very short time period for the

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‘benchmarking’ by correlation analysis. In this study, a time series from 2001 to 2012 is used with 8 out of these 12 years representing drought events (Section 3.1., beginning) – hence 2/3 of the time. The common understanding is, however, that drought is defined as a rare extreme climatic/hydrologic event. This has a few implications for the chosen method and conclusions from the study:

-To the international reader who doesn’t know the Spanish climate history, it is not convincing whether simply ‘dryness’ or real ‘drought’ was analysed if 2/3 of the time were ‘drought events’ – how was this distinguished in the textual search? Does the Spanish language distinguish between the two? Some languages do, others don’t.

- To define drought based on monitored hydro-meteorological or remotely sensed anomalies, long time-series are necessary to obtain the full range of situations and hence define the average and moments of distribution of the variable to be used to index drought as an extreme. How does that influence the results? The time series and temporal resolution for each index need to be given (inconsistent in current methods section), and where applicable the reference time series for standardization/normalization.

- Correlation depends on variability, but if two thirds of the time period are mostly dry, I would expect that this will have a considerable effect on the results of a statistic that relies on variability in the data. Some analysis regarding the sensitivity of the index series (and range of variability) on the results is therefore necessary to make an assessment of the uncertainties.

2) The objectives and used methods need to be better harmonized, in particular the relation of the use of statistical correlation analysis and the aimed for ‘predictability’ of impacts needs to be developed more clearly. An assessment of predictability of impacts would require a predictive application (some validation experiment). For hazards, it is also common to consider false alarm rates for an assessment of predictability. Overall, the use of statistics and their interpretation in this study requires more clarification and

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precision (see specific comments below).

3) The discussion section is rather vague in its comparison with other studies. In particular, there are many other studies that have correlated agriculture yields with drought indices in many countries (e.g. see table of the review by Bachmair et al., 2016 in WIRES Water). How do the findings for best-correlated indices and time scales compare to other studies? If this case study wants to contribute to the international literature these need to be better compared in the discussion. The niche of this study within the wider range of studies needs to be worked out more specifically to appreciate this small scale case study's contribution to the field.

Specific comments

Methods section:

4) The twofold use and steps of impact report analyses, (1)construct a narrative of the events, (2) use as binary indicator of impact in correlation analysis, should also be introduced as such in the methods section. The narrative in the first part of the results otherwise comes really unexpected, whereas the reader expects only correlation results.

5) P1 Last paragraph: another reason that RS data needs benchmarking are the short time series – compared to precipitation and hydrometric records a definition of drought as statistical extreme is not possible. THIS requires elaboration on assumptions made and limitations for the analysis.

6) P8 lines 3-5. It is necessary to add some justification for the categorizations of sectors and of type of information. Sectors: e.g. readers may wonder why only rainfed 'cereal' – are there no other rainfed crops? Is all irrigated agriculture similar regarding seasonality/demand? Type: why were these distinguished and e.g. what would be different whether a drought is retrospectively reported or "mentioned". I couldn't find how this classification was used in the correlation analyses – if it wasn't used in the

analyses, it is not relevant to mention in the methods section.

7) p.8 line 14 ff. From that section it is unclear, which variables were correlated with which exactly. I suggest to name all variables in the data section and then here add a clear list/matrix of what to expect in the results section.

8) P8 line 15 Isn't it the other way round: predictability may be (! But not necessarily) related to the strength of the relation Please elaborate more precisely the link between a correlation analysis and it's potential for prediction (of what exactly?) – this is not necessarily methodologically straightforward.

9) p.8 line 28. How is binary information used in the calculation of correlations, which normally require ordinally scaled data. How is significance tested? How is auto-correlation corrected/considered (Spain always emphasizes that they have multi-year droughts). Citing an R function cannot replace a complete introduction of the statistical methods used, incl. all variable transformations and an introduction of all the measures used later in Results and Discussion sections.

10) P9 line 13-end belongs into the methods section. Now it is partly a 1:1 repetition (unnecessary) and more explanation on the method than in the methods section – should be the other way round.

Results section

11) Similar to the impact reports, some time series of the longer series standardized indices (and RS variables?) need to be shown to support that we are indeed looking at drought as an extreme event (e.g. also in 3.1).

12) p.11 line6ff – a description of figure legend in the text is not necessary. This info should be clear from methods section/variable definition and figures' legends and captions. Instead, a more detailed description of correlation patterns shown by the figures needs to be given to warrant having them all in the paper.

12) Quite a bit of discussion is already included in the results section, which makes

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it a bit difficult for the reader to see the main outcomes, i.e. the strengths of correlations found, the differences and similarities, etc... without immediately being biased towards a possible background explanation. I suggest to moved all sentences with references into the discussion section and here purely describe own results first (see also introductory comment on better focus on unique results).

Figures

13) Fig. 3 (and similar) The heading is misleading. Many readers would expect scatter-plots of indicators vs impacts. The legend needs a header relating to a clear variable from the methods section (maximum of cross-correlation what - coefficient? What exactly is shown?)

14) Figure 5 (and similar) is very hard to see/read as labels are too small on A4 print. Also captions and legend labels need to be more precise (p-Values for which test – methods section?) shown are circles scaled to p-values (range not one value), but not “significances” (better to define previously).

Discussion section

13) The discussion misses a critical evaluation of the statistics used with respect to assumptions on methods and data (besides the impact reports). In particular, more insight into the RS data and what they inherently ‘see’ or not linked to drought impacts would help advance the selection of future usage of these as indices.

14) p. 18 line 17ff. The list of caveats is good, but will be more useful if it were expanded by how these will actually each affect the results.

Suggestion: For consideration of seasonal aspects and time trends in impact occurrence modelling, perhaps the discussion by Stagge et al. 2015 may provide some further ideas.

References

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Stagge, J.H., Kohn, I., Tallaksen, L.M., Stahl, K. (2015) Modeling drought impact occurrence based on meteorological drought indices in Europe *Journal of Hydrology* 530, 37–50 doi.:10.1016/j.jhydrol.2015.09.039

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