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Interactive Comment

Interactive comment on "A quantitative analysis to objectively appraise drought indicators and model drought impacts" *by* S. Bachmair et al.

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Thank you for reviewing our manuscript and pointing out different issues where further clarifications will improve the manuscript. From the comment there seem to be four major concerns, which subsume several individual points:

1) Choice of NUTS1 region level for analysis and impact/indicator aggregation:

The reviewer is certainly right that the NUTS1 region level does not correspond to local-scale information. However, it integrates that information at this scale. Many studies have shown that drought signals (e.g. compared to floods) are regional to large-scale. There have been several studies for the UK and Germany that grouped regions affected by drought based on precipitation, streamflow and groundwater revealing homogenous responses across regions larger than the typical NUTS-1 region (e.g. Hannaford et al. (2011), doi:10.1002/hyp.7725; Burke and Brown (2010), JOH; doi:10.1016/j.jhydrol.2010.10.003). Furthermore, most monitoring and early warning systems cover continental scales and are found useful by a range of users. The target for this study is not the local scale but represents a first attempt at an overview of "ground-truthing" drought indicators with impact information. We can clarify this better in the revised manuscript.

The NUTS1 region level (major socio-economic regions) was chosen because of a lack of sufficient data for analysis with finer-scale resolution. We initially explored the potential of using NUTS3 or NUTS2 level data but data availability did not permit the analyses we conducted in this study using NUTS1 level data. Upscaling to the NUTS1 region level was thus necessary. However, it needs to be pointed out that a large portion of impact reports only makes reference to the NUTS1 region and not to smaller scales. In the introduction we state that "the aim is to develop methods that can be extended to other geographical areas in future applications". In fact, with potentially better data availability in the future the applied methods could be used for more local-scale analyses in addition to further geographical areas. Further data may also allow more detailed analyses for different types of impacts.

We acknowledge the recommendation to explore NUTS1-2-3 level interactions (review comment on "assessment of how many reports would have to actually be prepared (how larger a sample size) would be required in order to resolve some of these impacts at the NUTS2 and NUTS3 level") but refrain from it given the already lengthy paper, as was stressed by reviewer 1. Please note that the numbers in the figures from the Stahl et al. (2012) DROUGHT-R&SPI report, which were presented in the review comment, are outdated by now since many impact reports have been added to the European Drought Impact report Inventory. We also want to point out that we are aware of the effect of drought indicator aggregation to NUTS1 level, as evidenced by the paragraph discussing potential reasons for a low correlation between drought impacts and streamflow/groundwater levels (page 9461); hence we think we are sufficiently transparent about this. Nevertheless, we will add further information regarding the reason for selecting the NUTS1 region level in the methods part (2.1.) and are grateful to the reviewer for pointing out the need to provide this information.

2) Identification of indicator thresholds and their potential use:

There are two points of criticism: first, that the used indicator thresholds solely represent single drought indicators, while a single indicator likely is not sufficient for capturing the multifaceted drought hazard; second, the potential use of the identified thresholds derived for the NUTS1 level may not be relevant for guidance in drought management plans because such triggers should be grounded at the local level.

Regarding the first point of criticism it needs to be clarified that the figures indeed show splitting values during random forest (RF) construction for individual indicators, but this is for presentation purposes; however, we want to emphasize that the models are all based on multiple drought indicators. A tree approach with multiple indicators has, in our opinion, even an advantage over a pre-defined combined drought index. It accounts for multiple conjunctional causality and allows us to describe different combinations of multiple indicators that eventually lead to an impact. Hence, the splitting values per indicator are extracted from models considering multiple predictors and possible interactions, e.g. while for the root node SPI-3 with a certain splitting value may represent the best discriminator, for a finer-split node a different SPI or SPEI accumulation period and corresponding threshold may be selected. Therefore the derived median of the splitting value distribution, which we regard as a threshold representative for impact occurrence, factors in multi-predictor interactions. We hence disagree that the presented thresholds represent single indicators only and that they be omitted from the paper. Instead they represent splitting values that are conditional on other drought indicators as predictors. We thank the reviewer for the valuable comment because this shows that this point was not clear in the paper. While the figures show thresholds for individual indicators we will add that this is just for presentation purposes, because the values themselves come from multi-predictor models.

Concerning the second point we want to emphasize that the purpose of identifying indicator thresholds representative for impact occurrence is to 1) complement and allow comparison with local-scale decision making that is usually based on stakeholder knowledge or the experience of individuals, and 2) to provide an impact-driven perspective of indicator thresholds in addition to common hazard intensity classes 'passed-on' through time (e.g. SPI-n < x demarking mild/moderate drought). We stress that the identified thresholds are by no means meant to replace (or "short cut", as the reviewer stated) drought triggers identified by stakeholders. We will assure that in the revised manuscript this is clear.

The reviewer particularly articulated the concern that the thresholds for streamflow may not be useful because these often correspond to localized impacts. To address this concern we could potentially omit the plots showing streamflow and groundwater level thresholds – depending on other reviews and the editor's decision. The thresholds for meteorological indicators (especially for longer aggregation periods), however, will be informative for impacts that are less localized (e.g. not occurring just within a single river).

3) Identified conclusion:

The reviewer noted that "The real main conclusion of their study is: "Agricultural and hydrological drought impacts were generally best linked to shorter and longer SPI (and SPEI) time scales, respectively. Here, shorter and longer refer to 1-4 (Germany) and 7-8 months (England)." We feel this paper covers more than this brief conclusion but in essence this particular issue is discussed in detail on pages 9460/9461.

4) "The Groundwater Issue":

It is correct that we did not standardize streamflow or groundwater level data but used percentiles instead. However, since we apply rank correlation this does not affect our results. We also discussed possible reasons for lower correlation between impacts and streamflow/groundwater levels in the paper (see comment above for point 1).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 9437, 2015.

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