Author comment to editorial comments (2nd round) in "Droughts in western Central Europe and associated atmospheric circulation patterns since 1844"

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Editorial comments

Comment of the authors

1) The last sentence of the abstract does not read well: (i) Droughts have become increasingly associated with ... intense droughts is a circular argument, and (ii) I don't understand how the first part of the sentence is "providing a dynamical explanation for the recent emergence of spring drying". Di you say that the European High pattern became more frequent in spring?

Thank you for pointing this out. We agree that the previous formulation was ambiguous. By "intense droughts", we intended to indicate that drought events occurring under the European High pattern are particularly intense compared with those associated with the other circulation patterns. Regarding spring drying, we did not find that the European High itself has become more frequent in spring, as circulation patterns were only classified during drought events. Rather, the proportion of drought events associated with the European High has increased over time, and this pattern occurs predominantly in spring, a feature that was not specified in the abstract. We have revised the sentence to clarify these two points.

2) L82 and elsewhere: "Section" should be "Sect."

"Section" has been replaced by "Sect." throughout the manuscript.

3) The caption of Fig. 1 does not read very well: I find "data evaluation" not very clear, why not start the caption with sth like "Time-series of ..."? Mention the 4 stations in the caption and correct for typos (double comma). The legend for the colours is much too small.

Thank you for this suggestion. We agree that the previous caption did not clearly describe what is shown in the figure. However, we retained "Data evaluation" at the beginning of the caption to distinguish this figure from the subsequent drought results. The caption now starts with "Data evaluation: time series of…", and the four reference stations (Uccle, De Bilt, Frankfurt, and Paris) are now explicitly mentioned. We also corrected the typos and increased the size of the colour legend.

4) Also the caption of Fig. 3 is not easy to understand "decadal counts ... complete decade"(??). Please reconsider all your captions and make them as clear and reader-friendly as possible. It is a pity if the reader don't understand what you show in the figures!

Thank you for pointing this out. We agree that the previous formulation of the caption was unnecessarily difficult to understand. We have rewritten the caption of Fig. 3 to describe more explicitly what is shown in each panel and have reconsidered the other figure captions throughout the manuscript to improve their clarity.

5) Figures 4 and 5: colorbar labels are much too small.

Thank you for pointing this out. The size of the colour-bar labels in Figs. 4 and 5 has been increased to improve readability, and the same adjustment has been applied to Fig. 6.

6) Consider improving the last paragraph of your paper. Currently, the reader might regard these aspects as obvious ("we knew this before reading your paper"), but I think you can be a bit more specific about what you found out about circulation, P vs. E etc.

Thank you for this suggestion. We have revised the entire *Conclusion* section to more clearly emphasise the specific findings and novelties of the study, particularly the long-term perspective on drought and seasonal hydroclimatic trends, the changing contribution of precipitation and AED, and the evolving associations between droughts and atmospheric circulation patterns. The final paragraph has also been rewritten to provide a more specific synthesis of these findings.

Author comment to Anonymous Reviewer #1 (2nd round) 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 thoroughly reworked their manuscript, according to the tracked-changes version almost no text is left unchanged. The new text reads very pleasant, and tells a logical story, my compliments for this effort.

I only have minor points now, and would be happy to see this manuscript published once these have been dealt with.

Thank you very much for your positive assessment. Your previous review greatly helped us improve the manuscript, particularly by encouraging us to consider AED and precipitation alongside the drought indices and to strengthen the discussion of multidecadal variability. We greatly appreciate your constructive comments and have addressed the remaining points as thoroughly as possible below.

Comments

I highly appreciate the shorter, more to the point introduction. I would however say, that the statement in the last paragraph: "… western Central Europe—defined here as Belgium, the Netherlands, Luxembourg, northern France, and western Germany—received comparatively limited attention." Followed by zero (!) References on drought is a bit extreme, please add here some of the existing literature. I provided a list of relevant studies in my previous report. They do appear in the discussion, but a few references are needed here as well. You don't need to add much text, one or two sentences can be enough, but you want to refer to the state of the art somewhat in your introduction.

We agree that the existing literature on drought variability in western Central Europe should be better acknowledged in the *Introduction*. We have added references to studies investigating drought trends over recent decades in the Netherlands and Germany (Philip et al., 2020; Huang et al., 2021), as well as previous work relating droughts to daily weather types (Bešťáková et al., 2024). These additions also help clarify how the present study complements previous work by extending the analysis both temporally and spatially and by characterising seasonal circulation patterns associated with droughts.

You could consider switching the data section with the method section in order, as I believe you refer to the data in the methods section, but not the other way around.

We agree that introducing the data before the methods provides a more logical flow and have therefore switched the order of the *Data and evaluation* and *Methods* sections.

