

Authors' replies to the Reviewers of HESS-2023-218

Dear Dr. Micha Werner,

Please find enclosed the revised manuscript entitled “60-years drought analysis of meteorological data in the western Po river Basin”. First of all we would like to apologise for the delays in submitting this revised version. All the comments/suggestions raised by the reviewers, in *italic* below, have been addressed in the responses below and accordingly in the revised version. The changes have been highlighted in red and blue in the track-changed revised manuscript (using the track-changed LaTeX software).

We would like to take this opportunity to express our appreciation for the work of the Reviewers who provided very relevant and constructive comments and suggestions. We have addressed all the requests and amended the manuscript accordingly. We hope that the revised document is sufficient to warrant publication in HESS, and we look forward to hearing back from you.

Editor

Dear Authors, we have received the referee reports on the revised manuscript. While both reviewers concede that the manuscript has improved in this revision, they also concur that the scope and scientific contribution of the manuscript is not clear. Several additional comments have also been raised. While I thank you again for your original revision, the issues raised on the scope are as yet not sufficiently addressed. In particular one of the reviewers provides some quite clear directions on how the scope could be clarified. I would therefore request you to consider these comments, and provide a clear response to the concerns raised.

Indeed, the provided comments and suggestions led us to considerably reshape the manuscript in order to better clarify the scope of the research and the innovative aspects of the analyses. In summary, three major changes have been made to the manuscript (in order of significance):

- 1) The objectives of the work have been better declared in the introduction by stating three research questions, which are then answered in the concluding section. The methods and result sections are also related to the research questions thus improving the readability and

clarity of the manuscript. A small change in the title has been made to indicate that the analyses are made on meteorological data.

2) Some of the analyses, for instance the preliminary analyses on precipitation and temperature data, have been removed. They were provided in previous versions of the paper for completeness but we agree that, not being directly related to the research questions, their presence would distract the reader from the focus of the work.

3) The limitations of the analyses, for instance regarding the temporal homogeneity of the used dataset and the limitations of detection studies respect to attribution ones, are now better declared in the text of the paper (see responses to the Reviewers).

Reviewer #1

This is my second review of the manuscript “60-years drought analysis of meteorological data in the western Po river Basin”.

The changes made the manuscript much clearer and easier to read. The methodology and the figures have been clarified and I believe the manuscript is now reproducible.

However, I still believe that the manuscript should aim for a clearer contribution to hydrology and earth system sciences. The conclusions have to be relevant for the hydrological/meteorological sciences in general rather than only for the current study area, which I don't think is the case here. For this, one idea would be for the manuscript to go beyond a simple correlation analysis (between trends and elevation) and analyze the physical link between those variables, that is, the causal association between them, to convince the reader that the conclusions found here can be extrapolated to other regions. There's a minor typo in the title, which should be “60-year drought analysis” rather than “60-years drought analysis.”

We acknowledge that the overall aim and contributions of the previous version of the manuscript were not conveyed appropriately and therefore the contribution to hydrology was unclear. We have therefore reworded the introduction and conclusions of the paper by stating three research questions which are then answered using evidence from the case study. The three questions are:

- 1) Are there temporal trends in drought indices such as SPI and SPEI, and how do these trends translate into changes in the characteristics of drought events, in terms of duration, severity, and intensity?

- 2) Is there a relationship between drought trends and topographical characteristics of the landscape? And if so, is elevation the topographical variable most correlated to these trends?
- 3) Do these conclusions change if drought events are defined at different spatial scales?

Even though the answer to these questions are given by referring to the particular case study, we believe that they may be of general interest and trigger further research to confirm or refute the generality of the findings. For instance, responding to question 1, we found that despite the worsening of drought conditions related to precipitation and temperature being clear, the effects on the characteristics of individual drought events are weaker. It would be of interest if this is the case also in other studies and why. In responding to question 2, we find that terrain ruggedness is better correlated to temporal drought dynamics than elevation, which has been proposed in other studies. Therefore mean elevation, although certainly a variable to be considered, shouldn't be the only topographic variable taken into account in drought change studies. We agree with Reviewer #1 that attributing this correlation to physical causes, e.g., change in atmospheric circulation behavior over the studied region, would have been even better but this should be done together with meteorologists and could be the objective of further research, beyond the scope of this paper. Finally, in responding to question 3, we find that drought characteristic changes at local and regional scales are different. While locally drought periods obtained from SPEI series show more pronounced increases in severity, duration and intensity than those obtained from SPI series, drought events at a region-wide scale show more marked shifts in severity and duration for SPI than for SPEI, denoting a more significant influence of regional precipitation patterns than of temperature on droughts at a regional scale. This is a non trivial result that could trigger further research in other regions. Also in this case the attribution to physical causes would have been better but, as stated above, beyond the scope of this paper.

