

Comment on hess-2021-209

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Referee comment on "Drought impact links to meteorological drought indicators and predictability in Spain" by Herminia Torelló-Sentelles and Christian L. E. Franzke, Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-209-RC2>, 2021

1 General Comments

The manuscript investigates the link between meteorological drought indicators and drought impacts and, based on that, further attempts to predict drought impacts in a modelling study.

While I find the basic idea behind the study intriguing, I think the manuscript is not particularly well executed in terms of structure, logic and intelligibility. It was at times not easy to follow the read thread and to grasp what had been done methodological. But my two main concern relate to the underlying research questions and justifications for this paper as I further explain hereafter in the specific comments. Technical comments and corrections are included further below.

2 Specific Comments

2(a) Link between drought hazard and impacts

Trying to make a link between meteorological drought and climate indicators (i.e., the hazard) to the actual impacts, which – as the authors themselves state – are "*highly dependent on a region's vulnerability to drought*" (line 27), without properly discussing the conceptual frameworks for vulnerability and the importance of exposure (the latter term is not once mentioned in the manuscript) is a major flaw in this study. Exposure is related to the tangible entities exposed to the hazard and can be made up of buildings, people, livestock, crops etc. Vulnerability on the other hand, is the susceptibility of a system to be negatively impacted by the hazard. Consequently, impacts will not be reported to EDII just because there has been a drought hazard, but only if a certain

region/economic sector or ecosystem has actually been exposed to the hazard and is actually vulnerable.

This issue becomes apparent in the results of the study where two of the regions (NE and CE) show lowest correlations with the drought indicators (line 311). When comparing Figure 1 of the manuscript to a population density map of Spain, these two regions clearly are least populated (i.e., less exposure), which might have affected the number of reported impacts.

To round up, similar attempts have been made by other authors and e.g. Sutanto et al. (2019) particularly suggest to "*consider the vulnerabilities and exposure of the impacts in each [...] region, which can provide a good measure for drought impact forecasting*". In addition, Blauhut (2020) states that "*the single use of impact information has to be seen critical. The information on past impacts merely proxies past vulnerability to drought. It does not inform on potential drivers of vulnerability nor provide an actual state of the present situation. **Accordingly, the impact forcing driver besides the hazard, namely vulnerability to drought, has to be integrated to drought risk analysis***".

2b The potential of indicators as predictors for drought impacts

The authors tested the suitability of different indices to be used as predictors for drought impacts and argued that it takes about 15 to 33 months for droughts to cause impacts (though this number depended on the index under consideration). So, if I understand correctly, in order to calculate drought indices that can be useful to reliably predict impacts, one would need sufficiently long records (i.e., 15-33 months of data). Thus, in practice, I wonder how useful it will be to "predict" potential impacts with help of these indices? I would argue that a region will already suffer from severe impacts after more than 1 year of drought conditions and that – after having lost some harvests or after reaching certain thresholds of low groundwater or reservoir levels – there is no added value of starting to look at the data of the past 15-33 months to try to predict the already ongoing impacts... To me that is in fact the nature of droughts, i.e., that they are considered "creeping disasters" with slow onsets and difficult to predict their magnitude and impacts. Therefore, the real question here still remains: *How can we use drought indices over short(er) periods of time to predict impacts of ongoing and potentially much longer droughts, if the study results suggest that only long-term data can actually be used to predict them?* I guess this is somewhat of a chicken or egg dilemma.

3 Technical Comments/Corrections

- Page 6, line: 164: where did the evaporation data come from, how was it calculated/measured?
- Page 6, line 184: The climate indicators receive only very little attention in the methods, while they are discussed in much more detail in the results. It is actually easy to overlook their short description in the methods part. They should be explained in more details, especially what their abbreviations mean and why they might potentially be relevant as drought predictors
- Page 8, censoring methods: CM1 and CM2 seem too similar to me. To me they are not separate censoring methods, because they actually do not "censor" the given problematic cases in different ways, they simply imply a different way of counting the DIOs.
- Page 9, Figure 1: colour choices are not optimal, there are too many similar colours that are difficult to distinguish.
- Page 13, line 309: perhaps emphasize that drought indicators are actually negative in

case of droughts, which would explain the negative correlations

- Page 13, line 311: NE and CE (the two regions I highlighted above to have the lowest population density) clearly stick out in terms of correlations. The authors should discuss potential reasons for that. Perhaps a closer look at different sectors could help (e.g., less agriculture, less industry in these regions? Or simply less people to notice any impacts?).
- Table 2a/Table 2b and Figure 10: as mentioned above, I think CM1 and CM2 are too similar and, thus, demonstrating the results only for those two cases is not showing the full picture. Especially in Figure 10, it would be very easy to add additional bars (with different colors) for CM3 and CM4
- Page 25, lines 479-481: The statement of drought impacts responding fast to hydrological or soil moisture drought is directly implied by the drought propagation chain suggested by Van Loon and Laaha (2015).
- Page 25, line 491: you cannot be sure that agriculture and livestock farming were the sectors suffering from most impacts, but you can rephrase to that these two sectors were "most frequently reported to be affected". So, my point is that there is a difference between actually experiencing impacts and looking at a list of reported impacts, which could be highly biased.
- Page 26, line 514: authors state that due to the reduced sample size of impact reports, the analysis did not include an investigation of sector-specific impacts. However, the authors include Figure 1, which explicitly lists the impacts by sector and different regions, which becomes somewhat obsolete if there is no subsequent analysis of the different sectors.

4 References

- Blauhut, V., 2020. The triple complexity of drought risk analysis and its visualisation via mapping: a review across scales and sectors. *Earth-Sci. Rev.* 210, 103345. <https://doi.org/10/ghs66t>
- Sutanto, S.J., van der Weert, M., Wanders, N., Blauhut, V., Van Lanen, H.A.J., 2019. Moving from drought hazard to impact forecasts. *Nat. Commun.* 10, 4945. <https://doi.org/10/ggcfxg>
- Van Loon, A.F., Laaha, G., 2015. Hydrological drought severity explained by climate and catchment characteristics. *J. Hydrol.* 526, 3–14. <https://doi.org/10/f7ftzg>

Best regards,

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