We present here a point-by-point response to the comments made by Reviewer #1 and #2. Other than the changes described in the following, we have performed minor editing on the manuscript (visible in the track-changes file), rephrasing some paragraphs and correcting minor mistakes.

Response to reviewer #1

We thank Reviewer #1 for the detailed analysis and the precise comments about our methodology and presentation/discussion of the results. We respond here to the more specific concerns raised and how they are addressed in the revised manuscript. In the following, Reviewer #1 comments are in plain text, while our comments are in italics.

- The findings are not contrasted with other studies and the novelty for hydrology and earth system sciences is unclear.

The Discussion section has been expanded by including a comparison between the presented study and similar ones found in the literature, indicating the novelty of our study and the originality of its results (see Lines 485-501 of the revised manuscript). In particular, we state that "[our] type of analysis is in common with a growing body of literature focused on the elevation effects on drought characteristics" (Line 488-489 of the revised manuscript), and that "our study shows that mean elevation, although certainly a variable to be considered, shouldn't be the only topographic variable taken into account" (Line 495-496 of the revised manuscript) given that "[i]n our analysis, using [a] different classification leads to stronger correlations between drought characteristics and topographical characteristics" (Line 499-500 of the revised manuscript).

- Trend attribution is not investigated or discussed.

Although the attribution of the identified trends is surely an interesting issue, it is out of the scope of the present paper, which is focused on trend detection for both drought indices and drought event characteristics. We have however now mentioned the attribution issue in the discussion section, as a possible further development of the research. The following lines have been added (Lines 502-504 of the revised manuscript): "Although strong correlations between drought trends and the mean elevation and ruggedness of the terrain are found, attribution of these results to physical phenomena is not straightforward. The presented methodology doesn't focus on this aspect and, given the complexity of the involved phenomena, attribution is outside the scope of our study".

- The first paragraph of the Introduction is too general and not directly associated with the aim of the study. It would read better if presented more concisely.

The paragraph has been rewritten and made more concise, see Lines 19-23 of the revised manuscript.

- Line 36 needs citations for "changing patterns of meteorological droughts"; and also for "an increase in drought occurrence in the area has been detected".

The phrase "changing patterns of meteorological drought" was intended as a summary of the aim of the cited studies (i.e. trend detection, changes in drought characteristics...); in order to avoid confusion, it has been removed. The paragraph has been rearranged to make clearer that the "increase in drought occurrence" is a summary of the results in the cited literature, see Lines 26-37 of the revised manuscript.

- Lines 36-43 are unclear. When and where are such changes detected? How do the "reported changes differ significantly"? How consistent are the following results: "studies considering both precipitation and temperature (Hanel et al., 2018; Falzoi et al., 2019; Arpa Piemonte and Regione Piemonte, 2020b; Vogel et al., 2021; Baronetti et al., 2022) have found more consistent results"? Why is "the rise in evaporation as a main factor in drought increase"?

•

Lines 31-37 of the revised manuscript have been changed to address the Reviewer #1 comments. In particular, Lines 36-43 of the old manuscript have been changed in order to convey the area and the time period to which the cited results apply ("Overall, these studies have found an increase in meteorological drought occurrence in north-west Italy, particularly after the 1970s"). Furthermore, the cited significant difference in the reported precipitation change refers to the presence of either summer or winter precipitation decrease, and so this has been made more explicit ("On the other hand, studies considering temperature...). The consistency of the results related to temperature increase (and thus to evaporative demand) is now mentioned ("consistently shown rising temperatures, and thus a rise in evaporative demand, to be a main factor in drought increase"). Finally, an erroneous citation (Baronetti et al., 2022 instead of Baronetti et al., 2020) has been corrected.

• Lines 48-49: What are the results conflicting and what is the consensus?

The conflicting results are the presence of a relation between the latitude and the temperature trend, while the cited consensus is the presence of enhanced warming at higher altitudes; Lines 48-49 (old manuscript) have been modified in order to make them clearer: "In general, despite conflicting results regarding the presence of an elevation effect on warming rates and the lack of adequate climate data for mountainous regions, a consensus on enhanced warming rates at higher altitudes emerges" (Lines 42-44 of the revised manuscript).

• Lines 51-54: When and where did these studies investigate? Does it also cover the study area of the current manuscript?

The meta-analysis mentioned at Lines 52 (Pepin et al., 2022) is based on studies from both station data and gridded datasets related to various mountain ranges (including the Alps); the study period also varies widely, ranging from the early 1950s to the end of the 2010s, with varying study periods lengths. Lines 52-53 of the old manuscript have been revised to mention this information: "A comprehensive meta-analysis of both in-situ studies of precipitation data from mountainous regions (including the Alps) and of global gridded databases from the early 1950s to the late 2010s reported a relative decrease in precipitation compared to lowlands, although without high confidence" (Lines 46-48 of the revised manuscript). The study of Giorgi et al. (2016) focuses on the western part of the Alpine range and compares climate models (in the present, near-future and future) at different scales using high resolution observation data (for the 1975-2004 period) as a baseline. The fact that this study does include the present study area is mentioned in the revised manuscript: "Furthermore, analyses such as Giorgi et al. (2016) have shown the importance of spatial resolution in understanding these processes in topographically complex regions, reporting that increases in summer precipitation in higher elevation areas of the Alpine range could only be detected by high resolution regional climate models and observed by high resolution observation networks." (Lines 47-51) of the revised manuscript).

• Lines 58-60: Why did the authors choose the present study area? For example, does it have longer or more extensive observed data sets to analyze the effect of elevation on meteorological trends? Please specify that only meteorological droughts are investigated. It is also proposed to contrast the drought conditions in northern Italy to those in northwestern Italy, but the manuscript only presents for northwestern Italy.

We opted for North-Western Italy due to our familiarity with the region (given our work in Turin) and the significant institutional and public concern regarding the onset and characteristics of droughts, particularly following the unprecedented drought of 2022 in the area. Lines 58-60 of the old manuscript have been changed to address Reviewer #1's comments: "Understanding the possible effects of topographically related phenomena on drought conditions is thus of particular interest in an area such as the western Po river basin, which comprises both wide plains and high mountains. Despite the presence, as detailed above, of studies on drought in the chosen region, these lacked either the needed spatial resolution or focus to evaluate possible effects of terrain characteristics on drought conditions." (Lines 52-55 of the revised manuscript).

Furthermore, we propose to change the title to "60-years drought analysis of meteorological data in the western Po river basin" in order to convey the meteorological drought focus of the article.

• Figure 1. What are the elements shown in the larger map (colors, lines, names)? Is it elevation, land cover, rivers, roads? Every element should be either described or removed from the map. The river network should also be included, as many readers may not know the extent of the Po river. Roads and region names that are not relevant to the study should be removed. Also, the same projection should used in all maps, so that the shape of the study area (and the latitude-to-longitude ratio) on the larger and smaller maps are the same. Similar reasoning applies to the other figures.

Figure 1 has been modified according to Reviewer #1's suggestions.

• Line 71-72. Do you mean "bordered by France on the west and south-west"? And "two other Italian regions (…) on the east and south-east?

