Author comment to RC1 in "Droughts in western Central Europe and associated atmospheric circulation patterns since 1844"

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Comment of the reviewer

Comment of the authors

Neimry et al. have analysed historical drought trends in western Central Europe (BE,NL,LUX,partFR,partDE). Using three different reanalysis products and four meteorological stations, they calculate SPI3 and SPEI3 and analyse trends and circulation associated with drought events. I find parts of the study interesting, for example the circulation drivers, the long historical perspective and the decadal variability in drought occurrence shown in Figure 5. However, I sometimes got lost in reading the results sections where many statistics are noted and discussed, often in a very lengthy way.

My first point of advise to the authors is thus to make an attempt at shortening the manuscript, by being more direct or where possible leaving out some explanations or moving things to the supplementary materials. An example, at the start of your data section:

"In addition to the identification of droughts, an objective is to connect them to atmospheric circulation. Consequently, reanalyses are pivotal datasets for this study. A reanalysis is a combination of historical weather observations and weather forecasting models, using a method called data assimilation (Slivinski et al., 2019; Valler et al., 2024). This approach provides a comprehensive dataset that represents fields consistent with each other through the application of physical laws within the model and the elimination of spatial and temporal gaps."

In my opinion this whole bit can go. The objectives are already listed in the introduction, and I think the readers of WCD can be expected to know what reanalyses datasets are. Hopefully by critically assessing the text as a whole, you will be able to achieve a more streamlined and clear manuscript, that better showcases your work and results.

A substantial effort was made to streamline the manuscript by removing background information that can reasonably be assumed for the readership of WCD. In particular, the general description of reanalysis datasets was shortened, and unnecessary methodological explanations were removed.

The manuscript was revised throughout to improve conciseness. In particular, the former *Study area, Data* and *Data Evaluation* sections were merged and condensed. The *Discussion* was reorganised and shortened, with several subsections merged or reduced to improve the overall flow and to better emphasise the interpretation, significance, and limitations of the results.

Overall, these revisions reduced the length of the main text from 31 to 20 pages (excluding references), while improving its focus and preserving its core scientific content.

My second point is a hesitation regarding the fact that all analysis (outside Fig 8a/b) is related to SPI or SPEI trends/values, rather than the actual physical input variables of precipitation, temperature and evapotranspiration. I struggle with terms like 'dryness', when SPI/SPEI indicate a relative position on the wet/dry spectrum. Please consider performing or adding (in the SI?) Some analysis on the physical variables, and using the language to go with that (related to my point on clarity before). E.g. I believe that when you write about a 'decline in dryness' measured by SPI, you in fact mean an increasing precipitation trend. As a physically trained climate scientist that is easier to interpret.

The physical input variables underlying drought were analysed to complement and clarify the physical interpretation of the results. Their long-term trends are now presented directly in the main manuscript (Fig. 4a), replacing the previous summary of SPI-3 and SPEI-3 trends, while the latter has been moved to the Supplementary Material. This revision follows the reviewer's suggestion and facilitates the physical interpretation of the results by directly presenting the underlying hydroclimatic variable (precipitation, AED, temperature).

In addition, we agree that the term *"dryness"* is not the most appropriate wording in the context of SPI-3 and SPEI-3, which represent standardised indicators of relative hydroclimatic anomalies and drought conditions rather than direct measures of physical dryness. The terminology was therefore revised throughout the manuscript to better reflect the meaning and direction of the analysed indicators. Depending on the context, references to *"dryness"* were replaced by more precise terms such as *"dry or wet conditions"* or *"drought conditions"*, while references to *"dryness trends"* were replaced by *"drying/wetting"*, *"hydroclimatic trends"*, or wording directly referring to the underlying variables (e.g. precipitation or AED), where appropriate.

Finally, I miss a critical evaluation in the discussion of the validity of your results and assumptions. Throughout the results section there are quite a few places where the three reanalysis products thoroughly disagree with each other, even when a common period is used. No statements or conclusions are made regarding this, whilst for me this is very important. Furthermore, given your interesting Fig. 5, should you not comment more on the influence of interannual/multidecadal variability on trends in relation to climate change trends (signal to noise discussion)? In a similar matter, I miss a critical discussion on the use of Thornthwaite, which as far as I know is a dated method of calculation PET. I fully understand that when only monthly data is available, this is your only option, but this will influence your computed trends and results, so I expect a critical discussion of implications.

A dedicated *Strengths and limitations* subsection was added to the *Discussion* to provide a more critical evaluation of the robustness of the results and the main methodological assumptions. This subsection discusses the agreement and discrepancies

between the three reanalysis products and their implications for the confidence that can be attributed to the different conclusions. In particular, the influence of multidecadal variability on the interpretation of hydroclimatic trends is now explicitly addressed, highlighting the greater robustness of long-term trends compared with shorter periods that are more strongly affected by internal climate variability. In addition, the use of the Thornthwaite formulation for AED estimation is now critically discussed. Its limitations are evaluated through comparisons with Penman–Monteith estimates presented in the Supplementary Material, together with a discussion of the assumptions and uncertainties associated with both approaches.

As such, my advise to the editor is to offer the authors a chance to reword, and consider the manuscript again after major revisions have been made.

We thank the reviewer for their thorough evaluation of the manuscript and are grateful for the time and effort dedicated to providing detailed and constructive comments, which have significantly helped improve the quality of the work. We address all comments point by point below.

Major comments

- I'm based in the Netherlands, a prominent part of your region of interest. I'm most familiar with the Dutch literature on drought, and these are often missing in your reference list. I list some, as a suggestion to extend your literature review, note this is not a complete list. These are a mix of trend analyses, event analyses and future projections: Philip et al. (2020), Zscheischler et al. (2020), Huang et al. (2021), Van der Wiel et al. (2021), Mens et al. (2022), Aalbers et al. (2023).

The suggested references were reviewed and incorporated where relevant throughout the manuscript, particularly in the *Introduction* and *Discussion*. They strengthen the regional context by complementing the existing literature on historical drought variability, recent drought events, hydroclimatic trends, and future projections in western Central Europe. Several of these studies also provided valuable support for the interpretation of regional features identified in this study, including the recent spring drying, coastal wetting in the Netherlands, and projected future drought evolution. We appreciate the reviewer's suggestions, which helped improve the completeness and regional relevance of the literature review.

- Line 157: I'm confused how one can reliably add detail to datasets by interpolation, as you do by upscaling from 1.8 degree resolution to 0.25 degree resolution. Have you verified what the resulting (spatial) fields look like?

We fully agree that interpolation does not add spatial detail or new information, and this was not its purpose. The interpolation of 20CRv3 and ModE-RA to the ERA5 grid was performed solely to enable spatial comparison on a common grid while retaining the higher spatial resolution of ERA5 over this relatively small study area. The interpolated fields were verified to ensure that the large-scale spatial patterns were preserved. At the aggregated regional scale considered here, this resampling

does not affect the conclusions. The manuscript was clarified accordingly, emphasising that the analysis focuses on mid- to large-scale features and that local-scale differences remain subject to uncertainty because of the differing native resolutions of the reanalyses.

- Why have you chosen to use ERA5-land rather than normal ERA5? You don't use any of the land surface variables in your analysis, that would benefit from the additional offline land-model calculations?

ERA5-Land was initially considered to assess the sensitivity of the evaluation to spatial resolution because of its finer grid spacing than ERA5. However, it was acknowledged that ERA5-Land is not simply a higher-resolution version of ERA5, but a separate land-surface reanalysis. As it did not provide additional information relevant to the objectives of this study and was not used in the core methodological framework, it was removed entirely from the manuscript to avoid unnecessary complexity and potential confusion. This simplification does not affect the results or conclusions of the study.

- Line 306: In your analysis of the quality of the different reanalyses datasets, you fully rely on SPI/SPEI variables which are normalised, derived variables. Please check the reliability of input variables as well, e.g. PR, TAS, PET. Similarly, I think your analyses of trends may be helped by analyses of the input variables rather than SPI/SPEI alone.

