

# Author comment to RC3 in “Droughts in western Central Europe and associated atmospheric circulation patterns since 1844”

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## 5 Comment of the reviewer

Comment of the authors

### Summary:

The authors conduct a study about droughts in western Europe with the aim of comparing recent events with a historical context, and show possible weather patterns associated with drought events. Although previous literature has highlighted the weather patterns associated with past European hydroclimate extremes (Brönnimann et al., 2025), this study focuses on a historical perspective on a specific region of Europe (needed for more local management). They used three reanalyses to have a reliable multi-evidence approach (plus ERA5-Land, which is only used for two figures and not in the result section). The study includes the validation of the three reanalyses against four stations and their use for calculating the SPI and SPEI indices. They show important events in the last ca. 175 years and conduct a clustering of weather patterns and modes of variability that then relate to periods of drought. They extensively discuss the patterns associated with droughts.

### General comment:

It is necessary to unravel the different factors modulating droughts in the important socio-economic region (western central Europe). The manuscript shows a huge amount of work that is relatively well-connected and coherent. However, the manuscript is extremely long, so much, that it is difficult to remember the structure/sections and procedures described in the manuscript (a copy-edited version would be a 30-page article, way too long). The writing style is very wordy; one can say the same in fewer words, mainly using the active voice. Moreover, there are calculations and representations (plotting) that are methodologically questionable. There are many basic explanations, in my opinion, for a reader of WCD; on the attached PDF, I suggest or give examples of what can be modified (155 comments, including section 5). I recommend that the authors make several major adjustments to the manuscript, adjustments that need to be evaluated again before acceptance.

25 We thank the reviewer for their thorough evaluation of the manuscript and for the considerable time and effort dedicated to providing detailed and constructive comments. These remarks have significantly contributed to improving the quality and clarity of the study. We first address this general comment, followed by point-by-point responses to the other comments below.

We agree that the original version is excessively long and overly detailed in several sections. In response, a substantial effort will be made to streamline and refocus the manuscript. Parts of sections containing background information that can reasonably be assumed for the readership of WCD will be shortened or removed. In addition, the Data and Data Evaluation sections will be shortened and merged. Section 6.2 will be revised to focus more clearly on the key findings, avoiding detailed repetition of results already displayed in the figures. The Discussion section will also be substantially reorganised and shortened, with several subsections merged or reduced to better emphasise the interpretation, significance, and limitations of the results. The writing style will furthermore be revised throughout the manuscript to improve conciseness and readability.

Concerning the methodological and visualisation-related remarks, we respond to each point specifically in the comments below. Overall, these revisions are expected to substantially improve the focus and conciseness of the manuscript while preserving its core scientific content.

The manuscript shows four circulation patterns and discusses some possible dynamics (variables) related to their influence on droughts. However, my major concern is that the form in which the authors tackle the relationship between the weather patterns and droughts does not specify the mechanisms of drought development. That is, showing other specific variables could be necessary to elucidate what makes the drought evolve. For instance, how do the circulation patterns affect moisture transport, convergence, clouds, and radiation? How do these variables – under the circulation patterns – interact with soil moisture in previous months?. At the moment, it is only discussed by mentioning the influence of atmospheric subsidence with cloud-free skies or advection of warm air masses on evaporation. The aforementioned variables are important for the temporal development of the drought.

We agree that a more process-based analysis of the mechanisms linking circulation patterns to drought development — including moisture transport, convergence, cloud cover, radiation, and soil-moisture feedbacks — would provide valuable additional insight into the evolution of drought conditions. However, the present study is not designed to explicitly resolve all these coupled processes quantitatively. Instead, these mechanisms are discussed in terms of hypotheses and interpreted in light of the existing literature.

To strengthen the physical interpretation of the results, additional analyses of the main variables underlying drought development, including precipitation and AED, will be added and provided in the Supplementary Material alongside the drought indices. These additions will provide further insight into the physical characteristics of the drought conditions and complement the interpretation of the circulation patterns.

A comprehensive diagnostic attribution of the respective roles of moisture transport, atmospheric convergence, radiative processes, and land–atmosphere feedbacks would nevertheless require dedicated process-based analyses that go beyond the scope of the present work and would constitute a separate study in itself. In the revised manuscript, this perspective will therefore be explicitly added to the discussion of future research directions.

Other major concerns are the description and implementation of some procedures:

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- There are two reference periods: one for the drought indices (1947-2008) and the other for the Z500 anomalies clustering (1979-2008); this is very confusing for the reader (lines 190 and 225).