The reported typo has been corrected, see Lines 69-71 of the revised manuscript.

• The latitudes described in lines 76-77 and 96 are not needed and can be inferred from Fig. 1.

The latitude/longitudes values have been omitted, see Lines 69-72 of the revised manuscript.

• A description of the data projection in Line 96 is not needed. The total number of grids described in lines 96 and 106 are also not needed because the spatial resolution of the data is already provided.

The number of grid points cited at Line 96 of the old manuscript has been removed (see Line 93 of the revised manuscript). The number cited at Line 106 of the old manuscript has not been omitted as it refers to the number of actual series studied, meaning points in the dataset actually falling inside of the studied domain.

• Please provide a reference to the data set (Line 95) and the interpolation method (Line 97).

The references to the dataset and to the interpolation method used have been added, see Lines 91-2 and 95 of the revised manuscript.

• Section 2.2. Why are these data explored and how do the results change if other data sets are analyzed? Do the data explored have a dense gauging network? How many gauges does it include? It is important to provide specific data details due to the small study area and the high result sensitivity to different data sources (cited in the Introduction).

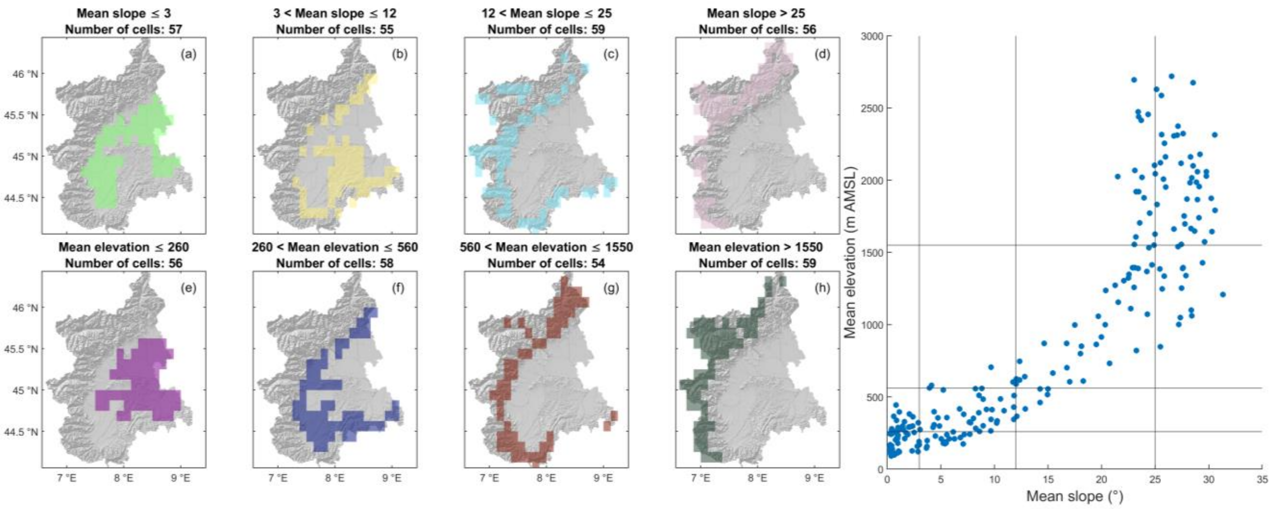
Our choice of dataset is functional for the purposes of our study, because of its spatial resolution and its observation-based nature. As detailed in Appendix A, the interpolation method used for the dataset doesn't add or remove elevation trends and is thus suitable for studying the relations between meteorological variables and terrain characteristics. Furthermore, the dataset has a much higher number of gauging stations in the region compared to other datasets, with hundreds of stations in the area (see https://doi.org/10.5194/nhess-13-1457-2013). More details on the distribution of stations over the domain and their number, as well on the density of the chosen dataset compared to other available ones have been added in the revised manuscript: "The data used in the interpolation method is provided by a dense gauging network (roughly 200 stations) covering both low and high altitude areas, providing a much higher number of stations than other available datasets for the area (Turco et al., 2013)." (Lines 99-101 of the revised manuscript).

• Figure 2. Is “Terrain roughness” the same as “Elevation standard deviation”? Also, is it “terrain roughness” or “terrain ruggedness”? It would be nice to standardize throughout the text. Please clarify the units of “Terrain roughness”. What does F and F(x) refer to?

Terrain ruggedness is calculated as the standard deviation of the elevation inside a cell, and as such has a unit of meters; to avoid confusion, the term “elevation standard deviation” is not used outside of the explanation at Lines 110-111 of the revised manuscript. Furthermore, the use of the term terrain ruggedness (sometimes cited wrongly as “terrain roughness” in the old manuscript) has been standardized throughout the manuscript. Finally, F(x) referred to the empirical cumulative distribution function of terrain ruggedness values, and this has been made explicit in the revised figure.

• Lines 113-116. What is the definition of terrain ruggedness (its concept) and how is it calculated? Please provide precise details in the main text. Also, why did the authors choose the present metric and how does it compare with other metrics (e.g., terrain slope, amplitude, other terrain ruggedness indices)?

A definition of terrain ruggedness and citations for how it can be calculated are now provided in the revised manuscript: “The terrain ruggedness (also known as surface roughness or topographic hetereogenity) is defined as the "deviations in the direction of the normal vector of a real surface from its ideal or intended form" (Whitehouse, 1994), meaning the irregularity of a landscape” (Lines 108-110 of the revised manuscript). The choice of terrain ruggedness as a variable is motivated by a need to differentiate areas with distinct terrain characteristics (plains, hills and mountains) which would have been grouped together if classified through elevation bands. We acknowledge that other variables could have been used, such as those proposed by Reviewer #1, but the impact of a similar classification would not change the results. As an example, the following Figure shows the results of a classification based on mean slope inside a cell, obtained from the same DEM as the terrain ruggedness (this figure can be compared with Figure 2 of the revised manuscript):



• Line 113: “height”. Do you mean “elevation”?

The use of elevation instead of height has been standardized throughout the revised manuscript.

• Lines 116-118 are a repetition of the previous section. Either make it more concise or remove the sentence.

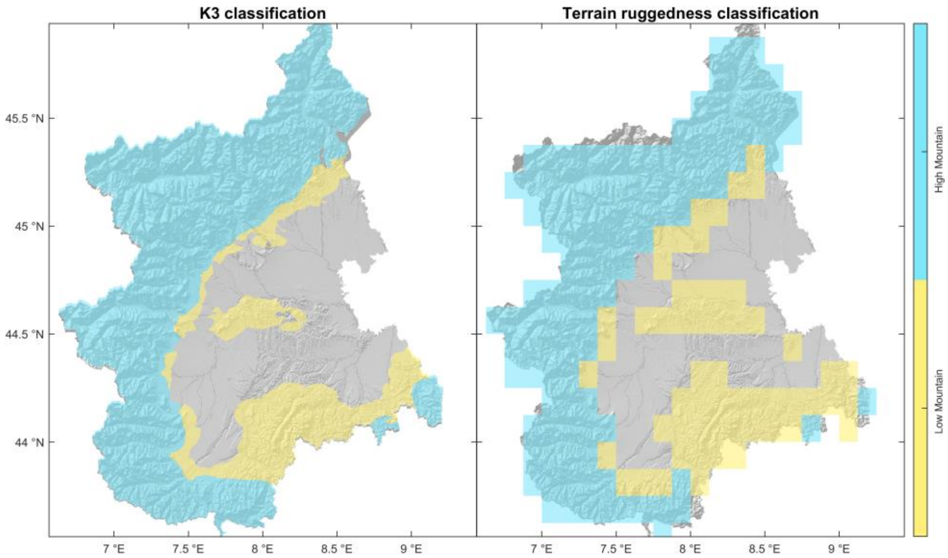
The lines have been removed.