The suggested analyses of the physical input variables were incorporated into the trend analysis. Long-term trends in precipitation, temperature, and atmospheric evaporative demand are now presented in the main manuscript and in the Supplementary Material (Fig. 4a; Table S2), providing a more direct physical interpretation of the hydroclimatic changes underlying the drought indices.

The evaluation of the reanalysis products, however, remained based on SPI-3 and SPEI-3 rather than on the individual input variables. Because the primary objective of the evaluation is to assess the ability of the reanalyses to identify drought events consistently, evaluating the drought indices themselves is the most relevant approach. In addition, SPI-3 and SPEI-3 are standardised variables, allowing performance to be assessed relative to the local hydroclimatic variability rather than absolute precipitation/AED amounts. Extending the evaluation to each individual meteorological variable would considerably broaden the scope of the manuscript. Furthermore, the uncertainties in these reanalyses are primarily associated with precipitation, whereas temperature and AED generally exhibit much smaller biases; this point is now discussed in the *Strengths and limitations* subsection. The revised manuscript therefore focuses the evaluation on the variables directly used throughout the subsequent analyses while complementing their interpretation with the physical input variables.

- Line 205 and 327: Thornthwaite, through its solo dependence on temperature, likely shows a stronger (too strong) trend over the analysis period towards higher PET values. This can somewhat be seen in Figure S1, where the red line has a stronger slope towards low SPEI values (drought). Similarly, in line 327, SPEI-3 drought occur at the end

of the time series, whilst SPI-3 droughts occur more at the beginning. Can you analyse, maybe in a similar figure to S1, how the PET differences are, i.e. without the step towards SPEI? Please then also add a point in the discussion on this, and how you think the choice for Thornthwaite has impacted your results (section 7.4 I guess).

Additional analyses comparing Thornthwaite and Penman–Monteith AED estimates were added to the Supplementary Material. These analyses assess the differences between the two AED formulations directly, before and after their integration into SPEI-3, and evaluate their influence on drought identification and long-term trends.

The results show that Thornthwaite slightly overestimates AED in western Central Europe, but that this does not substantially affect the main conclusions regarding drought events, hydroclimatic trends, or atmospheric circulation. The implications of using Thornthwaite AED are now explicitly discussed in the new *Strengths and limitations* subsection, including the uncertainty associated with AED estimation.

- Line 403: what does SPI3 minus SPEI3 physically mean, it is a difference between normalised quantities that partially rely on the same input variable. Wouldn't it make more sense to asses the trend in (normalised if you wish) PET, rather than in this strange quantity?

We agree that the physical interpretation of the SPI-3 minus SPEI-3 difference was insufficiently explained in the original manuscript. In the revised manuscript, the interpretation of long-term hydroclimatic changes is therefore primarily based on the underlying physical variables, with trends in precipitation and atmospheric evaporative demand (AED) now presented directly in the main manuscript (Fig. 4a).

The SPI-3 minus SPEI-3 difference is now used only as a complementary diagnostic in the Supplementary Material to assess the relative contribution of AED to drought conditions. Unlike AED trends alone, this comparison accounts for the standardisation of the drought indices and therefore for the seasonal variability of the climatic water balance. As discussed in the revised manuscript, the relative contribution of AED to drought conditions is not solely determined by the magnitude of AED trends, but also by the variability of precipitation and AED within each season. Consequently, the SPI-3 minus SPEI-3 difference provides complementary information that cannot be inferred from AED trends alone. The revised manuscript was clarified accordingly, particularly in the discussion of the changing contribution of AED to drought evolution.

- Section 6.2: This section is very dense with numbers and comparisons, but as a result difficult to follow and distill your main results from. There are a lot of differences mentioned, which are partially attributed to differences (biases?) in reanalysis datasets, but without coming to a conclusion. The influence of multidecadal variability is not mentioned, even though we know (and you showed in Figure 5) that this is of large influence especially for precipitation.

Section 6.2 was substantially revised to improve readability and better emphasise the main findings. The amount of numerical detail and inter-comparisons was reduced, and clearer conclusions are now provided throughout the section.

The influence of multidecadal variability is now explicitly discussed in the new Strengths and limitations subsection, where its implications for trend interpretation and the robustness of long-term versus short-term trends are critically assessed. This organisation was adopted to maintain a clear distinction between the presentation of the results and their interpretation.

Minor comments:

- Line 21: “raising particular concern” while I agree there is concern, I’m not sure it is more concerning in this region then in other regions globally. The Mediterranean, or Sahel, see larger trends towards droughts, transitioning to seasons without significant rainfall. The trends in Central Europe are relatively smaller, the climates relatively wetter and cooler.

We agree that drought trends and associated impacts are more severe in several other regions of the world. Our intention was not to imply that western Central Europe is of greater global concern, but rather to emphasise the increasing attention given to droughts in Europe over recent decades. To avoid this ambiguity, the sentence was revised to refer to "growing concern in Europe" instead of "raising particular concern."

- Line 45: the transition zone between drier South and wetter North in climate projections is also a prominent feature of the Netherlands national climate scenarios (Van der Wiel et al. 2024).

We appreciate this suggestion. However, to improve the conciseness and focus of the manuscript, the discussion of future hydroclimatic trends was removed from the *Introduction*. The statement now refers only to the observed historical transition between drying in Southern Europe and wetting in Northern Europe. As van der Wiel et al. (2024) focuses on future projections, it was therefore no longer appropriate to include this reference in that context.

- Line 67: You only use CHDs six times in the manuscript, do you need the abbreviation?

We agree that the abbreviation was not used frequently enough to justify its introduction. The abbreviation "CHDs" was therefore removed from the revised manuscript.

- Line 120/Figure 1: Generally I’d advise to avoid using reanalysis-based precipitation products, can you recalculate these values using (for example) the E-OBS gridded observational data? Do mention this limitation in your discussion as well.

We appreciate this suggestion. As part of the substantial streamlining of the manuscript, Figure 1 was removed because it only provided general climatological context and was not essential to the study. Consequently, the use of reanalysis-based precipitation for this figure is no longer relevant. The limitations associated with reanalysis precipitation fields are, however, now discussed explicitly in the *Strengths and limitations* subsection.

- Line 157: Please add details on your method to bias adjust to the 1979-2008 climatology, do you remove all trend or calculate the bias in this period and adjust the full dataset according to this bias?

The manuscript was clarified to better describe the climatological adjustment procedure. No detrending was applied, and the temporal variability of the datasets was preserved. Monthly anomalies from 20CRv3 and ModE-RA were first calculated relative to their respective climatologies and then multiplied by the corresponding 1979–2008 ERA5 climatology to express all datasets on a common climatological reference framework for comparison. This procedure preserves the relative temporal variability of each dataset while adjusting the climatological baseline to match ERA5. Its purpose is therefore not to remove long-term variability, but to ensure comparability between the reanalyses while retaining their inherent temporal evolution.

- Line 191: I think you have calculated SPI/SPEI at grid point level, and then spatially averaged, but am not 100% sure due to "In order to obtain the SPI-3 and SPEI-3 values for the entire western Central Europe region, precipitation and PET values for all land cells have been averaged over the study area.", can you clarify?

We clarified that SPI-3 and SPEI-3 were computed at both the grid-cell and the regional scales. For the regional indices, precipitation and atmospheric evaporative demand (AED) were first area-weighted across all land grid cells within western Central Europe, and the drought indices were then computed from these aggregated time series. This approach was adopted because the standardisation underlying SPI-3 and SPEI-3 is non-linear; consequently, indices computed from aggregated inputs are not equivalent to spatial averages of grid-cell indices. Following Spinoni et al. (2016), aggregation prior to index computation preserves a more consistent regional drought signal. The corresponding description in the manuscript was revised to remove this ambiguity.