We thank the reviewer for pointing this out. This was indeed an error in the manuscript. The Z500 anomalies are calculated relative to the 1947–2008 reference period, rather than the 1979–2008 period as originally stated. This will be corrected in the revised manuscript.

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- Moreover, the SPI and SPEI indices at 3-month accumulation are the underpinning of the study, but then they specify that the indices are calculated grid-wise and also with the average of the region (line 191). Moreover, the authors calculate a difference between both indices that represent different dynamics: one, the standardised rainfall variability, the other the standardised water balance variability. What is the physical meaning of that difference? (Line 403).

70 Firstly, the indices are calculated at two scales: at the grid-cell level to assess spatio-temporal variability within the region, and at the regional scale, which is used as the basis for the atmospheric circulation analysis underpinning the study.

75 Secondly, we agree that the physical interpretation of the SPI-3 minus SPEI-3 difference was not sufficiently clarified in the original manuscript. This quantity is not intended to be interpreted as a direct physical variable, but rather as an indicator of the relative contribution of atmospheric evaporative demand (AED/PET) to drought conditions compared to precipitation alone. Since SPI-3 is based solely on precipitation whereas SPEI-3 additionally accounts for AED through the climatic water balance, their difference highlights the extent to which variations in AED amplify or mitigate drought conditions beyond precipitation-driven variability. We adopt this approach rather than analysing AED trends in isolation because AED shows a widespread increase across seasons, as will be shown in the Supplementary Material, whereas its effective impact on drought conditions depends on its balance with precipitation variability. The SPI-3 minus SPEI-3 comparison therefore helps to identify the seasons in which changes in AED play a relatively stronger role in modulating drought conditions. This interpretation will  
80 be clarified in the revised manuscript.

- The use of Thornthwaite instead of a more physically based PET is detrimental if one wants to analyse the influence of the increase in temperature in droughts (as reiterated throughout the discussion about the Atmospheric Evaporative Demand, AED).

85 We acknowledge the reviewer's concern regarding the use of the Thornthwaite formulation. While we agree that more physically based methods would be preferable, we also show in the revised manuscript that Thornthwaite performs comparably to more physically based formulations. A more detailed comparison between methods will therefore be added to the Supplementary Material to clarify this point.

In addition, the implications of using the Thornthwaite method will be explicitly discussed in the scope and limitations subsection of the revised Discussion.

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- Moreover, the authors also plot the 10-year moving average indices – which is strange for indices which are already accumulated – with the trends on top, what is the aim with that combination? Likewise, plotting all four seasons and the whole series with transparency makes the plots almost impenetrable to the eye; plotting so many panels and the long time series also does not help.

The aim of showing the 10-year moving averages is to highlight low-frequency variability and decadal fluctuations in the indices, which are not visible in the event-based analysis (Fig. 5). Given the strong high-frequency variability, the moving average provides a clearer view of longer-term evolution and complements the drought-event perspective. We acknowledge, however, that the original figure was overly complex and difficult to interpret. In the revised manuscript, readability will be substantially improved by reducing on-plot text, transferring numerical information to a table, and adjusting the visual design (including transparency and trend representation) to enhance clarity.

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- Furthermore, the procedure to create Figure 8a is not described, and the figure is very confusing itself (the authors try to link the four weather patterns with major modes of variability – “three first EOFs”); the y-axis says index coefficient (the SPI or SPEI?), but apparently is related to the EOFs, not the SPI or SPEI. This figure is essential for the discussion and conclusions of the weather patterns and thus the conclusions of the manuscript.

We thank the reviewer for pointing this out. We agree that the original description of Figure 8a was insufficiently clear and could lead to confusion regarding the meaning of the plotted values. The y-axis does not represent SPI-3 or SPEI-3 values, but the coefficients associated with the EOF modes of atmospheric circulation. The methodology used to construct the figure will therefore be clarified more explicitly in the figure caption.

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In particular, the former caption “Boxplot of seasonal EOFs indexes values” will be replaced by a clearer description: “Boxplots of EOF coefficients (NAO, EA, and SCA) from seasonal EOF analyses of the full Euro-Atlantic time series, shown for drought events associated with each circulation pattern (all seasons pooled).”

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The discussion is also very long and deviates to other topics rather than comparing the drought dynamics identified by other studies.