In brief, even though the objective of attributing to physical causes the behavior obtained by our analysis, as suggested by Reviewer #1, is indeed the right final goal, we believe that the detection analysis done in our paper is anyway useful to the hydrologic community in raising interesting questions about the connection between different drought characteristics and their change at different spatial scales and in complex terrain settings. Analogous analyses in other regions of the world could provide information for attribution studies based on comparative hydrology (Falkenmark and Chapman, 1989, ISBN: 9231025716; Blöschl et al, 2013, <https://doi.org/10.1017/CBO9781139235761>).

Reviewer #2

The overall aim and added value of the paper is still unclear. It seems that the main novelty is the analysis of drought characteristics in relation to terrain ruggedness. However, a large part of the methods and results discusses analyses that don't seem to contribute to this aim. The parts of the analysis that do contribute to this aim are a bit hidden in between the other analyses.

Reviewer #2 is right. Our revision of the first version of the manuscript has biased the discussion toward one of the objectives of the paper, i.e., the correlation analysis between drought (change) characteristics and terrain ruggedness, thus resulting in an unclear scope. In this version, we state more clearly the objectives of the paper by stating three research questions in the introduction section that should help the reader understand why certain analyses have been done. The three questions are:

- 1) Are there temporal trends in drought indices such as SPI and SPEI, and how do these trends translate into changes in the characteristics of drought events, in terms of duration, severity, and intensity?
- 2) Is there a relationship between drought trends and topographical characteristics of the landscape? And if so, is elevation the topographical variable most correlated to these trends?
- 3) Do these conclusions change if drought events are defined at different spatial scales?

The Discussion section in the paper summarizes now the answers to these questions.

While the changes related to terminology make things more clear, the results are still rather difficult to follow and interpret. The paper reads more like a subsequent application of many different statistical analyses rather than a coherent methodology to investigate the differences across terrain ruggedness. It seems that many of the reported results could be moved to the supplementary material, since they don't contribute to the overall aim and don't show significant results. In addition, throughout the results elevation and terrain ruggedness are reported intermittently and are sometimes mixed up. Sometimes they are compared to each other and in other cases not, without an apparent reason. The results present several different versions of mean drought characteristics, sometimes referring to a mean across all pixels and sometimes referring to a mean value across different drought runs in one pixel. This is very confusing and it is difficult to keep track of what kind of mean a specific section is talking about.

We hope that the rewriting of introduction and conclusions, plus the clarifications in the other sections of the paper allow now an easier readability of the paper. As said above, the research questions asked are more than one and the analyses made are instrumental in tackling them. Nevertheless, we have removed some of the analyses which are not explicitly related to the research questions, such as the analyses on precipitation and temperatures that were done before calculating the drought indices.

Regarding the analyses on the connection between elevation and ruggedness and droughts, we acknowledge that the previous version of the paper was sometimes confusing and inconsistent because the results obtained stratifying drought (change) characteristics based on the two variables were not always reported, giving more space to the ruggedness. In the new version of the paper, we show all analyses for both elevation and ruggedness and we show that, although both are correlated to drought characteristics and their change in time, terrain ruggedness is a better predictor than mean elevation, and therefore a potentially useful variable to be considered in other drought change studies.

Overall, I do believe that the paper could make a valuable contribution. However, the presentation still needs to be improved. It would be good if the authors present an overall methodology, detailing why they are doing each of the different tests and analysis and how this contributes to the overall research aims (which seems to be investigating the influence of elevation terrain ruggedness). Similarly the results could be presented in a more coherent way, with a focus on the overall research aims.

We thank a lot Reviewer #2 for this suggestion that we have tried to follow in the best way we could. Indeed the previous version of the manuscript had relevant clarity issues that, we believe, have been resolved with the revision made.

Some specific remarks:

The added value of the regional drought analysis is not clear to me. There don't seem to be any interesting results related to this analysis and this analysis does not contribute to the aim of the paper which is to look at differences in changes in drought across differences in terrain ruggedness.