- Line 205: Thornthwaite, through its solo dependence on temperature, likely shows a stronger (too strong) trend over the analysis period towards higher PET values. This can somewhat be seen in Figure S1, where the red line has a stronger slope towards low SPEI values (drought). Can you make a similar figure to S1, just for PET, to quantify this?

As mentioned in the response to the major revision comment above, the requested analyses were added to the Supplementary Material. These include a direct comparison of AED trends estimated using the Thornthwaite and Penman–Monteith formulations, together with the corresponding SPEI-3 trends derived from each method. This allows the sensitivity of both

AED and SPEI-3 trends to the choice of AED formulation to be quantified and provides a clearer assessment of its influence on the main conclusions.

- Line 225: your reference period for circulation is different from the period for SPI/SPEI calibration, is this done on purpose?

We thank the reviewer for pointing this out. This was indeed an error in the original manuscript. The Z500 anomalies are calculated relative to the 1947–2008 reference period, not the 1979–2008 period as originally stated. The latter period is used only for the climatological adjustment between reanalyses, as described in the *Data* section. The manuscript was corrected and clarified accordingly.

- Line 262: "Whilst the aforementioned two variables are hypothesised to remain constant across time for each reanalysis, this is not the case for the former." I'm not a native speaker, but I don't think this makes grammatical sense. 'The former and the latter', refers to a list of two items, you have three.

We agree that the original wording was grammatically incorrect and potentially confusing. This part of the manuscript was substantially rewritten during the revision, and the ambiguous use of *former* and *latter* was removed. The revised text now describes the different sources of uncertainty more directly and clearly.

- Line 272: the Climate Explorer asks to be referred to as a dataset/tool as well, see https://climexp.knmi.nl/citation.cgi.

The appropriate reference and citation for the Climate Explorer dataset and tool were added to the revised manuscript following the recommended citation guidelines.

- Figure 3: the SPI reference period is different in the caption than in the main text, the y-label should state SPI-3 [10-year], the figure doesn't include labels a) and b), I suggest adding to the caption that the shaded area is in the colour of the dataset with the large error relative to the station measurement.

The y-axis label was corrected to specify SPI-3 [10-year], clarifying that the figure shows low-frequency SPI-3 variability. The inconsistency in the reference period between the caption and the main text was also corrected, and the missing panel labels (a and b) were added. Finally, the caption was revised to clarify that the shaded areas are shown in the colour of the reanalysis dataset considered unreliable over the corresponding extended period.

- Line 357: why does ERA5 'support' this finding, if it is the only reanalysis in Figure 5 that includes this decade?

We agree that the wording "ERA5 supports" was misleading, as ERA5 is the only reanalysis covering the 2010s and therefore does not provide an independent confirmation. The sentence was revised accordingly to remove the implication of cross-dataset validation and now simply reports the ERA5-based result for this period.

- Line 362: I'm not sure what you mean with 16th driest conditions here.

We agree that the original wording was unclear. The intention was to refer to the theoretical expectation that, under a standard normal distribution, approximately 16% of values fall below the −1 threshold used to define SPI-3 and SPEI-3 drought conditions. The sentence was revised accordingly to explicitly refer to the 16% expected from the standardised drought threshold (SPI/SPEI < −1), thereby clarifying the intended meaning.

- Figure 5: please do not use the rainbow colour scale, and please consider having a discrete colour bar (e.g. 10 colours) rather than a continuous, to help reading the figure. The discs are very difficult to see, maybe you can consider a different way of showing those. Just brainstorming: an additional subfigure with a line graph, or larger discs below the map.

We agree that the original colour scale was not optimal for interpretation and readability. The figure, now Figure 3 in the revised manuscript, was therefore revised using a perceptually uniform colormap from the khroma R package (Frerebeau et al., 2023), adapted into a diverging scheme and discretised into classes to improve interpretability while remaining suitable for colour-blind readers. The size of the discs was also increased to enhance readability.

- Line 369: what is the mean dryness? If it is the mean SPEI3 value, is it not by definition that the two quantities are closely correlated?

We agree that the wording "mean dryness" was unclear and potentially misleading. The terminology was therefore revised throughout the manuscript. The corresponding paragraph was also rewritten to clarify that the regional drought index is used as a representative measure of regional drought conditions, whose relevance is supported by its strong relationship with the spatial extent of drought. The emphasis is therefore no longer on the correlation itself, but on the ability of the regional index to characterise drought conditions over western Central Europe.

- Figure 6: a) please replace the rainbow colour scale, consider choosing a white-centred colour scale instead. Why is De Bilt/Frankfurt/Paris not included as a disc? b) I had to zoom in to about 200% to be able to read these graphs, consider modifying such that that is no issue anymore (line width, text size, font weight etc). Maybe all the slope values and p-values can be written in a table?

Figure 6, now Figure 4 in the revised manuscript, was substantially revised following the reviewer's suggestions. The original colour scale was replaced by a perceptually uniform diverging colormap with a neutral midpoint to improve interpretability while avoiding misleading visual emphasis. The station representation was also clarified: Uccle is the only station shown as a disc because it is the only station with a complete record covering the full 1947–2023 period considered in this analysis, as now explicitly stated in the figure caption.

In addition, to improve readability, the figure was reorganised and now presents the precipitation and AED trends more concisely in Figure 4a. The original panels displaying the slopes and p-values were moved to the Supplementary Material.

- Line 380: this is slightly silly thing, but SPI or SPEI don't measure 'dryness', they are an indicator of the relative dry or wet state of the past months. As such I don't think the term 'dryness trends' are correct, maybe just (meteo)hydrological trends? Also in caption of section 7.3.

We agree that the term "dryness" was not the most appropriate wording in the context of SPI-3 and SPEI-3, which represent standardised indicators of relative hydroclimatic anomalies and drought conditions rather than direct measures of physical dryness. The terminology was therefore revised throughout the manuscript to better reflect the meaning of the analysed indicators. Depending on the context, references to "dryness" were replaced by more precise terms such as "dry or wet conditions" or "drought conditions", while references to "dryness trends" were replaced by "drying/wetting", "hydroclimatic trends", or wording referring directly to the underlying physical variables (e.g. precipitation or AED), where appropriate.

- Figure 7: can you add somewhere (maybe in the caption) over how many seasons these were calculated? So for ERA5, 31% of how many droughts are related to the Baltic High? This may give the reader a sense of (statistical) stability.

Figure 7, now Figure 5 in the revised manuscript, was revised to include the number of seasonal drought events considered for each dataset. This additional information provides the context needed to interpret the reported percentages and better assess their statistical robustness.

- Figure 8: a) please specify what the box (P25/P75?) and whiskers (min/max?) show, b) why not show this as four bar charts?, c) please replace the rainbow colour map, why not include De Bilt/Paris etc as a disc?

Figure 8 (now Figure 6 in the revised manuscript) was revised following the reviewer's suggestions. (a) The figure caption now explicitly defines the boxplot elements, including the median, interquartile range (25th–75th percentiles), whiskers, and outliers. (b) The original representation of seasonal occurrence was replaced with bar charts to facilitate comparison of event counts across circulation types. (c) The original rainbow colour scale was replaced with a perceptually uniform diverging colour map with a neutral midpoint to improve interpretability and avoid misleading visual emphasis. The station

representation was also clarified: Uccle is the only station shown as a disc because it is the only station with a complete record covering the 1947–2023 period analysed, as now explicitly stated in the figure caption.

- Line 510: "(Fig. 6b, Fig. S8b and Fig. S9b)" should this be Fig 8b?

We thank the reviewer for pointing this out. This was indeed a typographical error in the original manuscript. Following the reorganisation and renumbering of the figures during the revision, the correct reference is now Fig. 6b.

- Figure S11: What are the units on the y-axes? What is shown on the x-axis, are these all identified droughts consecutively? So a modified time axis?

The caption of Figure S11, now Figure S14 in the revised Supplementary Material, was clarified to specify the units shown on the y-axes and to explain that the x-axis does not represent continuous chronological time, but rather a concatenated sequence of identified drought events displayed consecutively for comparison.