• Lines 120-123. A description of the results is not needed here. Can remove these sentences.

The Lines have been removed in the revised version of the manuscript.

• Lines 124-127. I could not find the results compared with the K3 Mountain classification. How did the authors find that the classification is "quite satisfactory"? How satisfactory is it?

The classification has been deemed satisfactory as it correctly distinguishes between the hilly region at the center of the domain and the surrounding plains, as well as correctly identifying mountainous areas, as can be seen in the following figure comparing the terrain ruggedness classification and the K3 Mountain classification.



• Table 1. These thresholds were not used or discussed in the manuscript. Thus, the table could be removed.

Table 1 has been removed, and the only reference threshold (SPI < -1) has been maintained in the text: "a series of consecutive months under a certain threshold (-1, corresponding to a moderately dry condition in the SPI classification)" (Line 193-194 of the revised manuscript).

• Lines 157-159. These sentences are unclear.

Lines 157-159 of the old manuscript report the way in which the parameter of the gamma distribution have been estimated and the normal inverse function has been calculated for the current study. This was made explicit because these two steps are usually performed via linear approximations given in the literature rather than with dedicated software functions.

• Line 164. Is it potential evaporation?

ET_0, in the context of SPEI calculation, refers to the "Reference Evapotranspiration", meaning the maximum evapotranspiration from a reference well-watered reference "alpha-alpha grass" surface (see https://doi.org/10.1016/j.agwat.2020.106043 for the differences with potential evapotranspiration, and https://doi.org/10.1002/joc.3887 for the reasons for its use in the SPEI calculation).

• Line 184. How were the series deseasonalized? With "seasonal precipitation series", do you mean the series for each season, or the series with the seasonal time scale?

Deseasonalization was done using a function in the Climate Data Toolbox (https://doi.org/10.1029/2019GC008392), which removes the mean of the detrended series for each month. The missing citation for the method has been added: "Furthermore, deseasonalization is performed by subtracting the mean of the detrended temperature series for each month using the Climate Data Toolbox (Greene et al., 2019)." (Lines 177-179) of the revised manuscript. Seasonal precipitation series were meant as the series of cumulative precipitation values for different seasons; this definition is now provided at Lines 175-177 of the revised manuscript: "Seasonal values are defined as the cumulative precipitation values and the mean temperature values over the four three-months periods December-January-February (Winter), March-April-May (Spring), June-July-August (Summer), September-October-November (Fall)".

• Figure 3. I could not find DDr and DSr in the figure.

Figure 3 has been changed and now includes DDr and DSr.

• Lines 200-202. It would be nice to clarify the differences between Fig. 3a and 3b. Also, please define precisely when the drought ends in each case. How sensitive are the results for different thresholds in defining when the drought ends?

A more detailed description of the method used for the definition of drought runs has been added to the revised manuscript: "The differences between the use of a single threshold and the inclusion of the run onset and offset are shown in Figure 3: the method used in the present study considers two periods of months with index value under the -1 threshold as part of the same run if the drought index remains lower than 0 between them; in any case, a drought run ends if the drought index becomes positive." (Lines 196-199 of the revised manuscript). The use of a different threshold for the definition of the end of local droughts were considered, both using a simple –1 threshold (as in Figure 3 (a) of the revised manuscript) and the inclusion of months with negative index values only after a month under –1 threshold. Results for this latter method differ very slightly from the ones presented in our study. As an example, the Pearson correlation values between the terrain ruggedness and the change in drought run characteristics obtained with this method are reported in the following table (these values can be compared with those of Table 3 of the revised manuscript):

	Δ Number of runs		**Δ Mean DS_R**		**Δ Mean DD_R**		**Δ Mean DI_R**	
	C	**p-value**	**C**	**p-value**	**C**	**p-value**	**C**	**p-value**
SPI-3	0.18	5.22E-03	0.43	1.62E-11	-0.35	7.73E-08	0.40	4.09E-10
SPEI-3	0.23	5.57E-04	0.37	1.26E-08	-0.33	3.10E-07	0.15	2.37E-02
SPI-12	-0.35	5.37E-08	0.24	2.09E-04	-0.26	9.66E-05	0.17	9.38E-03
SPEI-12	-0.30	6.35E-06	0.32	1.13E-06	-0.29	7.84E-06	0.32	8.67E-07

Results obtained using a single threshold differ more significantly, especially regarding the mean drought characteristics value, but the overall results still support the conclusion presented in our study (relative values of drought characteristics between SPI and SPEI, change of drought characteristics over time and correlation between drought characteristics and terrain characteristics). As an example, the Pearson correlation values between the terrain ruggedness and the change in drought run characteristics obtained with a simple threshold method are reported in the following table (these values can be compared with those of Table 3 of the revised manuscript):

	Δ Number of runs		**Δ Mean DS_R**		**Δ Mean DD_R**		**Δ Mean DI_R**	
	C	**p-value**	**C**	**p-value**	**C**	**p-value**	**C**	**p-value**
SPI-3	0.20	2.89E-03	0.59	5.09E-23	-0.57	1.24E-20	0.64	9.01E-28

SPEI-3	0.14	3.75E-02	0.52	7.75E-17	-0.48	2.03E-14	0.54	1.99E-18
SPI-12	-0.36	2.52E-08	0.09	1.62E-01	-0.08	2.30E-01	0.16	1.88E-02
SPEI-12	-0.28	1.77E-05	0.30	5.82E-06	-0.25	1.88E-04	0.35	5.07E-08

- Line 213. “discarded”. Do you mean “analyzed”?

The line referred to the fact that the criteria of a minimum duration of three weeks for drought events, as proposed in the literature, is always met in this study given the use of monthly data. Lines 213-214 of the revised manuscript has been changed to better convey the intended meaning: “the minimum duration threshold of 3 weeks, used in the cited papers, is always met as monthly data is used in this analysis”.

- Sections 2.4.4 and 2.4.5. I find it hard to distinguish between “drought run”, “drought event”, “drought episode” in the Methodology and the Results sections. It might be clearer to refer to droughts in a single cell as “local droughts” and in multiple cells as “regional droughts”, or some other term related to the spatial differences. Please standardize throughout the manuscript.