Line 418: "The increasing association of droughts with the European High suggests that compound hot and dry events may become increasingly characteristic feature of droughts in western Central Europe, in agreement with the growing influence of AED identified in this study." This sentence made me wonder (note I'm non-native English), whether you are implying if one is causal of the other or not? Do you mean to say there is a forced circulation trend, towards more European High, which increasingly makes drought compound hot-dry? Or the other way round, due to forced warming and higher AED, do circulation patterns with high AED now increasingly show up? Or are you just making an observation? That there are more hot-dry droughts, and that there is more European High-droughts?

Thank you for pointing out this ambiguity. We did not intend to imply a causal relationship in either direction. Rather, this statement combines two observations from our results: droughts have become increasingly associated with the European High, which is characterised by large AED anomalies, while the contribution of AED to droughts has also increased. Together, these findings suggest that compound hot and dry conditions are becoming increasingly characteristic of droughts in western Central Europe. We have revised the sentence to make this interpretation clearer and avoid implying causality.

Line 482: Would it be suitable to add here a statement/question on the role of multi-decadal variability in this last dry decade? I love your last paragraph, I think putting the question of multi-decadal variability slightly more prominent somewhere is a fitting 'conclusion' for your work.

Thank you for this suggestion. We agree that the role of multidecadal variability in the exceptional dryness of the 2010s is an important open question. As our analysis does not disentangle internal climate variability from externally forced warming, we cannot attribute the recent dry decade to either mechanism. We have therefore made this question more prominent in the conclusion by highlighting the respective roles of long-term warming and multidecadal variability in shaping recent drought trends. We retain separately the finding that the 2010s are no longer exceptional when AED is neglected, which highlights the growing contribution of AED and its importance for drought monitoring in western Central Europe.

Textual/figures/tables:

Line 32: Unclear what is central Europe, can you explain in a few words? Later you define Western-Central-Europe, which is great, but until there central Europe could mean Austria, or anything larger.

We have added a brief geographical definition of Central Europe at its first mention: “Central Europe—the area roughly delineated by the North Sea, the Alps, the Carpathians, and the Baltic Sea”.

Line 80: please add: “the three datasets used in this study.”

“The three datasets used in this study” has been added as suggested.

Figure 3b: what do the two numbers underneath the plot show? For ERA5: “20.16”

The numbers below the maps represent the absolute number of drought months at the regional scale. We have clarified this in the revised caption of Fig. 3, specifying that these numbers are shown below each map in both panels (a) and (b).

Figure 5b: it would be nice to have a legend here as well, even if the colours are the same as in Fig 1.

A legend has been added to Fig. 5b to make the panel easier to interpret independently.

Line 235: maybe add that the seasonal cycle of AED is such that it is very low in winter (relative to precipitation, or relative to summer AED).

Thank you for this suggestion. We preferred to address this point in more detail in the *Discussion*. At Line 235, we therefore removed “despite substantial warming”, as this statement already introduced interpretation into the Results while providing an incomplete explanation of the seasonal differences in AED. In the *Discussion*, we now clarify that the smaller increase in the contribution of AED to SPEI-3 trends in winter results from the strong energy limitation of winter evapotranspiration rather than from weaker warming, as winter warming is comparable to the year-round trend. We also note that winter nevertheless shows the largest relative increase in AED per degree of warming (33% K^{-1} in ERA5), reflecting its low climatological AED baseline.

Figure 6c/d: I assume these are for all SPEI-3 droughts, independent of season and year in dataset? It is relevant to be clear here, given the seasonal cycle of AED (my previous point), and the trend in AED (your analysis).

Thank you for pointing this out. Yes, these analyses include all SPEI-3 drought events over 1947–2023, independent of season and year. This also applies to the EOF coefficients shown in Fig. 6a. We have clarified this in the figure caption by specifying that all drought events across seasons and years are pooled.

Line 355: For the summer period, I doubt if SPEI-3 is indeed ‘predominantly precipitation driven’, e.g. SPEI over period JJA or JAS, or is it due to the fact that inter annual variability of precip is so much larger than variability of AED? (I now see the next paragraph answer some of these questions.)

We actually meant that drought events are predominantly precipitation driven, rather than SPEI-3 variability in general. We agree that the previous wording was ambiguous and could be interpreted as referring to the background variability of SPEI-3. The paragraph has been revised to make this distinction clearer and to better describe the changing relative contributions of precipitation and AED during drought events.

Line 400: it is a bit weird that you refer to compound hot-dry events, but list (seemingly?) references to hot and dry separately.