- Line 550: what do you mean here by "given the natural variability"? If 1921 was a large outlier, than would we expect an even larger (or slightly smaller if due to PET trends) to occur in the next 100 years?

We agree that the original wording was unclear. The paragraph was substantially revised to better distinguish the perspectives provided by SPI-3 and SPEI-3. It now clarifies that the increasing contribution of atmospheric evaporative demand indicates an increasing potential for recent droughts to reach severities comparable to historical events when assessed with SPEI-3.

- Line 551: maybe add some literature on multi-year droughts in the region? Van der Wiel et al. (2023) or Van Mourik et al. (2025).

Van Mourik et al. (2025) was added alongside van der Wiel et al. (2023), strengthening the discussion of multi-year droughts in western Central Europe and complementing the existing literature.

- Line 565: "Some studies have been linking" should probably be "Some studies have linked"

We thank the reviewer for pointing this out. This part of the manuscript was substantially rewritten during the revision, and the original wording is no longer present.

- Line 640: degree symbol missing. Furthermore, is CC scaling relevant to year-round precipitation? I always assumed it was mostly associated with convective summer rain events, rather than all precip year round? In the KNMI23 projections, extreme precip sometimes goes beyond CC (Van Dorland et al. 2023).

K was adopted throughout the revised manuscript instead of °C to comply with SI conventions.

The discussion of Clausius–Clapeyron (CC) scaling was substantially shortened and clarified. While CC scaling is indeed most commonly discussed in the context of short-duration extreme precipitation, recent studies have also related it to long-term mean precipitation changes (e.g. Cavitte et al., 2020; Dalaiden et al., 2020). The revised manuscript therefore retains a brief discussion of the broadly consistent year-round precipitation trends while emphasising that the contrasting seasonal responses primarily highlight the role of atmospheric circulation.

- Line 735: I think you mean negative precipitation anomalies rather than precipitation deficits Deficits have been defined in many different ways in the literature so might cause confusion. (Also please check for other places in the manuscript.)

We agree that the term "precipitation deficits" may be ambiguous. The manuscript was therefore revised to replace this terminology with "negative precipitation anomalies" or "reduced precipitation" depending on the context.

- Line 746: Maybe add to this paragraph something short on temporally compounding or multi-year droughts?

The paragraph referred to in the original manuscript was removed during the substantial restructuring of the manuscript. Nevertheless, the discussion of compound hot and dry events and multi-year droughts was integrated at several relevant points throughout the revised manuscript, where it better supports the interpretation of the results.

References

- Aalbers, Emma E., et al. "The 2018 west-central European drought projected in a warmer climate: how much drier can it get?." Natural Hazards and Earth System Sciences 23.5 (2023): 1921-1946.
- Huang, Yafei, et al. "Long-term trends in agricultural droughts over Netherlands and Germany: how extreme was the year 2018?." Hydrology and Earth System Sciences Discussions 2021 (2021): 1-27.
- Mens, Marjolein, et al. "Integrated drought risk assessment to support adaptive policy making in the Netherlands." Natural Hazards and Earth System Sciences Discussions 2022 (2022): 1-24.
- Philip, Sjoukje Y., et al. "Regional differentiation in climate change induced drought trends in the Netherlands." Environmental Research Letters 15.9 (2020): 094081.
- Van Dorland, R., et al. "KNMI national climate scenarios 2023 for the Netherlands." Report WR23-02, KNMI 605 (2023).
- van Mourik, Jonna, et al. "Regional drivers and characteristics of multi-year droughts." Weather and Climate Extremes 48 (2025): 100748.

- Van der Wiel, Karin, Geert Lenderink, and Hylke de Vries. "Physical storylines of future European drought events like 2018 based on ensemble climate modelling." Weather and Climate Extremes 33 (2021): 100350.
- Van der Wiel, Karin, et al. "KNMI'23 climate scenarios for the Netherlands: storyline scenarios of regional climate change." Earth's Future 12.2 (2024): e2023EF003983.
- Zscheischler, Jakob, and Erich M. Fischer. "The record-breaking compound hot and dry 2018 growing season in Germany." Weather and Climate Extremes 29 (2020): 100270.

References

- Aalbers, Emma E., et al. "The 2018 west-central European drought projected in a warmer climate: how much drier can it get?." Natural Hazards and Earth System Sciences 23.5 (2023): 1921-1946.
- Cavitte, M. G. P., Dalaiden, Q., Goosse, H., Lenaerts, J. T. M., and Thomas, E. R.: Reconciling the surface temperature–surface mass balance relationship in models and ice cores in Antarctica over the last 2 centuries, The Cryosphere, 14, 4083–4102, https://doi.org/10.5194/tc-14-4083-2020, 2020.
- Dalaiden, Q., Goosse, H., Klein, F., Lenaerts, J. T. M., Holloway, M., Sime, L., and Thomas, E. R.: How useful is snow accumulation in reconstructing surface air temperature in Antarctica? A study combining ice core records and climate models, The Cryosphere, 14, 1187–1207, https://doi.org/10.5194/tc-14-1187-2020, 2020.
- Frerebeau, N. (2023). KHROMA: colour schemes for scientific data visualization. *Zenodo*.
- Spinoni, J., Naumann, G., Vogt, J., and Barbosa, P.: Meteorological Droughts in Europe: Events and Impacts: Past Trends and Future Projections, https://doi.org/10.2788/79637, 2016.
- Van der Wiel, Karin, Geert Lenderink, and Hylke de Vries. "Physical storylines of future European drought events like 2018 based on ensemble climate modelling." Weather and Climate Extremes 33 (2021): 100350.
- van der Wiel, K., Batelaan, T. J., and Wanders, N.: Large increases of multi-year droughts in north-western Europe in a warmer climate, Clim. Dyn., 60, 1781–1800, https://doi.org/10.1007/s00382-022-06373-3, 2023.
- Van der Wiel, Karin, et al. "KNMI'23 climate scenarios for the Netherlands: storyline scenarios of regional climate change." Earth's Future 12.2 (2024): e2023EF003983.
- van Mourik, Jonna, et al. "Regional drivers and characteristics of multi-year droughts." Weather and Climate Extremes 48 (2025): 100748.

Author comment to RC2 in "Droughts in western Central Europe and associated atmospheric circulation patterns since 1844"

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Comment of the reviewer

Comment of the authors

Summary

The author team investigates drought variability in western Central Europe since 1844 using different reanalysis products. Thereby, they focus on links between drought conditions and atmospheric circulation to identify potential drivers of drought. They further employ 2 drought measures. The results show that recent extreme droughts have historical precedents, illustrating strong multidecadal variability of drought conditions in western Central Europe. Assessing the trend behavior of droughts, the author team find seasonal differences as well as differences between the two drought indices. The authors hypothesize that the diverging trends of the two drought indices sued are indicative to an increasing role of atmospheric evaporative demand (AED). They back this hypothesis with changes in circulation patterns towards AED prone patterns highlighting the role of dynamics over thermodynamics in this region.

General statement

The author team certainly touches interesting questions on whether recent droughts are unprecedented and more importantly how drivers of drought in in western Central Europe have changed over time and what is the role of atmospheric dynamics versus thermodynamics. In this sense the study is of high importance and deserves publication in WCD. Still, in the current form there are major concerns, the structure of the manuscript needs to be improved, the manuscript needs substantial shortening, the atmospheric circulation classification needs better explanation, and the discussion needs more focus. So, I recommend acceptance after major revisions.

We thank the reviewer for their thorough evaluation of the manuscript and are grateful for the time and effort dedicated to providing detailed and constructive comments, which have significantly helped improve the quality of the work. We first address this general comment, followed by point-by-point responses to the other comments below.

We agree that the original manuscript would benefit from improvements in structure, conciseness, clarity of the atmospheric circulation classification, and a more focused discussion. A substantial effort was therefore made to streamline the manuscript

by removing background information that can reasonably be assumed for the readership of *WCD* and by reducing descriptive repetition throughout the text.