The use of the terms “drought run” and “drought event” has been changed in the revised manuscript following Reviewer #1’s comment, now referring to them as “local droughts” and “region-wide drought events” to better differentiate between the two. Furthermore, the pedex R previously used to denote the drought run characateristics has now been changed to L in order to better reflect the local level of the analysis.

- Figure 4. (a) Should also be presented and discussed in relative terms (units of % per year relative to the long-term precipitation). The negative values in the colorbar of (b) and (c) should have a smooth transition from zero, symmetric to the positive values. The colorbar in (a) is ok. Please label the numbers in the x and y axis (◦N and ◦E) and use the same projection as Figure 1.

Figure 4 has been changed according to Reviewer #1’s comments. The change in terms of relative precipitation change is now discussed at Lines 261 and 262-263 of the revised manuscript.

- Line 251-252. Figure 4 does not present seasonal trends.

References to the Figure 4 and supplementary Figure B1 have been revised in order to avoid confusion, see Lines 255-256 of the revised manuscript.

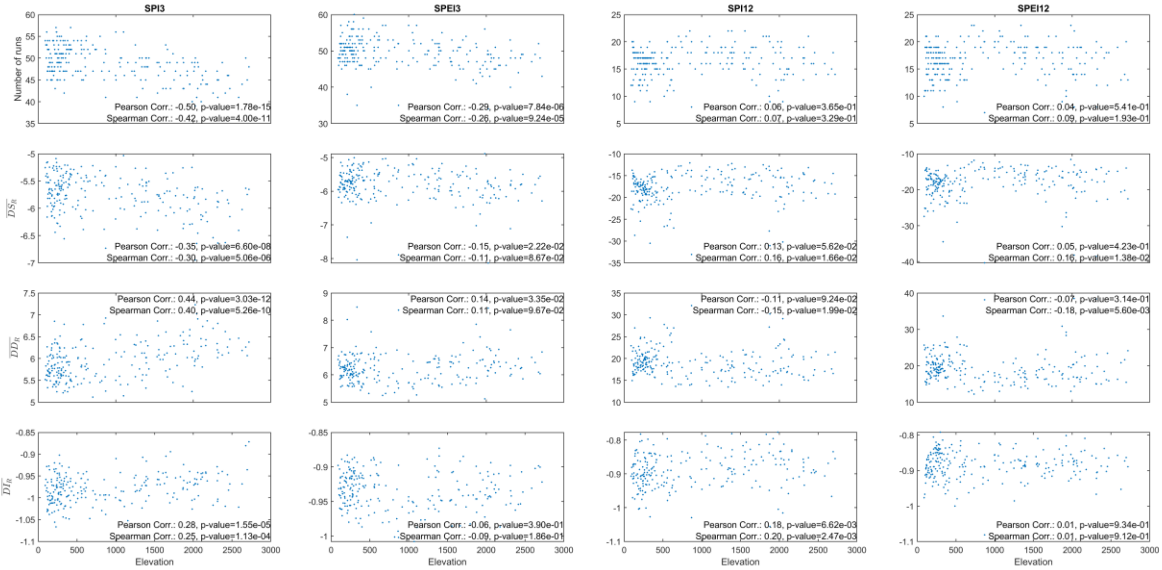
- Line 255-257. Do the authors mean that precipitation trends are not the same in the entire area?

When trend analysis is performed on the spatial average of the precipitation over the whole study-domain no significant trend is found. Lines 255-257 of the old manuscript were meant to convey this, and they have been revised in order to be clearer: “Still, spatially averaged precipitation over the whole region does not show a statistically significant decrease either at the annual or seasonal scale.” (Lines 262-265 of the revised manuscript).

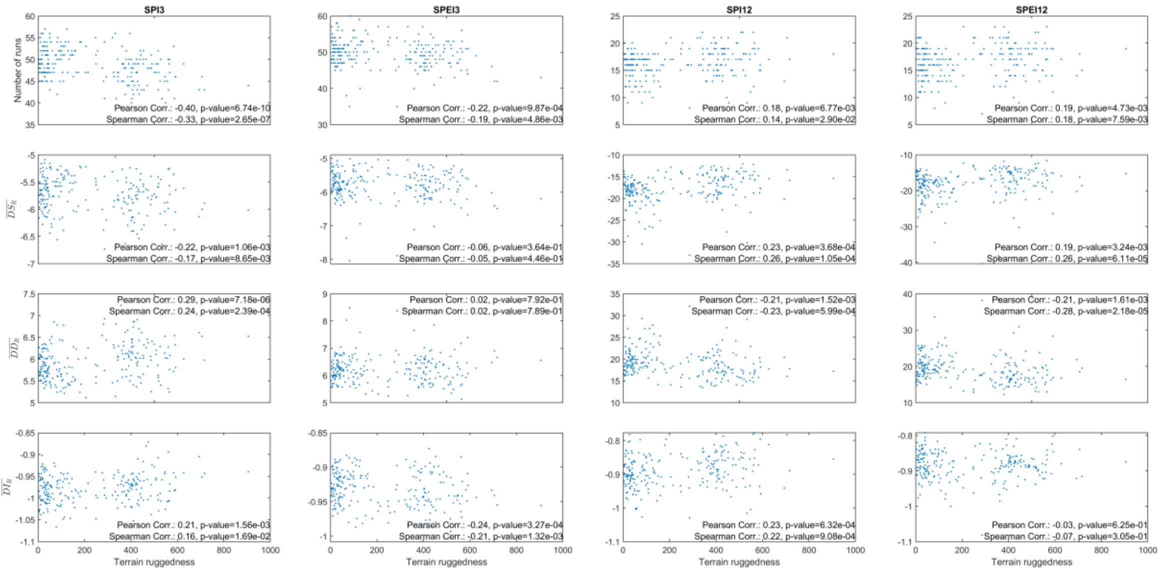
- Correlation values should always specify their associated p-values and which correlation method is used (Pearson, Spearman). If Pearson correlation is used, are its assumptions met? These should be clarified in Lines 260, 265, and throughout the manuscript.

Correlation values reported in the manuscript have been calculated using Pearson’s methods, but Spearman’s values have also been calculated following Reviewer #1’s comments. In the following figures the scatter plots between drought run characteristics and mean elevation/terrain ruggedness and between drought run characteristics change from the first to the second half of the study period and mean elevation/terrain ruggedness are reported:

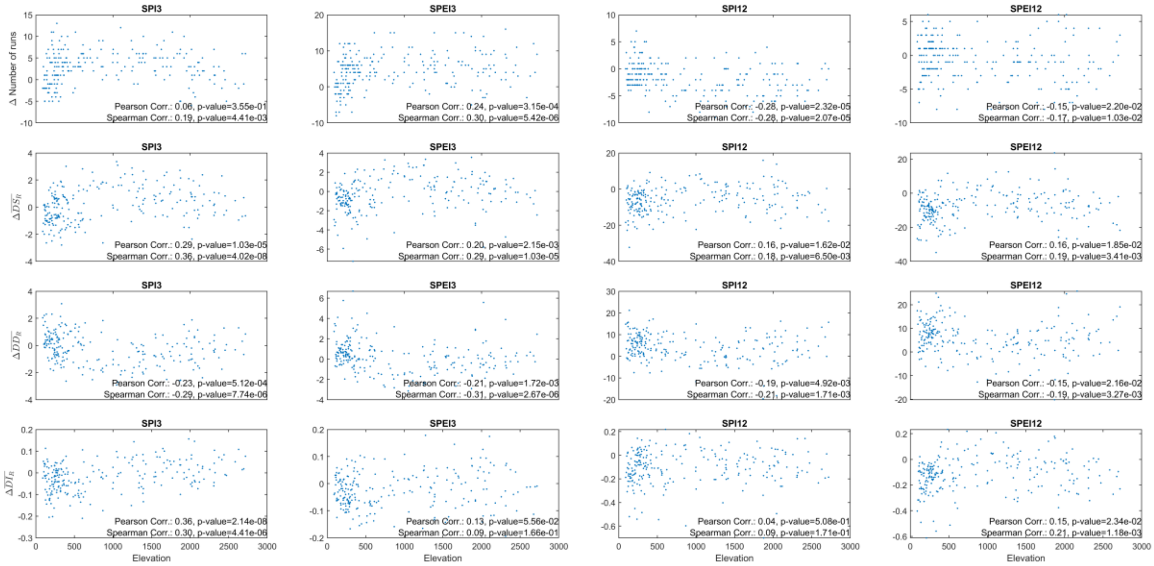
Drought run characteristics – Mean elevation (values reported in Table 1 of the revised manuscript)



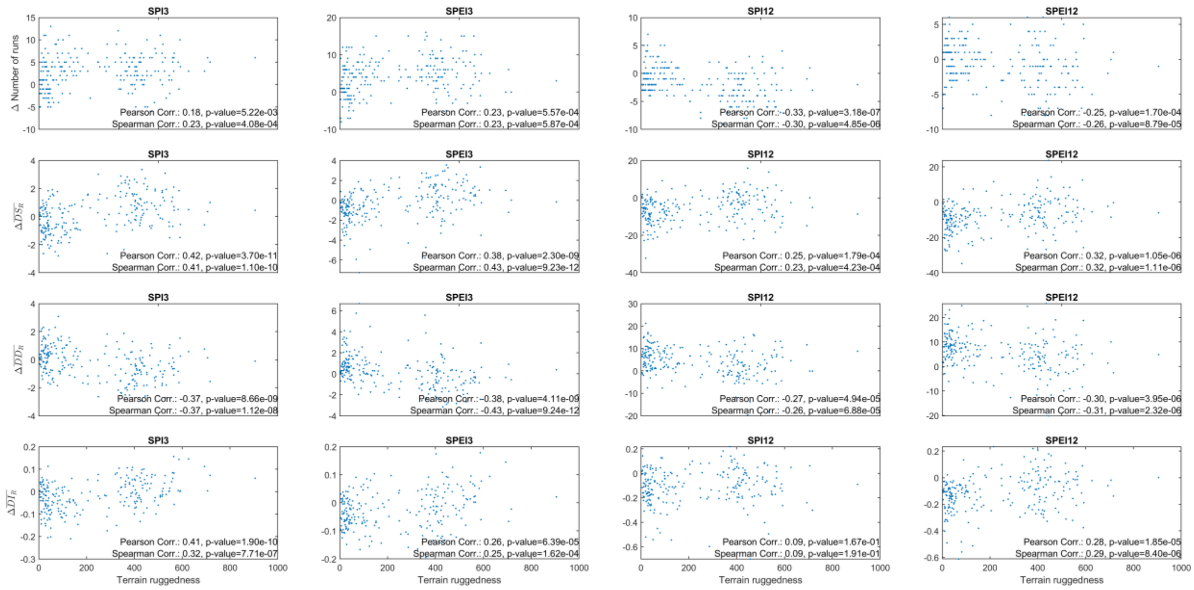
Drought run characteristics – Terrain ruggedness (values reported in Table 2 of the revised manuscript)



Drought run characteristics change – Mean elevation (values discussed but not reported in the manuscript)



Drought run characteristics change – Terrain ruggedness (values reported in Table 3 of the revised manuscript)



As can be seen from the previous figures, the reported Pearson's and Spearman's correlation values are quite close, with Spearman's values usually higher and more significant. Given that the scatter plots show linear correlations, and to be more conservative in the reported results, Pearson's values are the ones reported in the revised manuscript. The fact that both correlation values have been calculated and that only one type is reported is now mentioned in the revised manuscript at Lines 234-237. Finally, by checking the correlation values an error in the calculation of the mean drought intensity value—and thus its correlation value with either mean elevation or terrain ruggedness — was found: the error has been corrected and Tables 1 and 2, as well as their discussion at Lines 347-365 of the revised manuscript, and Figures C1 and C2 (which reported the wrong mean drought intensity values) have been amended.

- Line 279-281. Why are these associated with soil moisture and groundwater? Please provide citations or the results of an analysis.

The correlation between SPI/SPEI at different time scales with different water resources is usually accepted in the literature as a property of the indices based on the propagation of drought in the hydrological cycle (see for example the Standardized Precipitation Index User Guide issued by the WMO, https://library.wmo.int/records/item/39629-standardized-precipitation-index-user-guide). Although this correlation has been studied and somewhat confirmed in the literature (e.g. https://doi.org/10.5194/hess-9-523-2005), our reference to it is based just on the generally accepted operative use of the drought index. Thus, to avoid confusion with an actual water resources/drought index correlation analysis, we prefer to leave it without citation. We have however reformulated the sentence as follows: "Furthermore, trend analysis on indices at the shorter 3 month time scale and the longer 12 months time scale indicates, respectively, how drought conditions might have evolved over smaller time scales, closer to the response time of soil moisture conditions to meteorological conditions, and over larger time scales, closer to the response time of water reservoirs and groundwater levels to meteorological conditions." (Lines 288-291 of the revised manuscript)

- Lines 283-285. These sentences are unclear, please revise them. What are "worse conditions"?

Lines 283-285 of the old manuscript have been revised: "Trend analysis on SPI-3 and SPI-12 values shows results that mostly agree with the trends in annual precipitation, as a majority of cells reports both significant negative trends in annual precipitation values and in index values (and thus a tendency towards dryer conditions)." (Lines 293-294 of the revised manuscript). "Worse conditions" referred to a dryer overall climate, represented by lower SPI values over time; as such, "worse conditions" have been changed to "dryer conditions" in the revised manuscript, see Lines 294-295.

- Figure 5. (a) Is precipitation units in mm per month? This figure is not discussed in the manuscript. Either discuss it or remove it.

The figure was intended to give a visual representation of the type of data analyzed in the following sections; since we now feel that it is not needed and that it could lead to confusion, as also noted by Reviewer #2, it has been removed.

- Lines 290-293. How relevant is the magnitude of the SPI and SPEI trends?

