

Interactive comment on “Estimating drought risk across Europe from reported drought impacts, hazard indicators and vulnerability factors” by V. Blauhut et al.

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Dear referee #1, Many thanks for the positive evaluation of our work. We highly appreciate your explicit comments and suggestions which will clearly help us to improve the submitted manuscript. Please find our response to your questions and suggestions below (we first repeat the referee’s comment in italic and then provide our answer) General comment “However, a brief assessment on the main uncertainties would be beneficial to understand the reliability of main results.” Thanks a lot for raising the issue of uncertainties in the database. In the discussion section, all results are explicitly related to uncertainties in EDII data. We will amend this assessment of uncertainties

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Discussion Paper



with references to Stahl et al 2016 (doi:10.5194/nhess-16-801-2016). The strong imbalance in the distribution of entered reports per country (Fig.1) can be explained by a number of restrictions and biases regarding the availability of information to populate this database: for countries with a wealth of reported drought impacts, often governmental reports are the major contributor whereas countries with fewer impact reports mainly consist of reports by nongovernmental sources as well as internet and (international) media sources. Nevertheless, governmental reporting culture varies across Europe and hence the availability of such reports also differs. Furthermore, the barrier of language may have played a crucial role in the collection process, which was carried out by a group of people limited to searching in only a few European languages, particularly lacking Slavic languages. From a temporal perspective, the availability of data has increased with the emerging prevalence of digital documents and online dissemination techniques during the 20th century resulting in more reports and categorized impacts for more recent droughts. The identification of the time and location of reported drought impacts challenges the resolution and accuracy of the data set. The specific month in which an impact started and ended was often not detailed in the original information source and as a consequence, many database entries only state a season or even only the year of occurrence. Similar to this, impact might occurred very locally but information in the description is only vague whereas a more coarse spatial reference had to be selected. Regarding contents, we assume that differences in translation or synonyms may have introduced uncertainty. Furthermore we identified the correct categorization of reported impacts as the critical step, the reason a moderator performs a screening and quality assessment of the data. Particular comments 1. Regarding the indicators and factors used, while you state which indicators you selected, I don't fully understand how you did the regional and temporal aggregation of indicators. A brief quantitative assessment in this respect would be beneficial. In our revised version we will explain the aggregation procedures of all indices. 2. Please briefly describe the drought indicator aggregation procedure for severity calculation. In our revised version we will also explain the aggregation procedures for severity calculations. 2. Baselines

C7048

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12, C7047–C7053, 2016

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[Full Screen / Esc](#)

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[Interactive Discussion](#)

[Discussion Paper](#)



used to calculate indicators. Indeed, the indicators are based on different baselines. Whereas SPI & SPEI cover 1970-2010, the EDO's indices have a much shorter time span. We will state this more clearly in the revision. 4. Fully named impact categories Will be changed accordingly. 5. Page 12519 lines 16-17: It's not clear what the authors mean with multiple hazard predictor? What hazards? This is misleading indeed and we suggest to change it to "multiple drought indices". 6. Objectives are stated in the paragraph starting at line 17 of page 12519 and somehow repeated in the last paragraph of page 12520. To avoid recurrences, I would recommend to condense these two paragraphs in one, preferably at the end of the introduction.

Many thanks for this suggestion, we will follow your advice and merge the starting line with the formulation at the end of the paragraph.

Page 12522 Lines 25-29: "Drought impact reports stem from various sources and are assigned with a certain level of reliability, decreasing by its enumeration-rank: academic work, governmental reports and documents, reports, media and webpages and other sources." How this reliability was accounted for? Different weights were assumed for each rank?

It was not possible to quantify or account for reliability of different sources, since explicit information on this very special topic are lacking, and arbitrary definitions of weighting schemes are very likely to increase uncertainties. We will add a comment on this fact and consider to add it as a suggestion for further work.

Page 12523 Section 2.2: Different levels of severity are shown as representatives of drought severity. However there is little explain on how drought severity was defined, how it was aggregated in time and space, etc. As far as I understood there is one annual value that defines drought, usually there are single months as stated in Figure 3. These months are related to some drought characteristic, like the peak of the drought, or are just the values of SPEI regionally aggregated? Were other metrics like total area, absolute minimum, etc. tested? A better description on this key issues would be

HESSD

12, C7047–C7053, 2016

Interactive
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beneficial to the understanding on how the functions were built.

Building on a previous study of Blauhut et al. 2015, we tested monthly and regionally aggregated hazard indicators on their ability to predict annual drought impacts. Different possibilities (e.g. the regional Minimum of SPEI instead of the average) were tested, but showing detailed analysis on this would be beyond our scope of the study. In the revised manuscript will describe more clearly how the regional average of the indices were calculated to describe the severity of the drought and define the terminology.