The relevance of the regional drought analysis is explained better in the revised manuscript. One of the research questions (the third one) is dedicated to whether different results may be obtained by conducting drought analyses at the local and regional scales. Interestingly, drought characteristic changes at local and regional scales are different. While locally drought periods obtained from SPEI series show more pronounced increases in severity, duration and intensity than those obtained from SPI series, drought events at a region-wide scale show more marked shifts in severity and duration for SPI than for SPEI,

denoting a more significant influence of regional precipitation patterns than of temperature on droughts at a regional scale. This is a non trivial result that could trigger further research in other regions.

Lines 50-54: “On the other hand, studies considering temperature values have consistently shown rising temperatures, and thus a rise in evaporative demand, to be a main factor in drought increase, even when significant changes in precipitation patterns were detected.” Which studies? This needs references.

Reviewer #2 refers to lines 35-37 of the manuscript after the first round of revisions. The sentence refers to those studies cited in the previous sentences that have also considered temperatures, and therefore the SPEI, in detecting drought trends. We have rephrased the sentence to “Among these studies, those also considering temperature values consistently showed rising temperatures, and thus a rise in evaporative demand, to be a main factor in drought increase.”

Methods: I still don’t see the added value of using the gridded data and analysing 227 grid points instead of analysing the station data at the 200 stations. The authors mention that it is difficult to attribute stations to the different regions, but it seems that you would know the elevation of each station, so it should be possible to assign the stations to a region? If instead the problem is that many of these stations have a time series that is too short, this should be highlighted in the manuscript more clearly and the implications for the results of this study should be discussed. Then the statement that the dataset is based on 200 stations is not entirely accurate and this could affect the results of your analysis. If the interpolated dataset is based on only 25 stations or a bit more for the period before 1990 and only after 1990 the dataset increases to 371 (from the manuscript or appendix it is not clear how many stations were available in each year exactly) then how will this affect the comparison of the drought event characteristics before and after 1990? Similarly, when evaluating the differences in drought and drought characteristics across the region, this may be heavily influenced by the stations that were available for the interpolation in each particular year. In the earlier years, with less stations you would probably expect variability to be lower than when you have more stations available for the interpolation. It seems like you are hiding some of the limitations of the dataset by using the interpolated dataset and not considering the effects it may have on your analyses.

This is indeed a relevant comment that should be discussed in the paper. Information on the temporal evolution of the number of stations is available at the url <https://www.arpa.piemonte.it/scheda-informativa/spazializzazione-dei-dati-temperatura-precipitazione-griglia>. Regarding precipitation the number of stations has been relatively high throughout the whole period. Regarding temperature indeed the number of stations is

low before 1990, but their information has been added to ERA40 data as a background information. We have accepted a compromise by using this official database for the region, which is more detailed and accurate than any other gridded database of precipitation and temperatures available in the area (and for larger areas). The advantage of using this database is the availability of spatially consistent information for a long time period (1950s to 2020s). The drawback is that being an interpolated product, the change in the density of the ground stations, which has been significant in the years, may have had an effect on the results, particularly on extremes occurring locally. We now discuss the issue in the revised manuscript. Nevertheless we believe that the advantages of having a long-term database are superior to the disadvantages due to its potential lack of homogeneity. We couldn't conduct a sensitivity analysis because we do not have access to the station data used to produce the gridded product (we can download the station data only for the most recent period, 2000s-2010s). Appendix A provided the information we could retrieve about the construction of the database. Being work done by others, we prefer in the new version of the paper to remove Appendix A and referring to the original documents in the text.

In the methods, line 146-147, the authors mention dividing the area in four groups based on both elevation and terrain ruggedness. However, it seems the groups are based only on terrain ruggedness? The four groups based on elevation are not reported anywhere.

Reviewer #2 is right (even though the lines are 125-127). The figure just reports the classification by terrain ruggedness, while also showing the corresponding mean elevation. We have rephrased the sentence and the figure caption to avoid confusion. The sentence now reads “The landscape is classified in areas with similar topography. Four distinct areas of an almost equal number of cells are identified based on terrain ruggedness, which represent the plains, the hilly region, and the lower and higher mountains respectively. Figure B1 shows the classified areas and the fact that mean elevation and terrain ruggedness are highly correlated. However, the advantage of using terrain ruggedness over mean elevation is that, in our study area, the hills in the center-south of the region are distinguished from the eastern flat part of the region, despite having similar mean elevation.”