The Discussion section will be the most extensively revised part of the manuscript. In particular, subsections 7.2 and 7.3 will be merged and substantially shortened, with a clearer focus on the atmospheric circulation associated with drought and its comparison with existing literature.

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The conclusions have a proper length, but they only refer to the European high weather pattern with some specificity; the other patterns are only mentioned without specifying how they influence the development of droughts. Conclusions also only try to

120 make a summary; they could connect the relationship between the weather patterns and the modes of climate variability or trends. how they would evolve in the future (for the discussion section as well), as a strong focus of the manuscript is the analysis of trends.

We acknowledge that the Conclusions section could better reflect the circulation patterns beyond the European High pattern. While we remain cautious in over-interpreting the specific influence of each pattern on drought development, the revised manuscript will provide a clearer synthesis of their respective features.

125 The relationships between the atmospheric circulation patterns and the main modes of climate variability will also be more explicitly emphasised in the merged subsection of the Discussion. Furthermore, given the strong focus of the study on trends, the Conclusions section will be revised to better highlight their potential implications for future changes in atmospheric circulation variability and associated drought characteristics.

Many of the concerns should also be addressed by adhering to the FAIR principle. That is, the authors should make their codes available for reproducibility!

130 We fully agree that transparency and reproducibility are essential and strongly support the FAIR principles. The code will be made publicly available upon publication of the article.

The reference to several subplots in the text is disorganised. Almost all subplots do not have an individual letter to identify them and are also not referenced in the figure description in the results text section (not only in the caption). The size of the labels in the plots is way too small.

135 Figure 3 will be updated with individual subplot labels. Additional panel labels will also be added to Figures 6 and 7 to improve readability and ensure consistent referencing in the main text. All subplots will be explicitly referred to in chronological order within the Results section. In addition, the size of plot labels will be increased where appropriate, and in some cases, labels will be removed from the figures and replaced by tables (as done for Figure 5) to improve overall clarity.

### **Specific comments:**

140 I attach the PDF file with my comments where I make specific suggestions on some sentences, ask questions about procedures or request better explanations. In the following, I specify the most important ones.

We thank the reviewer for providing detailed comments directly in the PDF, including specific suggestions on wording, methodological questions, and requests for clarification. These comments have been very helpful and will be carefully addressed in the revised manuscript.

145 In the following, we respond to the most important points in detail. The remaining comments will also be carefully considered.

Line 104 - It is debatable if Section 5 (datasets evaluation) should go in the main text of the manuscript. It can be shown as supplementary material. This evaluation, although important, diverts the attention of the reader. It adds too much information, making the manuscript very long. This will shorten the manuscript or give space for deeper analysis.

We agree that the current Data Evaluation section can divert the attention of the reader from the main results of the study.  
150 However, as noted by the reviewer, this evaluation remains important to support the robustness and interpretation of the subsequent analyses. To improve conciseness, the Data and Data Evaluation sections will be merged to improve the overall flow and coherence of the manuscript. In addition, elements of the evaluation will be moved to the Supplementary Material where appropriate in order to improve conciseness. These changes will ensure a clearer distinction between methodological description, evaluation, and results while preserving the importance of the evaluation. This restructuring will improve the  
155 clarity of the manuscript and allow the Results section to focus more directly on the scientific analysis.

Line 115 – The argument for choosing the region of study does not seem relevant. There are other very strong arguments, see the comments on the PDF.

The justification for the study region will be revised and refocused on its socio-economic relevance, which is one of the most important factors in terms of impacts in this region. Additional explanations and supporting arguments, together with the  
160 relevant information already included in the manuscript, will be retained to further strengthen this motivation.

Line 128 - Figure 1 can be found in an Atlas; then what is its significance in this long manuscript? Probably can be added to the supplementary material for the curious reader.

The study area represents a transition zone between Western and Central Europe and therefore does not correspond to a classical geographical unit. We consider it important to present it in the manuscript so that the reader has a clear spatial context.  
165 In addition, Figure 1 includes the locations of the weather stations used for the data evaluation, which is essential information for interpreting the subsequent analyses.

While the mean annual precipitation field can be found in general climatological atlases, its inclusion here provides important quantitative context on the spatial structure of the study area and its relative homogeneity. This supports the use of drought indices at the regional scale, which forms the basis for the circulation pattern clustering. This point will be further argued and  
170 clarified in the revised manuscript. For these reasons, we consider Figure 1 essential to the main manuscript rather than supplementary material.