Drought index trends indicate, if negative, a downward shift of precipitation/precipitation minus reference evapotranspiration (for SPI and SPEI respectively) values, meaning that over time it is more likely that a given month will have lower-than-average values and thus be in drought conditions. The magnitude of the trend is thus related to how much the average conditions are shifting towards the lower part of the meteorological values' distribution: for example, the mean for SPEI-12 is 6% change in a decade (see Figure 5 of the revised manuscript), comparable with the change in percentage of the average annual precipitation (Figure 4 of the revised manuscript). Still, as we show in Sections 3.3 and 3.4, this influences various drought characteristics leading to longer and more severe drought periods, as well as drought conditions influencing wider portions of the region at the same time.

- Figure 6. (e) and (f) should avoid differentiating the variables by blue and red colors because it creates some confusion with (a) – (d). Also, what exactly is the unit Δindex? Why are the trends presented in month units here but in year units in Fig. 8?

The colors denoting the different indices have been changed in order to avoid confusion. The Δindex unit is represents the change in the different indices (SPI-3,SPI-12,SPEI-3,SPEI-12) given their standardized nature and their lack of a unit measure. Finally, in order to avoid inconsistencies, the trends are now presented in yearly change rather than monthly change.

- Table 2. Are “Number of runs” the number of drought events? Is C the correlation coefficient? Why is it that only one variable has units?

In Table 2 “Number of runs” refers to the number of local droughts in a cell: the mean severity, duration and intensity are then DS_L, DD_L *and* DI_L *respectively. Of these latter values only* DD_L *has a units as it is measured in months, while the other two are a sum and a median of a standardized value lacking a unit measure. To avoid confusion, and since it is not needed to present a correlation coefficient, the unit measure of* DD_L *has been omitted. Finally, C is the correlation value, and this has been made explicit in the table caption.*

- Figure 8. This figure is hard to understand and is also not much discussed in the manuscript. Red and blue colors are used in the other figures to differentiate between increasing or decreasing trends, but here denote different variables, creating some confusion.

The colors used to indicate the different drought indices have been changed in order to avoid confusion.

- Figure 9. (a) Do the negative and positive y values represent different variables? If so, this should be clarified by using two different y axis and by describing in the figure caption.

Figure 9 has been changed according to Reviewer #1’s comment.

- Line 422. What does “worse” refer to here?

“Worse” referred to drought event characteristics (severity and duration) becoming more severe. The phrase has been changed to avoid confusion: “This seems to confirm that the shift towards worse region-wide drought conditions (higher severity and longer duration) is more evident at longer time scales, and that this shift is mainly caused by a change in precipitation patterns.” (Lines 436-438 of the revised manuscript).

Response to reviewer #2

We thank Reviewer #2 for the detailed analysis and the precise comments about our methodology and presentation/discussion of the results. We respond here to the more specific concerns raised and how they are addressed in the revised manuscript. In the following, Reviewer #2 comments are in plain text, while our comments are in italics.

- If the aim is indeed to investigate the correlation between drought trends and elevation/ruggedness this should be better reflected in the methods and results, since as it is now only a small part of the results takes into account the elevation/ruggedness and the rest just focuses on the trends in droughts.

  The aim is not only to investigate the correlation mentioned by the Reviewer but also to present drought characteristics and detect changes in time. However, the relationship between drought changes and elevation is central. Therefore, the Method section of the revised manuscript now focuses more on the different classification of the domain based on mean elevation/terrain ruggedness (se Lines 105-119 of the revised manuscript); furthermore, following Reviewer #1's suggestion, an additional section concerning the way in which correlation values are calculated has been added (see Lines 233-237 of the revised manuscript). Furthermore, the Discussion section of the manuscript has been revised, adding a comparison between the literature already analyzing drought-elevation relations and how our study adds to this field, and also providing a further discussion of the results obtained (see Lines 485-501 of the revised manuscript).

- The first part of the introduction is very general and not very relevant.

The first paragraph has been made more concise: "Drought is considered to be one of the main natural disasters, with widespread effects affecting large portions of the world's population (Wallemacq et al., 2015) and causing severe financial losses (García-León et al., 2021) and ecosystem impacts (Crausbay et al., 2020). Drought also has both short- and long-term effects on water availability (IDMP, 2022), which are relevant when considering the global increase in water demand in the last 100 years and the predicted challenges in meeting that demand in the future (Unesco, 2018; Wada et al., 2016; Burek et al., 2016)" (Lines 19-23 of the revised manuscript).

- In lines 36 to 38 the authors state that the fact that drought occurrence increases is contradictory to the finding that recent droughts are not exceptional. However, this does not necessarily contradict each other, droughts can occur more frequently, even if the individual droughts are not more exceptional than previous droughts.

We agree with Reviewer #2 consideration, and we have modified Lines 36 to 38 of the old manuscript in order to avoid confusion about their intended meaning: "Overall, these studies have found an increase in meteorological drought occurrence in North-West Italy, particularly after the 1970s, even when recent drought events have not been found to be exceptional when compared to historical records" (Lines 31-32 of the revised manuscript).

- In general, the introduction describes a lot of research on drought and its relation to orography that has already been done in Italy. It is not very clear to me which research gap the authors aim to address with this study and how this will contribute to an improved understanding of drought.

Drought has been studied in the area by focusing on the trends in drought indices, and no studies have investigated possible link between drought and terrain characteristics. We feel that such studies are important, given the growing literature focused on the elevation dependent warming/precipitation-change effects under climate change conditions. Furthermore, while some studies have considered drought-elevation relations in other parts of the world (China https://doi.org/10.1038/s41598-020-71295-1, Iran *https://doi.org/10.1007/s00704-020-03386-y, India https://doi.org/10.1016/j.atmosres.2023.106824 and the Canary Islands https://doi.org/10.1038/s41612-023-00358-7), our study area presents topographical characteristics that lead to evidence of more complex interactions between terrain characteristics and wetting/drying trends, as the ruggedness of the terrain is better correlated than elevation to the observed trends/changes.*

- In section 2.2 the authors describe that the data set they use is a gridded dataset, based on the interpolation of station data. How are these results affected by the interpolation method used? Why not analyse the station data, instead of the interpolated data?

Given our interest in comparing the drought conditions between different areas in our domain, meteorological series with a common length and with a common representativeness for each area are needed. The use of a gridded dataset allows us to have both these features, while the use of station data would present the problem of comparing series with different lengths and the problem of how to attribute the station data to certain portions of the territory. The possible effects of the interpolation method are discussed (with more details in Appendix A), in particular by focusing on the lack of elevation trend modelling in the interpolation method.

- In section 2.3, according to the section title, the authors describe how they divide the areas based on elevation. However, from the text I understand that the division is actually based on ruggedness and not on elevation. Also, what is the difference between terrain roughness and ruggedness? Or are they the same? They seem to be used interchangeably. Please explain the difference and clearly state which one is used, or if they are the same, make sure to be consistent throughout the manuscript. Also, the authors state that they investigate orography, meaning the combination of elevation and ruggedness, but in the end they define groups based only on ruggedness, and not elevation. This should be corrected in the rest of the manuscript, where it is sometimes stated that the correlation between drought and orography is investigated.