The description of the trend analysis in 2.4.3 is still not clear. Here you are describing the steps for pre-whitening methods mainly. Please explain the trend analysis (e.g. Sen's slope and Mann-Kendall test) in more detail, this may not be trivial for every reader.

A brief explanation of the Sen's slope and Mann-Kendall test has been added to the text. The following sentence has been added: “The trends are estimated using the Theil-Sen slope estimator (Theil, 1950; Sen, 1968), i.e., by calculating the median slope between the

indices values for all possible month pairs. The significance test is performed through the Mann-Kendall test (Mann, 1945), which is a non-parametric (distribution-free) alternative to the linear regression slope test available in regression analysis. To improve the power of the test, deseasonalization and pre-whitening of the data are performed.”

Methods section 2.4.4: Although you have changed the naming to a local drought analysis, you may still want to indicate at the start of the paragraph, that this analysis is performed pixel by pixel.

Done, thank you.

Methods section 2.4.5. From this section, it is still not clear to me whether by region you mean the entire study area or if you analyse “local regional” droughts, i.e. local areas of multiple cells that are experiencing drought. It does become clear later on, but should also be mentioned here.

Thank you, we rephrased the sentence as “In contrast to local droughts, which are calculated from a series of index values belonging to one cell, region-wide drought events are evaluated by considering what happens in the entire region.” Then the procedure is explained in detail.

Line 269-270: “used in the cited papers”, this is not clear, please just cite the proper references here, e.g. used by Name (year).

Done, thank you.

Results 3.1, at the end of the paragraph, lines 340-344, the authors state that the results agree with the results from other studies with the same data set. So why are the authors redoing this analysis? The added value is not clear to me.

The precipitation and temperature trend analyses were done for completeness but have now been removed from the paper since not directly related to the three research questions stated in the introduction. The sentence has therefore disappeared from the revised manuscript.

Results 3.2, line 374-376. This line refers to Figure 6 which shows the division according to terrain ruggedness, yet this line mentions a difference between altitudes. Should this be terrain ruggedness?

We guess Reviewer #2 refers to lines 308-309 (not sure why we have a different numbering) where the sentence was “...despite the trend in annual precipitation being not significant and the temperature trends having a lower slope coefficient than at higher altitudes (Figure 6)”. Indeed the sentence was unclear. The revised manuscript, heavily revised in this part,

does not include the sentence anymore. Figure 6 has been removed and only part of it is now included in the new Figure 3.

Figure 6 shows trend analyses for the different groups, but this analysis is not described in the methods section (i.e. how are you calculating the group mean prec, max T, min T, etc.?)

Figure 6 has been removed and only part of it is now included in the new Figure 3. The classification by terrain ruggedness of the indices is explained in the Figure caption by the sentence “Trend analysis on drought indices calculated from data belonging to areas defined by terrain ruggedness inside cells. The colour of the circles represents the slope coefficient of the trend, while the inner radius of the circles represents the significance of the trend (a smaller inner radius represents a more significant trend). The black circles denote a significance level of 5%.”

Results 3.3.1 Here correlations with elevation are discussed and the correlation with terrain ruggedness only briefly mentioned, even though the initial aim of the paper is to show the importance of including terrain ruggedness instead of elevation.

The correlation of local drought characteristics with mean elevation and terrain ruggedness is not very different. Terrain ruggedness seems to be more significantly correlated with runs characteristics evaluated with indices applied for the long (12 month) duration. For the short (3 months) duration perhaps mean elevation is better (see new Table 1). For changes in run characteristics, instead, the correlation with terrain ruggedness is always superior to the correlation with mean elevation (see new Table 2).

Figure 7 is discussed in section 3.3.2, after the discussion of tables 1 and 2, but the figure comes before the tables, this is quite confusing.

Right. Now Figure 4 (what was Figure 7) comes first and the tables next. LaTeX is to blame :-)

The caption of figure 7 is a bit confusing. All four subfigures seem to show decreases and increases (with a downward or upward arrow), but the caption says a and b show a decrease and c and d an increase? Also, the caption would be more clear if the sub figures are discussed in alphabetic order (instead of a, c, b, d).

We agree, the caption has been reworded.