The manuscript was revised throughout to improve conciseness. In particular, the former *Data* and *Data Evaluation* sections were merged and condensed. Section 6.2 was substantially revised to focus on the interpretation of the key findings rather than repeating results already presented in the figures. The *Discussion* was reorganised and shortened, with several subsections merged or reduced to better emphasise the interpretation, significance, and limitations of the results. The description of the atmospheric circulation classification was also clarified, including a more detailed explanation of the clustering methodology, complementary EOF analyses, and additional supporting information in the Supplementary Material.

Overall, these revisions reduced the length of the main text from 31 to 20 pages (excluding references), while improving its conciseness and focus and preserving its core scientific content.

Major comments

1. Structural problems concern the data section, not containing a description of all data sets used (see minor comments), method section (4.1) not explaining PET when it is introduced the first time, and parts of the results belong to the evaluation section.

The *Methods*, *Data*, and *Data Evaluation* sections were revised to improve the overall structure and clarity of the manuscript. Missing dataset descriptions were added, and the introduction of AED in the *Methods* section was clarified. The former *Data* and *Data Evaluation* sections were merged to improve the overall flow and coherence of the manuscript. In addition, material that was more appropriate for the evaluation of the datasets was moved from the *Results* section to the revised evaluation section. These changes provide a clearer separation between the methodological description, dataset evaluation, and scientific results.

2. Method section 4.3: I do not understand how the authors classify atmospheric circulation. It remains unclear to which variable the k-means clustering is applied, is it applied to drought indices as somehow written in paragraph 2 or to Z500?

The *Methods* section was revised to clarify the atmospheric circulation classification procedure. It is now stated explicitly that the k-means clustering is applied to 500 hPa geopotential height (Z500) anomalies, whereas SPI-3 and SPEI-3 are used only to identify drought events. For each drought event, Z500 anomalies are averaged over the month of drought onset and the two preceding months, and these event composites are used as input to the clustering algorithm. The manuscript now also clarifies that separate clustering analyses are performed for SPI-3 and SPEI-3 drought events. In addition, the description of the

clustering methodology was expanded and complemented by an EOF analysis to better place the identified circulation patterns within the dominant modes of atmospheric variability.

3. The discussion is not focused and far to long, see my suggestions below.

We agree that the Discussion was not sufficiently focused and was too long in the original manuscript. The section was therefore substantially reorganised and shortened. Descriptive passages were reduced, several subsections were merged or removed, and the emphasis was shifted towards the interpretation, significance, and limitations of the results rather than repetition of the findings. A dedicated Strengths and limitations subsection was also added to provide a more critical assessment of the robustness of the results and the main methodological assumptions. These revisions substantially improved the focus, coherence, and conciseness of the Discussion.

Minor comments

Throughout the manuscript: The authors do often use the wrong tense, So I suggest using present tense throughout the manuscript except you actively write that person A showed something (example: Paulin et al. (2006= suggested that …), then use past tense. Avoid using form like “have attracted” or “is becoming”.

The manuscript was revised to improve the consistency of verb tense usage throughout, following the reviewer's suggestion. Present tense is now used consistently when referring to established knowledge or the content of the manuscript, whereas past tense is reserved for describing specific findings or actions of previous studies. In addition, numerous sentences were reformulated to improve clarity and readability while maintaining the objective style of scientific writing.

L40: Please add the publications https://doi.org/10.5194/cp-19-2511-2023 and https://doi.org/10.5194/cp-17-887-2021 by Kim dealing with Med droughts during the last Millenium.

We appreciate this suggestion. However, as part of the substantial streamlining of the Introduction, the discussion of long-term drought variability in the Mediterranean region was removed to maintain a clearer focus on the study area. Consequently, the corresponding references were not added, as they were no longer directly relevant to the revised scope of the *Introduction*.

L43: There is a study assessing a lot of different drought indices over Europe which might be of interest to the authors: Raible, C. C., O. Baerenbold, and J. J. Gomez-Navarro, 2017: Drought indices revisited - Improving and Testing of Drought Indices in a Simulation of the last two Millennia for Europe. *Tellus*, **69**, 1296226

We thank the reviewer for this helpful suggestion. The study by Raible et al. (2017) was incorporated into the revised manuscript in the Discussion, where it supports the interpretation of the respective roles of precipitation and atmospheric evaporative demand in shaping SPI-3 and SPEI-3, rather than in the Introduction.

L45: The sentence reads bad, please reformulate.; L51-53: The sentence reads bad, please reformulate.; L59-62: Again the sentence needs to be improved. L58-59: Maybe change to “circulation dynamic is hypothesized to prevail ... land-atmosphere feedback dominate ...”

The highlighted sentences were reformulated to improve clarity, readability, and conciseness, and similar stylistic revisions were made throughout the manuscript where appropriate.

L75-76: Please add publication https://doi.org/10.5194/cp-19-2511-2023 here.

The suggested reference was added to the revised manuscript at the relevant location.

L94: “it is imperative” reads bad, please change.

The expression "it is imperative" was replaced by a more neutral formulation at both highlighted locations to improve the tone and readability.

L103-105: It is weird that section 5 is introduced after section 6.1 and 6.2

The introductory paragraph was removed as part of the substantial streamlining of the manuscript, as it did not provide substantive information and contributed unnecessarily to the overall length.

L116: Which alpine influence have the authors in mind?

The intention was to indicate that the study area was defined to exclude the Alpine region and its distinct climatic influence, rather than to suggest an Alpine influence within the study area. As part of the substantial streamlining of the manuscript, this detailed description of the study area was removed because it was not essential to the objectives of the study.

L122: I would not say that Western Central Europe is predominately forested, as over 63% is not forested. So please reformulate.

The description of land cover was revised to avoid overstating the extent of forested areas. The revised manuscript now refers to the main land use types relevant to drought vulnerability without emphasising their respective proportions.

L140-153: Please state which data assimilation schemes are used in these different reanalysis products.

The requested information on the data assimilation schemes used by the different reanalysis products was added to Table 1.

Section Data: It remains unclear which time resolution is used. I guess it is monthly not is it not stated clearly in this section.

The temporal resolution (monthly) was explicitly added in the *Data* section and added to Table 1 for clarity.

Section Data: I suggest to also introduce the observational station data in this section, so move the paragraph on line 271 and the table 2 to this section.

Following the reviewer's suggestion, the description of the observational station data was moved to the revised *Data* section, providing a more coherent presentation of all datasets before their evaluation. This reorganisation was facilitated by merging the former *Data* and *Data Evaluation* sections.

L156: Change “In order to” to “To” throughout the manuscript.

We thank the reviewer for this stylistic suggestion. "In order to" was replaced by "to" throughout the manuscript where appropriate.

L157: What is bilinear downscaling? For me it sounds like the authors just applied a bilinear interpolation where no new information is added (statistical downscaling adds sub-grid information from observations and dynamical downscaling runs a model in higher resolution adding additional resolved processes). I think this is a bit problematic as a reanalysis produced on a coarse grid like 1.8 degree never will the fine scale processes, so normally, if the authors would like to compare the data sets with each other one should interpolate the finer resolved data sets to the coarse resolution to make a fair comparison. At least the authors need to discuss this issue in the manuscript.

We acknowledge that bilinear interpolation (resampling) does not introduce new information and does not resolve scale mismatches between reanalyses of different native resolutions. Our choice to interpolate 20CRv3 and ModE-RA to the ERA5 grid was motivated by the relatively small spatial domain of the study and the desire to preserve the highest available spatial detail from ERA5, which is considered most reliable during the most recent period. We agree that reanalyses at coarser resolution (e.g. ~1.8°) cannot represent fine-scale processes, and that interpolation does not overcome this limitation. This point was therefore clarified in the revised manuscript by explicitly stating that comparisons focus on mid-to-large-scale patterns and that results at spatial scales below approximately 200 km remain subject to substantial uncertainty due to resolution differences. We also clarified that aggregated regional statistics are not significantly affected by the resampling choice.