The title for Section 2.3 was chosen because the mean and standard deviation of the elevation (the latter representing the ruggedness of the terrain) are used to obtain two different classifications of the study area. To avoid confusion between the two metrics, as both are elevation-based, we have chosen to use the term "mean elevation", instead of just "elevation" when the first classification is mentioned in the rest of the manuscript. Furthermore, we acknowledge our error in using the term "roughness" instead of ruggedness in the manuscript—this typo is corrected in the revised manuscript. Finally, given that the results from both elevation-based classifications are contrasted throughout the manuscript, the term "orography" had been chosen to denote this type of study based on terrain characteristics, whether mean elevation or ruggedness is used; in order to avoid confusion, the term has been substituted for "terrain characteristics" throughout the revised manuscript.

- The caption of figure 2 mentions ruggedness, while in the figure roughness is used. Also, in the caption "(d) correlation …" should be (f) and the caption states that it is the correlation between elevation and elevation standard deviation, while the axes in the figure describe mean elevation and terrain roughness. Although I understand that this is how the roughness is defined, it is better to be consistent and use the same term.

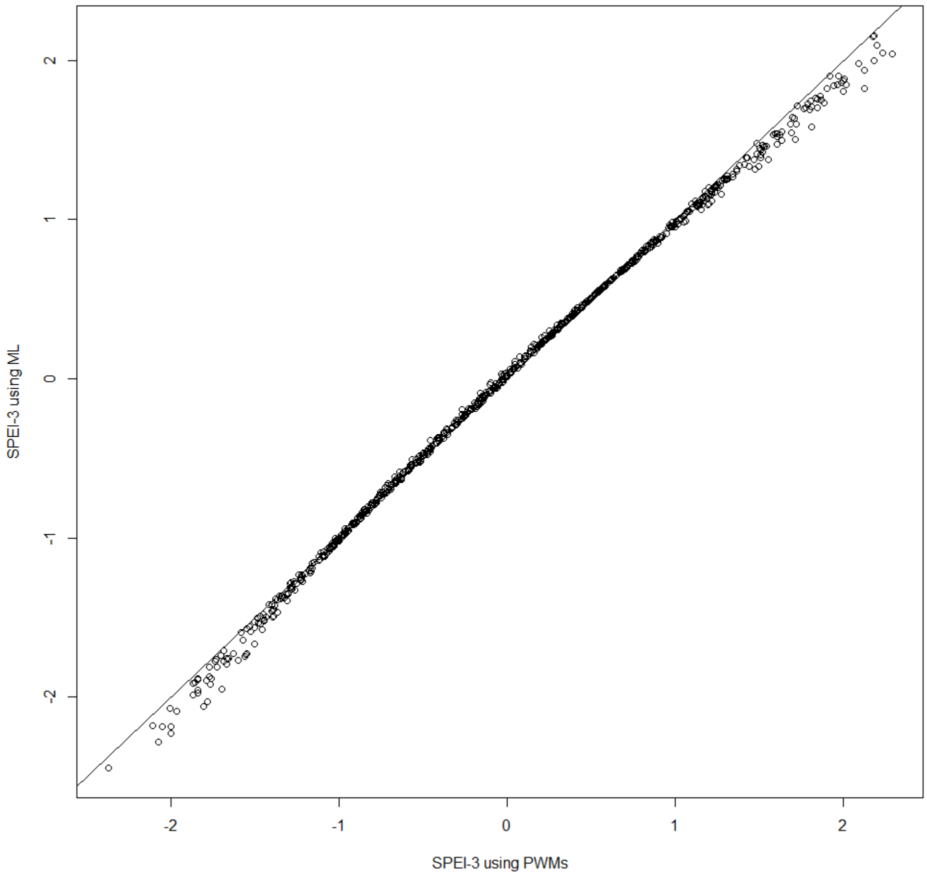
Figure 2 has been changed according to Reviewer #2's comments.

- From section 2.3 it seems that you are actually also investigating the differences in trends for different elevation groups and comparing that to the ruggedness groups. It would be good to make this more clear throughout the manuscript (e.g. also in the introduction). In addition, you could consider also showing the classification based on elevation in figure 2.

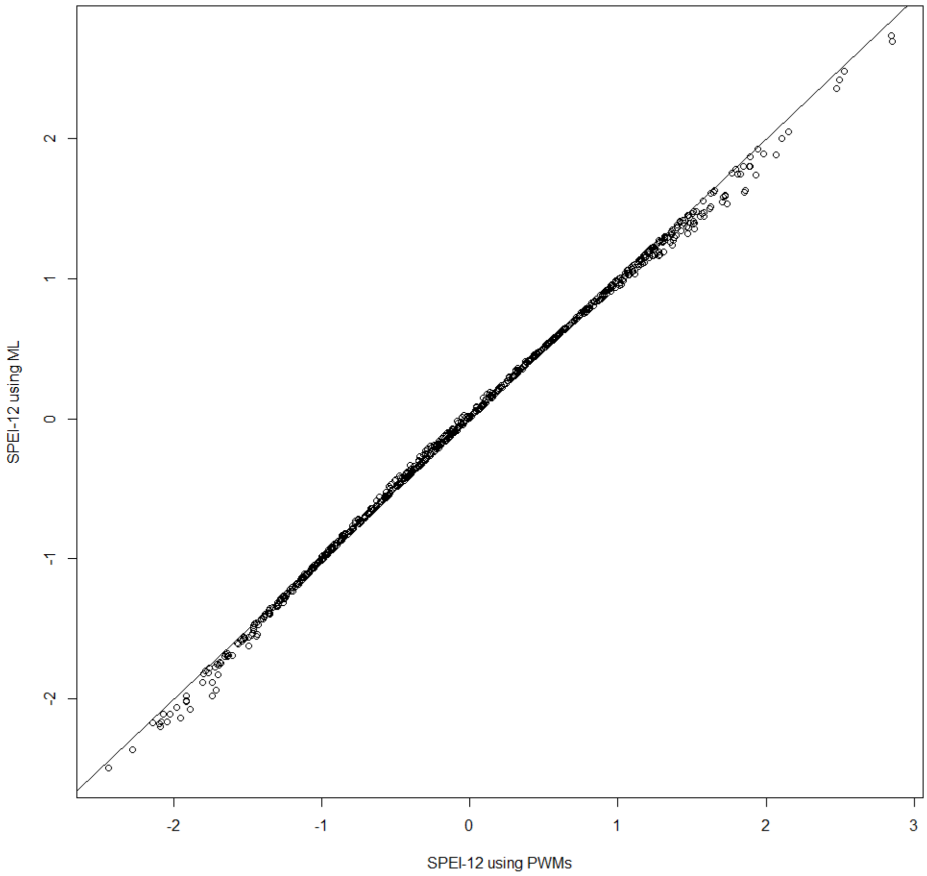
A reference to the comparison between areas classified through mean elevation and terrain ruggedness has been added to the introduction: "Results obtained by focusing on either mean elevation or terrain ruggedness *are also compared, to understand if only elevation-related effect are present, or rather more complex interactions between meteorological drought and terrain characteristics." (Lines 57-59 of the revised manuscript). Furthermore, Section 2.3 has been thoroughly rewritten in order to address multiple comments by the Reviewers.*

- To calculate the parameters of the SPI, the authors use the maximum likelihood method and for the SPEI, they use probability weighted moments. Why not use the same method (if there is a good reason, please explain) and could this affect the results (e.g. the difference in the trends between SPI and SPEI)?