Page 12523 line 16: “indictors” should read as “indicators”

Thank you.

Page 12523 lines 22-25: In this form this affirmation is not completely true. Neither in this work (you stated in page 12524 that a transformation using Gamma distribution was used) nor in McKee 1993, was the SPI computed as the difference from the mean divided by the SD. This is true for normally distributed variables, but usually this is not the case for precipitation.

Thanks for pointing out this mistake. Will be changed in the revised version.

Page 12524, line 11: EOBS-9 was used for a specific reason? Several updates since this version were made to this dataset that now integrates more data and with better quality of information. Do you think that using a different or updated dataset results could change largely?

The choice for the EOBS-9 dataset was made at the beginning of the project time and was used throughout by different partners in different studies in the project. A comparison to the older EOBS-8 dataset at the time showed marginal differences in results. As both the SPI and SPEI are relative measures, they are likely robust to systematic changes in the dataset that do not change the order of the values. As the baseline period would not change with an update we therefore do not expect substantial

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Discussion Paper



changes to more recent releases. Nevertheless, we suggest that will check the most recent data for a case region to see if there are substantial differences.

Page 12526 Section 2.3: I'm not sure if Table 2 is completely necessary. I found a bit confusing that at the beginning 16 vulnerability factors are presented but then in Table 3, 69 factors were selected. Also the header of the Table 2 is not clear, do exposure should be after the line as it is a component? Are drought characteristics the definition of exposure? If you decide to keep Table 2 factors in Table 3 should be associated to any category in Table 2.

Following your advice we will remove Table 2 and add more explanatory information to the respective chapter.

Page 12527 lines 5-18: It's not clear how vulnerability factors with different time steps were used? There are different maps of vulnerability or the last available data was used?

For vulnerability data without available multiple timesteps we used the most recent information for the entire period. Vulnerability data with multiple timesteps was assigned to the year it refers to and all previous years until the next timestep available.(E.g. available timesteps 1976, 1990, 2003 → 1970-1976: 1976; 1977-1990:1990; 1991-2012: 2003). This will be clarified in the revised version.

Section 3: Is there any other function that could be suitable for fitting drought LIO?

We did not explicitly consider or test other link functions given our priority to assess the role of the variables in this paper. However, other studies have considered variations of the logistic regression (Stagge et al., 2015) and tested or used other approaches to link impacts with indices (review by Bachmair, et al. forthcoming in WIRES Water). We will discuss options and our priorities better in the revised manuscript.

Page 12529 lines 10-14: Is unclear how SPEI data was included in the model. I guess that monthly data for each aggregation period was used. Please be more specific.

Full Screen / Esc

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Discussion Paper



Indeed, monthly data for each aggregation period was used. We will clarify this in the revised version.

Page 12532 line 3-6: did the authors checked D(fapar) for the growing season instead of the annual average? This could improve the suitability of the fapar as a predictor.

We agree. We did not analyse the growing season particularly, but found in accompanying studies that monthly aggregations actually do show a better suitability for the timeframe of growing season. However, since the growing season length varies with region across Europe, a seasonal investigating would also require smaller-scale regional models, which at this point was beyond the scope of the study. Nevertheless, we agree that this is an important point that will be added to the discussion.

Page 12532 lines 9-11: SPEI performs better because it has more data availability than the CDI or because it better represent the conditions?

Thank you for pointing out that this should be made clearer. Indeed, we think that the overall better performance of SPEI to the EDO data is caused by data availability. A detailed test of CDI vs. SPEI for only the short period will elucidate this and will be added to supplements.

Page 12537 lines 24-26: “For an application like this one, this can be interpreted in two ways: prior standardisation, composition and weighting appears unnecessary or a composite of factors may well replace the many individual ones.” This sentence seems a bit contradictory in the present form, please consider rephrasing.

We agree and will rephrase this part.

Page 12539 lines 2-7: “Hence, the most relevant SPEI may differ in month selected. This corresponds to different aggregation times, e.g. detected by Lei et al. (2011) for Northern China and Potopová et al. (2015) for Czech Republic for 5 maize. Furthermore, some combinations of selected hazard indicators may have been affected by the criterion of variable independence employed.” This two sentences are not completely

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Interactive Discussion

Discussion Paper



clear to me. Please consider rephrasing them.

We agree and will rephrase this part.

Figure 1. Please review the caption of this figure. It could be shortened as: “Number of annual NUTS-combo scale impacts reported and archived in the European Drought Impact report Inventory (EDII) by European macro region (left panel) and by NUTScombo region (right panel).”

We will change the caption accordingly.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 12515, 2015.

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