L173: Please move the used PET estimation here, which is mentioned at the end of this subsection.

Following the reviewer's suggestion, the description of the AED estimation method was moved to this subsection to improve the flow of the manuscript. In the revised manuscript, the *Methods* section now precedes the *Data* section, allowing the methodological description to be introduced before the datasets.

L175: How is AED calculated?

The description of AED estimation was clarified. The manuscript now explicitly states that AED represents the maximum combined evaporation from soil and transpiration from vegetation under given climatic conditions, assuming no soil water limitation, and that it is estimated using the Thornthwaite formulation. In addition, the terminology was standardised throughout the manuscript by consistently using AED instead of PET, following the recommendations of Katerji and Rana (2011) and DeJonge et al. (2025).

L192-194: You do not need to explain area weighted, so you can change to "... precipitation and PET values for land cells are area weighted. For each month ..."

We thank the reviewer for this suggestion. The explanation of the area-weighting procedure was simplified and shortened to improve the readability of the *Methods* section.

Fig.2: I think this figure is not necessary, it might be useful in a PhD thesis but for a paper the description in the manuscript is sufficient.

We thank the reviewer for this suggestion. Figure 2 was removed from the revised manuscript as part of the substantial streamlining of the paper.

L205-208: Please move to L 174.

Following the reviewer's suggestion, the description of the AED estimation method was moved to the preceding subsection to improve the flow of the manuscript.

238: What is AIC und BIC?

The reference to AIC and BIC in the main text was replaced by the more general term "common clustering diagnostics". The acronyms are now defined in full only in the caption of the corresponding Supplementary Material figure as Akaike Information Criterion and Bayesian Information Criterion, respectively.

262: replace "two variables" with features

We thank the reviewer for this suggestion. This paragraph was substantially rewritten during the revision, and the ambiguous terminology was removed.

L281-291: This paragraph remains unclear: I would rather introduce the metrics in the method section and only discuss the results in this chapter

We appreciate this suggestion. Rather than moving the evaluation metrics to the *Methods* section, they were incorporated into the revised *Data* section together with the dataset descriptions. This restructuring provides a clearer separation between methodology and results while maintaining a coherent presentation of the datasets and their evaluation.

Fig. 3: a) and b) are missing.

Panel labels (a) and (b) were added to the revised figure.

L319: I suggest making a kind of introduction of what will be presented in the results section.

We appreciate this suggestion. However, no separate introduction was added to the Results section in order to maintain the conciseness of the manuscript. The revised *Methods* section already outlines the analyses performed, and the merger of the former *Data* and *Data Evaluation* sections provides a clearer transition to the Results.

L321-324: This part belongs to the evaluation.

Following the reviewer's suggestion, the discussion of the agreement between drought events identified by the different reanalyses was moved to the revised evaluation section, where it is more appropriately presented.

L341-42: Do not make sentences which refer to a coming section, If you discuss this in section 6.2 then make a reference to section 6.1

We thank the reviewer for this suggestion. The text was revised accordingly to avoid forward references and improve the flow of the manuscript.

L350: what are the two drought types here, and how are they measured?

We thank the reviewer for pointing out this ambiguity. The term "drought types" was corrected to "drought indices" throughout the manuscript where appropriate.

Fig.5: do not use rainbow colour scales, it is unreadable for colour blind people (15 % of the word population); Fig.6a: do not use rainbow colour scales; Fg. 8d do not use rainbow colour scales

The original figures used a perceptually uniform colormap from the viridis R package (Garnier et al., 2018), which is suitable for colour-blind readers. However, we agree that a diverging colormap is more appropriate for representing the nature of the variables. The figures were therefore revised using a perceptually uniform diverging colormap from the khroma R package (Frerebeau et al., 2023), which remains suitable for colour-blind readers.

L384: This should be Fig 6b. Correct?; L401: This should be Fig 6b. Correct?; Fig 6b is discussed before Fig 6a, which is almost not discussed.

We thank the reviewer for pointing this out. As part of the substantial revision of the manuscript, the figure was reorganised: part of the original figure was moved to the Supplementary Material and replaced by Figure 4a in the main text. Consequently, the panel numbering and discussion were revised, and this issue no longer applies.

L460: Both clustering, what is meant here?

We thank the reviewer for pointing out this ambiguity. This part of the manuscript was revised to explicitly distinguish the two clustering analyses based on SPI-3- and SPEI-3-defined drought events.

L505: These are just “modes or variability”

We thank the reviewer for this suggestion. The expression "three first natural modes of variability" was replaced by "three dominant modes of variability".

L515: How is high confidence defined?

We thank the reviewer for pointing out this ambiguity. The terminology was revised throughout the manuscript to distinguish statistical confidence, referring to the outcome of statistical tests, from confidence in the robustness or interpretation of the results. Where appropriate, the surrounding text was also clarified to make the intended meaning explicit.

L537-553: What is the message of this paragraph?

The paragraph was substantially revised to better emphasise its main message. It now clearly distinguishes the contrasting perspectives provided by SPI-3 and SPEI-3, highlighting that recent droughts are not unprecedented when considering precipitation alone, whereas the increasing contribution of atmospheric evaporative demand has increased the potential for recent droughts to reach severities comparable to historical events. The discussion of the 1921 drought was also clarified to better place recent events in a long-term context.

L564: Given the low frequency variability of AMO (around 60 yr periodicity) a correlation of -0.3 to –0.68 is not impressive and rather significant over the ERA5 time scale of 70 years, I would argue that this is just noise. So, reformulate or remove this paragraph

We agree that, given the low-frequency variability of the Atlantic Multidecadal Oscillation (AMO) and its characteristic periodicity (~60 years), the correlation metrics computed over the relatively short ERA5 period were not sufficiently robust to support a quantitative interpretation. The paragraph was therefore revised by removing the correlation coefficients and avoiding overinterpretation of these relationships. It now simply states that no substantial relationship was identified between low-frequency drought variability in western Central Europe and the NAO, ENSO, or AMO indices.

L574-589: Well, you selected the area in a way that it shows a uniform behaviour so no wonder that there is a high correlation. This paragraph can be removed.

We agree that the strong relationship between the regional drought indices and the affected drought area is largely expected given the spatial definition of the study region and is not central to the objectives of this study. The corresponding paragraph was therefore removed as part of the substantial streamlining of the manuscript.

Section 7.2 and 7.3, What is the difference between drivers of trends and dynamics behind the droughts. I thought you discuss the driver and say which drivers re responsible for which trends. So, my suggestion is to merge these sections and shorten it to the main findings of the paper wrt existing literature.

Following the reviewer's suggestion, the former Sections 7.2 and 7.3 were merged. The revised discussion now more clearly links the identified atmospheric circulation patterns to the observed hydroclimatic trends and places these findings in the context of the existing literature, while substantially reducing descriptive and overlapping material.

Section 7.4: I suggest removing this section and may include 1-2 sentences of challenges in the conclusions.

We agree that the former Section 7.4 was disproportionately long relative to its purpose and interrupted the flow of the Discussion. The dedicated subsection on challenges was therefore removed. Rather than relocating the discussion, only one brief concluding statement highlighting the main remaining challenges was retained in the Conclusions, as suggested by the reviewer. This restructuring improved the focus and conciseness of the manuscript.

L769: Which processes are meant here?

We thank the reviewer for pointing out this ambiguity. As part of the substantial revision of the Discussion, this passage was reformulated and the ambiguous reference to "both processes" was removed.

References

DeJonge, K. C., Allen, R., Kilic, A., Thorp, K. R., Kukal, M., Marek, G., ... & Trout, T. (2025). Evapotranspiration terminology and definitions. Irrigation and Drainage Engineering, 15(5).

Frerebeau, N. (2023). KHROMA: colour schemes for scientific data visualization. *Zenodo*.