Line 170 – Figure 2 is not necessary; it can be consulted elsewhere and readers of WCD most certainly know the meaning.

We agree that the schematic conceptualisation is not strictly necessary for understanding the drought indices. Figure 2 will therefore be removed from the manuscript as recommended.

175 However, the introduction of SPI and SPEI will be retained, as these indices may not be familiar to all readers of WCD, as also noted by the co-editor. This section will be substantially shortened for conciseness while preserving the essential methodological information.

Line 191 - If the indices were calculated by cell, what do you then average the inputs for the indices in the whole area? Did you recalculate the indices? why the averaged inputs? Were the indices calculated twice? If you average the indices calculated on a grid, it should give the same value of the indices calculated with the average of the gridded Ppt and ETP. The indices only make a normalisation.

We thank the reviewer for this comment and for raising this point. The indices were computed at both the grid-cell and regional scales, but for different analytical purposes. For the regional-scale analysis, precipitation and AED were first area-weighted across all land grid cells to obtain a regional mean time series, and the indices were then computed from these aggregated values. This approach is necessary because the standardisation underlying the indices is a non-linear transformation; therefore, the index computed from aggregated inputs is not equivalent to the spatial average of grid-cell indices. This distinction is important in this study, as the resulting drought classification is subsequently used to analyse atmospheric circulation patterns. As shown in Spinoni et al. (2016), applying the standardisation after spatial aggregation preserves the regional signal, whereas averaging pre-computed grid-cell indices can distort the representation of drought conditions by mixing differently scaled anomalies. For this reason, aggregation will be explicitly clarified as being performed prior to index computation in the revised manuscript.

Line 205 and 208 - The Thornthwaite PET. If Penman-Monteith was calculated (as shown in Figure S1), why don't they use and report the most accurate formulation? This is very concerning as Thornthwaite is heavily influenced by changes in temperature.

195 We acknowledge the reviewer's concern regarding the use of the Thornthwaite AED (or PET) formulation. Penman-Monteith estimates were also computed for ERA5 and 20CRv3 to compare them with the Thornthwaite formulation. However, some variables required for a consistent Penman-Monteith implementation — notably relative humidity and ground heat flux — are not consistently available across all datasets, such as ModE-RA, preventing a fully reliable and homogeneous estimation of AED over the complete analysis period and across all reanalyses (Córdova et al., 2015). This clarification will be added to the manuscript.

The Thornthwaite formulation was therefore preferred because it allows methodological consistency across the three datasets while still showing comparable trends relative to Penman-Monteith for ERA5 and 20CRv3. In addition, a more detailed analysis of AED behaviour, independent of the drought indices, will be added to the Supplementary Material. The associated limitations and implications of the AED methodology will also be explicitly discussed in a dedicated scope and limitations subsection of the revised Discussion.

Line 236 - There are previous research that identified 7 patterns, and recently identified 9 (CAP7 and CAP9; Schwander et al., 2017; Pfister et al., 2025). If the number 4 was identified with some criteria based on intermediate results, it should be better explained.

210 The choice of four clusters will be further justified and clarified in the revised manuscript. In contrast to CAP7 and CAP9 classifications (Schwander et al., 2017; Pfister et al., 2025), which are based on daily weather-type variability over the full spectrum of atmospheric conditions, the present analysis focuses on seasonal circulation variability associated only with drought conditions. This substantially reduces the expected diversity of circulation states.

215 In addition, several commonly used clustering diagnostics (AIC, BIC, Silhouette, Calinski–Harabasz, and Davies–Bouldin indices) were tested but did not provide consistent guidance regarding an optimal number of clusters (Fig. S2), with suggested values ranging between two and six clusters. The final choice of four clusters was therefore based on the interpretability and separability of the resulting spatial patterns, for which illustrative examples will be added to the Supplementary Material.

220 Line 242 – The EOF results are mentioned in the methods section. Then the authors decide to assign a name – I assume to the first three EOFs – but you don’t show the variance explained, not even in the results section, Figure 8a, where you come back to them. Either the authors placed it in the supplementary material, or they only say in the methods that you used EOFs and show everything in the results section.

The variance explained by the first three EOFs, as well as their associated spatial patterns for each season and each reanalysis, will be provided in the Supplementary Material and explicitly referred to in the manuscript. This addition will clarify the identification and interpretation of the EOF modes discussed in the Methods and Results sections while keeping the main manuscript concise.