Thank you for pointing this out. We agree that the previous formulation incorrectly suggested that all cited studies specifically addressed compound hot–dry events. We have revised the sentence to distinguish studies linking persistent anticyclonic circulation to hot and dry events separately from those addressing their co-occurrence as compound hot–dry events, and added Ionita et al. (2021) as a reference for the latter.

Line 425: maybe add the model name in Table 1 (IFS?, NOAA?, ECHAM?), to verify this claim.

Thank you for this suggestion. We have added the numerical atmospheric model underlying each reanalysis to Table 1 (IFS CY41R2 for ERA5, GFS v14.0.1 for 20CRv3, and ECHAM6 for ModE-RA). We have also added the differences in numerical models to the description of the three reanalyses in the *Data* section.

Line 439: I wouldn't say multi-decadal variability is a problem for comparison in the common period, as each dataset there experiences the same phases of variability, we can check if they agree in their signals.

Thank you for this clarification. We agree that multidecadal variability does not hinder comparison between reanalyses over the common period, as all datasets experience the same phases of variability. Our intention was rather to emphasise that trends over this relatively short period can be sensitive to its start and end points within the multidecadal variability. We have revised the text accordingly.

References

Bešťáková, Z., Kyselý, J., Lhotka, O., Heilig, M., and Eitzinger, J.: Warm-Season Drying Across Europe and Its Links to Atmospheric Circulation, Earth Space Sci., 11, e2023EA003434, https://doi.org/10.1029/2023EA003434, 2024.

Huang, Y., Weis, J., Vereecken, H., and Hendricks Franssen, H.-J.: Long-term trends in agricultural droughts over Netherlands and Germany: how extreme was the year 2018?, Hydrol. Earth Syst. Sci. Discuss., 1–27, https://doi.org/10.5194/hess-2021-569, 2021.

Ionita, M., Caldarescu, D. E., and Nagavciuc, V.: Compound Hot and Dry Events in Europe: Variability and Large-Scale Drivers, Front. Clim., 3, https://doi.org/10.3389/fclim.2021.688991, 2021.

150 Philip, S. Y., Kew, S. F., van der Wiel, K., Wanders, N., and Jan van Oldenborgh, G.: Regional differentiation in climate change induced drought trends in the Netherlands, Environ. Res. Lett., 15, 094081, https://doi.org/10.1088/1748-9326/ab97ca, 2020.

Author comment to Anonymous Reviewer #3 (2nd round) 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

General comments:

I appreciate the attention and answer of my comments in the rebuttal. The authors considered most of the issues raised, resulting in an improved manuscript.

I am still concerned about the fact that the land-atmosphere processes are not shown and linked the circulation patterns and the evolution of droughts. The conclusions of the study are thus similar to other studies already showed; there are intense droughts in Europe when the jet stream moves north due to weather patterns associated with ridging and blockings. Also, new research highlights the role of changes in atmospheric circulation in driving summer drying (Dunkl et al., 2026, Nat. geo. Sci.). However, I understand that not everything can be studied in one article and the improved manuscript shows other results that are of interest for members of the community, such as the specific patterns by season and the intensity associated with specific patterns.

Another source of concern is still the difference between Thornthwaite and Penman-Monteith (PM). Figure S15 in panels b) show that Thornthwaite has a long-term trend of 5.973 mm/decade and PM of 4.99 mm/decade for summer (spring has a similar difference). ModE-RA does have all variables for calculating PM except for the ground heat flux, which is a relatively minor share of the energy balance in the soil at long time scales (it could be assumed as 0). Although changes in land-use change have other uncertainties. As stated in the previous round of review, the AED is an essential part of the conclusions of the study; thus, the differences in AED are important for the manuscript.

We thank the reviewer for their careful reading and constructive comments. Their input throughout the review process has greatly helped us improve the manuscript, and we appreciate the additional points raised in this round. We address the two general concerns here and respond to the specific comments separately below.

Regarding the link between land–atmosphere processes and atmospheric circulation, we acknowledge that these processes are not explicitly analysed in this study. The general relationship between European droughts and large-scale circulation features

such as ridging, blocking, and northward shifts of the jet stream is therefore not presented as a novel conclusion of our work. Rather, the dynamical contribution of this study lies in the seasonal characterisation of circulation patterns specifically associated with drought events, the corresponding drought intensity and hydroclimatic anomalies, and changes in their association with droughts over time. We agree that the recent findings of Dunkl et al. (2026) are relevant in this context and have added this study to both the *Introduction* and *Discussion* to better contextualise our study.