Garnier, S., Ross, N., Rudis, B., Sciaini, M., Camargo, A. P., & Scherer, C. (2018). Package 'viridis'. Colorblind-Friendly Color Maps for R.

Katerji, N., Rana, G. Crop Reference Evapotranspiration: A Discussion of the Concept, Analysis of the Process and Validation. *Water Resour Manage* **25**, 1581–1600 (2011). https://doi.org/10.1007/s11269-010-9762-1

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

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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.

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 manuscript was excessively long and overly detailed in several sections. A substantial effort was therefore made to streamline the manuscript by removing background information that can reasonably be assumed for the readership of WCD and by reducing descriptive repetition throughout the text.

The manuscript was revised throughout to improve conciseness and readability. In particular, the former *Data* and *Data Evaluation* sections were merged and condensed. The *Discussion* was reorganised and shortened, with several subsections merged or removed to better emphasise the interpretation, significance, and limitations of the results. The writing style was also revised throughout to improve clarity and conciseness.

The methodological and visualisation-related comments were addressed individually in the responses below. Overall, these revisions reduced the length of the main text from 31 to 20 pages (excluding references), while improving its conciseness and focus and 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.

We agree that a more process-based analysis of the mechanisms linking circulation patterns to drought development—including moisture transport, atmospheric convergence, cloud cover, radiation, and land–atmosphere interactions—would provide valuable additional insight into drought evolution. Such a comprehensive diagnostic attribution, however, would require dedicated process-based analyses that go beyond the scope of the present study and would constitute a separate study in itself.

To strengthen the physical interpretation of the results, the revised manuscript now includes analyses of the underlying physical variables, namely precipitation, temperature, and atmospheric evaporative demand (Fig. 4a, Table S2), alongside the drought indices. The Discussion was also revised to better interpret the hydroclimatic characteristics associated with the identified

circulation patterns in light of the existing literature. The inability of the present framework to fully resolve processes such as moisture transport, radiative effects, and land–atmosphere feedbacks is now explicitly acknowledged in the Strengths and limitations subsection.

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

- 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 original manuscript. The Z500 anomalies are calculated relative to the 1947–2008 reference period, rather than 1979–2008 as originally stated. This error was corrected and clarified in the revised manuscript.

- 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).

We thank the reviewer for these comments. First, the manuscript was clarified to explain that SPI-3 and SPEI-3 are calculated at both the grid-cell and regional scales. The regional indices, computed from area-weighted precipitation and atmospheric evaporative demand, form the basis of the atmospheric circulation analysis, whereas the grid-cell indices are used to investigate spatial variability.

Second, the interpretation of the SPI-3 minus SPEI-3 difference was clarified. This quantity is not intended to represent a physical variable, but rather an indicator of the relative contribution of atmospheric evaporative demand (AED) to hydroclimatic variability compared with precipitation alone. To complement this relative perspective with a more direct physical interpretation, the revised manuscript now presents long-term trends in precipitation, temperature, and AED in Figure 4a and Table S2. The SPI-3 minus SPEI-3 comparison is retained in the Supplementary Material because, unlike AED trends alone, it accounts for the variability of the climatic water balance and therefore better reflects the relative contribution of AED to hydroclimatic trends.

- 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).

We acknowledge the reviewer's concern regarding the use of the Thornthwaite formulation. Although more physically based formulations are generally preferable, the revised manuscript now includes a comparison between Thornthwaite and Penman–

Monteith estimates in the Supplementary Material, showing that Thornthwaite provides comparable long-term behaviour over the study region. In addition, the implications and limitations of using the Thornthwaite formulation are now explicitly discussed in the Strengths and limitations subsection of the *Discussion*.

- 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.

We agree that the original figure was overly complex and difficult to interpret. Following the reviewer's suggestion, the figure was substantially simplified by moving the low-frequency time-series panels to the Supplementary Material. The long-term trends are now summarised in Figure 4a in the main manuscript, providing a clearer presentation of the principal hydroclimatic changes while retaining the complementary information in the Supplementary Material.

- 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. The methodology underlying Figure 8a was clarified both in the *Methods* section and in the figure caption. The revised manuscript now explicitly states that the plotted values correspond to the coefficients of the seasonal EOF modes of atmospheric circulation, rather than SPI-3 or SPEI-3 values. The figure caption was also revised to more clearly describe the construction and interpretation of the figure.

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

Following the reviewer's suggestion, the *Discussion* was substantially reorganised and shortened. The former Sections 7.2 and 7.3 were merged, and the revised discussion now focuses more directly on the identified atmospheric circulation patterns, their hydroclimatic characteristics, and their interpretation in the context of the existing literature, while reducing descriptive and overlapping material.

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 make a summary; they could connect the relationship between the weather patterns and the modes of climate variability or 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 original Conclusions placed disproportionate emphasis on the European High circulation pattern. The revised Conclusions now provide a more balanced synthesis of all four identified circulation patterns by highlighting their distinct hydroclimatic signatures and their evolving associations with the observed seasonal hydroclimatic trends. The European High still receives particular attention because of its relevance to the emergence of spring droughts and its strong association with positive AED anomalies. The relationships between the circulation patterns and the dominant modes of climate variability are discussed in the revised Discussion rather than in the Conclusions.

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!

We fully agree that transparency and reproducibility are essential and support the FAIR principles. The analysis code will be made publicly available upon publication of the article, as stated in the Data and code availability section of the revised manuscript.

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.

We agree that the subplot labelling and referencing were inconsistent in the original manuscript. Panel labels were added where needed to improve readability and ensure consistent referencing between the figures, captions, and main text. The order in which panels are discussed in the Results section was also revised where necessary, and the size of plot labels was increased to improve legibility.

Specific comments:

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 were very helpful and were carefully considered throughout the revision of the manuscript.

In the following, we respond to the most important points in detail. The remaining comments were also addressed directly in the revised manuscript where appropriate.

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 former Data Evaluation section could divert attention from the main results of the study. However, as noted by the reviewer, the evaluation remains important for assessing the robustness of the reanalysis products used in the subsequent analyses. The former Data and Data Evaluation sections were therefore merged and substantially condensed to improve the overall flow and coherence of the manuscript, while retaining the essential evaluation in the main text. Less essential material, including the former Table 2, was transferred to the Supplementary Material. This restructuring substantially reduced the length of the evaluation and allows the manuscript to focus more directly on the scientific analyses while preserving the information needed to assess the reliability of the datasets.

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 was revised and refocused on its socio-economic relevance and vulnerability to drought impacts. The description was also streamlined to provide a clearer and more direct motivation for the selected region.

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.

We agree that the information provided by the original Figure 1 was not essential to the main manuscript and that much of the general climatological context can be obtained from existing atlases. As part of the substantial streamlining of the manuscript, Figure 1 was therefore removed.

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 was not necessary for understanding the drought indices. Figure 2 was therefore removed from the manuscript as recommended. The introduction of SPI and SPEI was retained, as these indices may not be familiar to all readers of WCD, as also noted by the co-editor, but the corresponding methodological description was substantially shortened to retain only the essential 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 raising this point. The indices are computed at both the grid-cell and regional scales, but for different analytical purposes. For the regional-scale analysis, precipitation and AED are first area-weighted across all land grid cells to obtain regional mean time series, from which the indices are then computed. This approach is necessary because the standardisation underlying the indices is a non-linear transformation; therefore, an index computed from spatially aggregated inputs is not equivalent to the spatial average of indices computed separately at each grid cell. This distinction is particularly important here because the regional drought classification is subsequently used to analyse atmospheric circulation patterns. Following Spinoni et al. (2016), aggregation prior to index computation preserves a consistent regional drought signal. The revised manuscript now explicitly clarifies that the indices are computed separately at the grid-cell and regional scales and that spatial aggregation is performed before computing the regional indices.

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.