The calculation for the two indices was based on the existing literature and the suggested methods for the distribution parameters estimation. For the SPI the maximum likelihood method is used as this is the method proposed in the literature (see https://doi.org/10.1007/s12145-014-0178-y, https://doi.org/10.1007/s11269-012-0026-0, both citing the formulas proposed in https://doi.org/10.1175/1520-0493(1958)086%3C0117:ANOTGD%3E2.0.CO;2). For the SPEI the proposed best method for parameter estimation is instead Hosking's PWMs. This method is proposed in Beguería et al., (2014), where the authors discuss briefly the effects of choosing one method over the other, stating that "[t]he SPEI series based on maximum likelihood were very similar to those based on the unbiased PWM method [...]. Given that calculation of the maximum likelihood estimation was about two-fold more time consuming, we conclude that the unbiased PWM method should be preferred for computation of SPEI series". Coherently, comparison between the results of the two method has been shown in our calculations to not have a meaningful impact on the results, as can be seen in the two following scatter plots between SPEI with ML and PWMs methods:
Scatter plot between SPEI-3 calculated with ML parameters and PWMs parameters



Scatter plot between SPEI-12 calculated with ML parameters and PWMs parameters



Although some slight differences are present, they are mainly located outside the 0 to –1 range used for drought definition in the study. Furthermore, all cells reported a Pearson correlation coefficient higher than 0.99 and RMSE lower than 0.09 between the series calculated with the two methods.

- The section on trend analysis does not describe the method that is used for trend analysis, please add this. In addition, how are seasonal precipitation series defined and how are temperature series deseasonalised?

The trend analysis method used for all series (precipitation, temperature and drought indices) is the one described in Section 2.4.3. Seasonal precipitation series are defined as the cumulative precipitation over the three month periods December-January-February (Winter), March-April-May (Spring), June-July-August (Summer), September-October-November (Fall). We have modified the manuscript in order to make this explicit (see Lines 175-177 of the revised manuscript). Deseasonalization was performed by applying the "deseason" function of the "Climate Data Toolbox for MATLAB" (https://doi.org/10.1029/2019GC008392), which calculates the seasonality as the mean of the detrended series for each month of the year. We acknowledge the missing citation for this function and the missing explanation of the method, and we have modified the manuscript accordingly (see Lines 177-179 of the revised manuscript).

- The difference between drought runs and drought events is not very clear. Are drought runs based on one pixel and drought events based on multiple pixels? For the drought events, is the same method used as for the drought runs, but with the additional condition that 25% of the domain needs to be in drought? In addition, why did you choose 25% as threshold and what does "domain" mean? Is this the total case study area or the area within the different ruggedness areas? If the latter, could this introduce some bias in your results? Since the areas with low terrain ruggedness are very close together and the higher terrain ruggedness are more spread out, so less likely to be all in drought conditions at the same time?

We acknowledge that the difference between the drought runs and events can be confusing, as (to our knowledge) no common way of referring to the two types of analysis is present in the literature. We called "drought runs" the droughts derived by analyzing the index series of a single pixel, derived via the application of thresholds and run analysis. "Drought events" (the name comes from https://link.springer.com/10.1007/s10584-022-03370-7) are instead droughts defined by considering all pixels in drought conditions (meaning all pixels that have a drought index value under a -1 threshold): if at least 25% of the pixels area experiencing drought conditions, a drought event is detected. This choice of area threshold is done to maintain consistency with the papers where this method was proposed, cited at Line 209-210 of the revised manuscript. In any case, following Reviewer #1's suggestion, we have decided to change the use of "drought run" and "drought event" in the manuscript to "local drought" (although still using the term run analysis as is common in the literature) and "region-wide drought event" to avoid confusion. Finally, the "domain" mentioned in the manuscript refers to the whole study area, not divided into different areas based either on mean elevation or on ruggedness.

• The authors use a t-test to calculate the difference between the means of the two periods. Why was this method used for trend detection and not another method? What are the underlying assumptions of this method? Do they hold and what are the potential implications for the results? In addition, why not compare the number of drought events between the two periods (in addition to severity, duration)?

The t-test method discussed in Section 2.4.6 is used to evaluate if a significant change in drought characteristics can be detected between the first and second half of the studied period. This is not an indication of the significance of the detected change; as stated at Lines 480-484 of the revised manuscript, this doesn't exclude that the changes between the two periods, even if significant, could be caused by the presence of particularly severe events not part of an overall trend. As the t-test is applied to compare the mean of a certain drought characteristic between two populations, the number of drought runs themselves could not be tested through this method. Trend detection was also performed on drought characteristics (see Line 367-372 of the revised manuscript), but almost no significant results could be obtained due to their discrete nature and the relatively small number of drought runs/events.

• Figure 5 shows time series for a representative point in the domain, where is this point? And how can one point be representative if four different areas are investigated?

Given that the drought indices series are not shown elsewhere, the aim of the figure was to give some context about differences between the SPI/SPEI at 3- and 12-month time scale (frequency of change, length of periods under the threshold…)—as such, the representativeness of the data was considered in regard to this aspect. To avoid confusion, and considering Reviewer #1 comments, we have decided to remove the figure.

• From section 3.3, it seems that linear regression was used to calculate trends? This is not mentioned in the methods.

The same trend detection procedure described in Section 2.4.3 is applied to the drought run data: given the lack of autocorrelation, this results in calculating the Mann-Kendall test and the Sen's slope.

• When analysing the trends in drought runs and events, this seems to not be separated by area. Why not, since the main aim of the paper is to show the effect of ruggedness on drought trends?

The relation between the change in drought run characteristics and ruggedness is studied by calculating the correlation between them. This was preferred to a comparison between the drought run characteristics' trends of areas defined through ruggedness values, given the low number of significant changes (as defined by the t-test) in drought characteristics. Furthermore, calculating drought runs from a drought index obtained from mean meteorological values belonging to an area instead of a pixel would have, in our opinion, created *confusion with the calculation of drought events. Finally, drought events are calculated on a region-wide level to have a point of comparison for the "local" drought runs' characteristics and their observed change.*

- There is no discussion of the results, only a summary. Are the results similar to the findings of the studies discussed in the introduction? And if not, why not? How are the results affected by the choice of methods?

The Discussion section has been expanded by including a comparison between the presented study and similar ones found in the literature, indicating the novelty of our study and the originality of its results (see Lines 485-501 of the revised manuscript). In particular, we state that "[our] type of analysis is in common with a growing body of literature focused on the elevation effects on drought characteristics" (Line 488-489 of the revised manuscript), and that "our study shows that mean elevation, although certainly a variable to be considered, shouldn't be the only topographic variable taken into account" (Line 495-496 of the revised manuscript) given that "[i]n our analysis, using [a] different classification leads to stronger correlations between drought characteristics and topographical characteristics" (Line 499-500 of the revised manuscript).