Regarding AED, we agree that differences between the Thornthwaite and Penman–Monteith formulations are important given the role of AED in our conclusions. However, applying Penman–Monteith consistently over the full study period using ModE-RA would introduce additional uncertainties related to missing meteorological inputs. The variables available in ModE-RA are limited to mean sea-level pressure, 2 m air temperature, total precipitation, 500 hPa geopotential height, vertical velocity, and 850 hPa zonal and meridional winds (Franke et al., 2023). In particular, ModE-RA provides neither relative humidity nor dew-point temperature, while ground heat flux is also unavailable. Therefore, calculating Penman–Monteith AED from ModE-RA would require approximations of several required variables, rather than only assuming a negligible ground heat flux. Given the sensitivity of Penman–Monteith estimates to the required meteorological inputs (Córdova et al., 2015), we consider such a reconstruction insufficiently robust for the present analysis.

Nevertheless, we fully acknowledge that the choice of AED formulation affects the magnitude of the estimated trends. As discussed in the *Strengths and limitations* section and illustrated in Fig. S15, the comparison performed for ERA5 shows that Penman–Monteith produces smaller summer and spring AED trends than Thornthwaite. Importantly, however, this methodological difference affects the magnitude rather than the direction of the identified changes in ERA5. We have therefore retained this limitation explicitly in the manuscript and ensured that the conclusions are restricted to the direction and increasing contribution of AED rather than relying on the precise magnitude of the Thornthwaite-derived trends.

I suggest moving the section 3 Data and evaluation, before the 2 Methods section. In the latter there are some sentences that might not be understood without knowing the data first; e.g. “corresponding to the common period shared by all post-evaluation reanalyses” (line 91).

We agree that introducing the data before the methods provides a more logical flow. We have therefore moved the *Data and evaluation* section before the *Methods* section and adjusted the corresponding references where necessary.

Figure 3 – The labels of the three datasets are minuscule.

We have increased the font size of the dataset labels in Figure 3 to improve readability.

Figure 4 – Year-round trend is a trend calculated on annual time series? In the abstract and conclusions is mentioned as annual, while in the figure apparently is refereed as year-round.

Thank you for pointing this out. The year-round trends are calculated using all monthly values rather than annually aggregated time series. We have therefore replaced "annual" with "year-round" throughout the manuscript, as "annual" could incorrectly suggest aggregation at the yearly scale. We have also clarified in the Figure 4 caption that year-round trends consider all monthly values.

Line 375 – the sentence is convoluted.

We agree that the sentence contained too much information and was difficult to follow. We have split it into two sentences, separating the main interpretation from the numerical values supporting it.

Line 440 – the sentence is no comprehensible. Rewrite.

We have revised the sentence to improve its clarity.

Line 496 – I do not support is called "drought evolution". The manuscript does show that the occurrence of drought events is influenced, or interconnected as authors say, to the atmospheric circulation; but it does not show the continuous short-scale change in hydroclimate and hydrological conditions (evolution of droughts). This might be picky wording, but the evolution of drought conditions is a very important concept that has dire implications. Please, consider also change the used concept in the whole manuscript.

Thank you for pointing out this important distinction. We agree that "drought evolution" may imply the short-timescale development of individual drought events and is therefore not appropriate for describing long-term drought variability. We have revised this terminology throughout the manuscript, including in the conclusion and the title of Sect. 5.3, which is now "Atmospheric circulation and hydroclimatic changes". We retained "drought evolution" only where it specifically refers to the short-timescale development of drought conditions.

Line 505 – I suggest to upload the scripts before the end of the publication process.

The data are now available at the link provided in the *Data and code availability* section. A link to the code repository has also been added, and the scripts will be uploaded during the week of 7–11 September, following further revision to improve their readability.

Given the considerable input that reviewers gave to the current version of the manuscript, I think that their efforts and content contributions deserve an acknowledgement, in the acknowledgment section, even with the reviews being anonymous.

We fully agree and have added an acknowledgement thanking the three anonymous reviewers and the editor for their thorough and constructive comments.

References

Córdova, M., Carrillo-Rojas, G., Crespo, P., Wilcox, B., & Célleri, R.: Evaluation of the Penman-Monteith (FAO 56 PM) Method for Calculating Reference Evapotranspiration Using Limited Data. Mred, 35(3), 230–239. https://doi.org/10.1659/MRD-JOURNAL-D-14-0024.1, 2015.

Dunkl, I., Bastos, A., and Sippel, S.: European summer drying largely driven by atmospheric circulation changes since the 1980s, Nat. Geosci., 19, 914–921, https://doi.org/10.1038/s41561-026-02050-w, 2026.

Franke, J.: ModE-RA - A global monthly palaeo-reanalysis of the modern era (1421-2008): overview, World Data Center for Climate (WDCC) at DKRZ, http://cera-www.dkrz.de/WDCC/ui/Compact.jsp?acronym=ModE-RA_info, 2023.