We acknowledge the reviewer's concern regarding the use of the Thornthwaite AED formulation. Penman–Monteith estimates were also computed for ERA5 to assess the sensitivity of AED and SPEI-3 trends to the choice of 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, particularly ModE-RA, preventing a homogeneous application of this method across the three reanalyses and the full analysis period (Córdova et al., 2015).

The Thornthwaite formulation was therefore retained to ensure methodological consistency across the three reanalyses. Comparisons of AED and SPEI-3 trends obtained with Thornthwaite and Penman–Monteith are now provided in the Supplementary Material, allowing the sensitivity of the results to the AED formulation to be assessed directly. The implications and limitations associated with the use of Thornthwaite are also explicitly discussed in the Strengths 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.

The choice of four clusters was further justified and clarified in the revised manuscript. In contrast to the CAP7 and CAP9 classifications (Schwander et al., 2017; Pfister et al., 2025), which characterise daily weather-type variability across the full spectrum of atmospheric conditions, the present analysis focuses specifically on seasonal circulation patterns associated with drought conditions. This more restricted framework is therefore expected to capture a smaller diversity of circulation states.

Several common clustering diagnostics were also tested but did not provide consistent guidance regarding an optimal number of clusters, with suggested values ranging from two to six clusters. The final choice of four clusters was therefore based on the interpretability, physical coherence, and separability of the resulting circulation patterns. This rationale is now explained more clearly in the Methods section.

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, together with their associated spatial patterns for each season and reanalysis, is now provided in Figure S10 of the revised Supplementary Material and explicitly referred to in the manuscript. This addition clarifies the identification and interpretation of the EOF modes discussed in the Methods and Results sections while keeping the main manuscript concise.

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.

We acknowledge that ERA5-Land is a land-surface reanalysis, meteorologically forced by ERA5 and based on downscaled estimates, and is therefore not simply a higher-resolution version of ERA5. We also acknowledge the inconsistency arising from its limited use in the evaluation and the original Figure 1.

ERA5-Land was therefore removed entirely from the revised manuscript to avoid unnecessary complexity and ensure consistency among the datasets used throughout the study. The original Figure 1 was also removed as part of the substantial streamlining of the manuscript. This simplification does not affect the results or conclusions of the study.

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.

We agree that the original description was confusing and incorrectly referred directly to the MAE rather than to the 10-year moving average of SPI-3. The text was corrected accordingly, and the missing panel labels (a) and (b) were added to the figure.

The purpose of the figure is not to display a time series of the MAE itself, but to use the MAE threshold as an evaluation criterion while showing the 10-year moving average of SPI-3. This representation allows the direction of deviations from observations to be visualised, indicating whether a reanalysis tends to be systematically drier or wetter than the observations,

which would not be apparent from an MAE time series alone. The shaded areas indicate periods during which the MAE threshold is exceeded, thereby making temporal drifts visually identifiable. The description of the figure and its interpretation were revised to make this distinction explicit.

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.

We agree that this analysis corresponds to the evaluation of the reanalyses rather than to the analysis of drought dynamics and their drivers. Its purpose is to assess the temporal robustness of drought-event identification in the reanalyses before conducting the subsequent analyses of drought characteristics and atmospheric circulation.

The Matthews Correlation Coefficient is used to evaluate the ability of the reanalyses to identify drought events, 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* and *Data Evaluation* sections were merged and condensed, providing a clearer separation between dataset evaluation and the subsequent scientific results.

Line 350 – drought types or drought indices?

We thank the reviewer for pointing out this ambiguity. The intended term was "drought indices" rather than "drought types", and the text was corrected accordingly.

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.

We thank the reviewer for pointing out this inconsistency. As part of the substantial reorganisation of the trend analysis, the original figure was revised, with part of the information moved to the Supplementary Material and the long-term trends now presented more clearly in Figure 4a in the main manuscript. The text and figure references were revised accordingly to clearly distinguish results based on ERA5 from comparisons across the three reanalyses.

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"?

Yes, this interpretation is correct, and we thank the reviewer for highlighting the ambiguity. The *Methods* section was revised to clarify that k-means clustering is applied to 500 hPa geopotential height (Z500) anomalies, rather than directly to the drought

indices. SPI-3 and SPEI-3 are used solely to identify drought events, and separate clustering analyses are then performed on the Z500 anomalies associated with SPI-3- and SPEI-3-defined drought events, respectively.

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 of the methods section.

The clustering procedure was clarified in both the *Methods* section and the figure captions. Two independent clustering analyses were performed on Z500 anomalies: one for drought events identified using SPI-3 and one for drought events identified using SPEI-3. The clustering shown in Figure 5 of the revised manuscript is based on SPEI-3-defined drought events, while the corresponding SPI-3-based analysis is provided in Figure S9 of the revised Supplementary Material. These figures are now explicitly referenced in the *Results* section.

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.

The order in which the panels of the former Figure 8 were discussed was revised to ensure chronological referencing. We also agree that the original waffle-chart representation was unnecessarily difficult to interpret. It was therefore replaced by a bar chart, providing a clearer representation of the seasonal distribution and number of drought events associated with each circulation pattern.

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 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 terminology and description were confusing. The term "index" was incorrectly used in this context, whereas the values discussed correspond to EOF coefficients rather than SPI-3 or SPEI-3 values. The terminology was corrected throughout the manuscript to remove this ambiguity.

The methodology underlying the figure was also clarified both in the *Methods* section and in the figure caption. The revised manuscript now explicitly states that the plotted values represent coefficients of the seasonal EOF modes of atmospheric circulation and that the drought indices are used only to identify the drought events associated with each circulation pattern. The description and interpretation of the figure were revised accordingly.

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 drought indices was confusing and could lead to misinterpretation. To improve clarity and avoid presenting anomalies of standardised anomalies, the figure was revised to show the original SPI-3 and SPEI-3 values directly. Although this representation provides less visual contrast, it offers a more direct and interpretable representation of drought intensity.

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 index).

We agree that the term "intensity" was ambiguous in this context. It was replaced by "mean intensity" to clarify that it refers to the mean value of the drought index during an event, consistent with the definition provided in the *Methods*, rather than to 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.

We agree that the discussion of wet periods diverted attention from the main focus of the manuscript. This discussion was therefore removed as part of the substantial restructuring and shortening of the *Discussion*.

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

We agree that the title of the former Section 7.4 could lead to confusion regarding its purpose. As part of the substantial restructuring of the manuscript, the dedicated subsection was removed rather than renamed. Only a brief concluding statement on the main remaining challenges was retained in the Conclusions, substantially reducing the emphasis on this topic and improving the focus of the manuscript.

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

We acknowledge that the original Conclusions placed disproportionate emphasis on the European High circulation pattern. The revised Conclusions now provide a more balanced synthesis of all four identified circulation patterns by highlighting their

distinct hydroclimatic signatures and their evolving associations with the observed seasonal hydroclimatic trends. The European High still receives particular attention because of its relevance to the emergence of spring droughts and its strong association with positive AED anomalies.

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

We agree that repeated citations contributed to the excessive length of the original *Introduction*. As part of its substantial streamlining, repeated references were reduced where appropriate. This substantially improved the conciseness and focus of the *Introduction*.

References

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- 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. https://doi.org/10.1659/MRD-JOURNAL-D-14-0024.1, 2015.
- Pfister, L., Wilhelm, L., Brugnara, Y., Imfeld, N., & Brönnimann, S.: Weather type reconstruction using machine learning approaches. Weather and Climate Dynamics, 6(2), 571-594. https://doi.org/10.5194/wcd-6-571-2025, 2025.

955
- Schwander, M., Rohrer, M., Brönnimann, S., and Malik, A.: Influence of solar variability on the occurrence of central European weather types from 1763 to 2009, Clim. Past, 13, 1199–1212, https://doi.org/10.5194/cp-13-1199-2017, 2017.
- Spinoni, J., Naumann, G., Vogt, J., and Barbosa, P.: Meteorological Droughts in Europe: Events and Impacts: Past Trends and Future Projections, https://doi.org/10.2788/79637, 2016.