225 Line 270 – No, ERA5-Land is a land surface simulation, meteorologically forced with downscaled estimates from ERA5. This should be better described and understood. Moreover, you only show ERA5-Land in the evaluation section and in Fig 1, then you never use it again. This is not consistent.

230 We acknowledge that ERA5-Land is a land-surface reanalysis, meteorologically forced by ERA5 and based on downscaled estimates, and that it is therefore not directly comparable to ERA5 as a simple higher-resolution version. We also acknowledge the inconsistency arising from its limited use in the evaluation and in Figure 1 only.

In response, we decided to remove ERA5-Land entirely from the manuscript to avoid unnecessary complexity and ensure consistency in the dataset set-up. ERA5 will also replace ERA5-Land in Figure 1 so that the descriptive figures remain fully consistent with the datasets used throughout the analysis. This simplification does not affect the results or conclusions of the study.

235 Line 288 and 300 - Fig 3a shows the SPI, not the MAE, or the y-axis is wrongly labelled. Also, there are several panels. If 3a refers to the left panels, then the y-axis is wrong. If it was related to the evaluation using the MAE, then you must show the time series of the moving MAE, not the 10-year moving SPI index.

The text was indeed confusing and incorrectly referred directly to the MAE instead of the 10-year moving average of the drought index. This will be corrected accordingly. The missing panel labels (“a” and “b”) will also be added to Figure 3 to  
240 improve clarity and consistency with the text references.

The purpose of the figure 3a is not to display the MAE time series itself, but to use the MAE threshold as an evaluation criterion while showing the 10-year moving average of SPI-3. This representation allows assessment not only of the magnitude of deviations from observations, but also of their direction (i.e. whether the reanalysis tends to be systematically too dry or too wet relative to observations), which would not be visible from a MAE time series alone. In addition, the shaded areas indicate  
245 periods during which the MAE threshold is exceeded, making drifts visually identifiable.

Line 322 – Why were Matthew’s Correlation Coefficient used to compare the three reanalyses? This seems like an evaluation and not the results of the drought dynamics and their drivers, which are the aim of the manuscript.

Indeed, this analysis corresponds to the evaluation of the reanalyses against observations, rather than to the drought dynamics themselves. The objective of this evaluation is to assess the temporal robustness of the reanalyses in order to increase  
250 confidence in the subsequent analyses of drought characteristics and atmospheric circulation.

As explained in the manuscript, the Matthews Correlation Coefficient is used to evaluate the quality of drought-event identification, thereby complementing the assessment of the general behaviour of the drought indices based on the MAE. To improve the structure and focus of the manuscript, the former Data Evaluation section will be merged with the Data section, which will help better distinguish methodological evaluation from the scientific results.

255 Line 350 – drought types or drought indices?

We meant drought indices rather than drought types. The text will be corrected accordingly in the revised manuscript.

Line 384, 437 – Why do the maps only show ERA5 if you are trying to make a historical analysis with the other two reanalyses? The line talks about the three reanalyses; it is better to talk about the recent trends and reference Figure 6a, which only shows ERA5. Then, in another sentence, you talk about the other reanalyses and reference specific panels in Figure 6b.

260 We thank the reviewer for pointing this out. This was indeed an error in the figure referencing. The original text incorrectly referred to the maps in Figure 6a, which only display ERA5, instead of the corresponding panels in Figure 6b that include the three reanalyses. The references will be corrected accordingly to improve clarity and consistency between the text and figures.

Line 460 - Is not the clustering calculated using the Z500 (line 225)? Why do you refer to SPEI clustering? Is it “clustering of Z500 based on the SPEI-identified droughts”?

265 Yes, this is correct, and we thank the reviewer for highlighting this ambiguity. The Methods section will be revised to clarify the clustering procedure. K-means clustering is applied to 500 hPa geopotential height (Z500) anomalies, and not directly to the drought indices. SPI-3 and SPEI-3 are used solely to identify drought events.

Line 475 - The weather patterns based on clustering of the Z500 were also based on the identification of droughts with a particular index. But here it is not specified which of those indices. It was also not properly described in paragraph Line 216  
270 of the methods section.

The caption of Figure 7 will be clarified by explicitly mentioning the SPEI-3 drought-based clustering. We will also clarify in the Methods section that two independent clustering analyses are performed: one based on SPI-3–defined drought events and one based on SPEI-3–defined drought events. The corresponding figures (Fig. S7 and Fig. 7) will also be explicitly referenced in the Results section to improve clarity.

275 Line 489, 520 – The y-axis (“count”) of Fig 8c does not match the number of squares and the figure is not explained, only called “waffle chart”. Also Fig 8c is described first than 8a.

Chronological referencing of Figure 8 panels will be corrected in the revised manuscript. The y-axis inconsistency in Figure 8c will also be corrected.

The waffle chart representation was chosen instead of a bar chart because it allows both the relative seasonal proportions and  
280 the total number of events associated with each circulation pattern to be visualised simultaneously, facilitating comparison between patterns. Its interpretation will also be clarified more explicitly in the figure caption.

Line 496, 497, 518 – Fig 8a supposedly is created based on SPEI but then the authors refer to an index of 0.09 for both SPEI and SPI. This is very confusing because, first, that is not a threshold for identifying the droughts, and second, the figure should be based on the clustering that is based on one drought index (according to the methods). If the “index” in the y-axis of the  
285 figure refers to the EOFs time series (it can also be called the PCAs time series or just PCA), then the analysis in the text is wrong, or vice versa. This needs to be corrected because it is essential for the discussion and conclusions of the weather patterns, and thus the conclusions of the manuscript.

We agree that the original wording was confusing. The term “index” was incorrectly used in this paragraph, whereas the values discussed actually correspond to EOF coefficients, and not to SPI-3 or SPEI-3 values. This terminology will be corrected  
290 throughout the manuscript to avoid ambiguity.

For the figure, as for the response of a major concern above, we agree that the original description of Figure 8a was insufficiently clear and could lead to confusion regarding the meaning of the plotted values. The y-axis does not represent SPI-3 or SPEI-3 values, but the coefficients associated with the EOF modes of atmospheric circulation. The methodology used to construct the figure will therefore be clarified more explicitly in the figure caption.

295 Line 519 – drought intensity anomaly: anomaly of the drought values? So, the authors calculated a climatology only considering the drought events? This is very wrong. Why are there positive values for a drought? The SPI and SPEI are already expressed as a standardised anomaly.

We agree that expressing anomalies of already standardised indices was confusing and could lead to misinterpretation. To  
300 improve clarity and avoid presenting anomalies of anomalies, the figure will be revised to show the raw SPI-3 and SPEI-3 values instead of anomaly values. Although this representation provides less visual contrast, it offers more direct and interpretable information.

Line 552 - In the manuscript, the authors don't define the intensity as severity (summatory of duration and value of the index - SPI or SPEI). Then the authors should say more explicitly what they mean by intensity (we understand the values of the  
305 index).

We agree that the term “intensity” was ambiguous. It will be replaced by “mean intensity” to clarify that it refers to the mean value of the drought index, consistent with the definition provided in the methodology, rather than drought severity.

Line 596 – If the manuscript is about droughts, why discuss the wet periods? Considering that the manuscript is very long, this sentence of discussion only makes the manuscript unfocused.

310 We agree that this discussion of wet periods diverted attention from the main focus of the manuscript, especially given its original length. This part belonged to the former subsection 7.2, which will be merged with subsection 7.3 and substantially refocused on the atmospheric circulation associated with drought conditions. The discussion of wet periods will therefore be reduced or removed accordingly to improve the focus and conciseness of the manuscript.

Line 739 – Section 7.4 should be better called "challenges for drought management". Otherwise, the reader interprets it as  
315 possible future research.

Subsection 7.4 will be substantially shortened and integrated into the Conclusions section, where it will be used to open perspectives on drought management challenges and future implications. This restructuring will also help avoid confusion with a dedicated future research section.

792 – The only weather pattern that is concluded about is the European High. For sure, the other patterns must have something  
320 to highlight about.

As addressed in response to a major comment above, we acknowledge that the Conclusions section could better reflect the circulation patterns beyond the European High pattern. The revised manuscript will provide a clearer synthesis of the respective features of each pattern.

Probably citing the same article many times is making the introduction too long as well.

325 We agree that repeated citation of the same references contributed to the excessive length of the Introduction. The number of repeated citations will therefore be reduced where appropriate to improve conciseness and readability of the manuscript.

## References

- Córdova, M., Carrillo-Rojas, G., Crespo, P., Wilcox, B., & Célleri, R.: Evaluation of the Penman-Monteith (FAO 56 PM) Method for Calculating Reference Evapotranspiration Using Limited Data. *Mred*, 35(3), 230–239.  
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