Review of 'Linking European droughts to year-round weather regimes' submitted to Weather and Climate Dynamics

Overview

The paper investigates the link between 3-month frequency in weather regimes and the 3-month Standardized Precipitation Index over Europe and the entire year. The paper concludes a modest impact of weather regime frequency on precipitation anomalies during droughts.

Overall, the paper is a useful application of an established approach, weather regimes over the North Atlantic, to the study of droughts, which fits within the scope of the journal. However, from the perspective of this reviewer, the methodological choices made in the paper do not seem optimal for the problem investigated and are, in parts, not well justified in the paper. The paper could be significantly improved by investigating some choices further and discussing others in more detail. This reviewer therefore recommends major revisions prior to publication in Weather and Climate Dynamics.

Comments (ordered from major to minor)

- The main finding of the paper, that 'only a fraction of droughts - primarily in western Europe and during winter - can be directly attributed to anomalies in regime frequency.' (Line 7) seems to be a lower bound estimate, as at least part of the influence of weather regimes on drought conditions is lost due to the choices of method. This should be further discussed and investigated in the text.
    - Droughts are defined as the negative exceedance of a chosen SPI index averaged over defined regions of Europe, which are derived from drought concurrence. However, the weather regimes project onto opposite anomalies in some of these regions, even when analysed over the entire year (see Figure 5: EuBL over Scandinavia, AT over West Europe, ScBL over Scandinavia and the Iberian Peninsula). This raises the question of how much signal is lost due to this specific regionalization of Europe. To investigate the sensitivity of the regionalization, the reviewer suggests to investigate the sensitivity to subsampling the time period and analysing the consistency of the identified regions, as well as showing the regions identified when selecting 5 or 7 regions in the Appendix.
    - Furthermore, other approaches to regionalization could be discussed and optionally investigated - such as clustering drought occurrences, and

emerging approach to identify circulation pattern targeted to explain specific impacts could be mentioned or discussed (Bloomfield et al 2020, Rouges et al 2023, Spuler et al 2024,2025).

  - Furthermore, as discussed in section 4.2 and shown in the Appendix (Fig C1), weather regimes can have different precipitation signatures depending on the season which is quite significant for some regions, regimes and seasons. However, the year-round analysis conducted here averages out this signal (see further comments on this in the next bullet point).

- The paper, including the abstract, emphasizes the added value of using year-round weather regimes "as a unifying framework for understanding drought drivers, overcoming the constraints of purely seasonal classification" (Line 11). However, from the existing text, it is not quite clear what the benefits are of a year-round classification. There are significant differences in the precipitation impact of regimes in different seasons, as discussed later in the paper which reduce the potential explanatory power of regimes for drought conditions, but aside from the obvious avoidance of splitting the data into categories, no clear benefit of the year-round definition is discussed in the paper. If this is presented as a key advantage of the paper, the benefits of the year-round definition should be more clearly discussed.

- Related to the point above, in Line 42 the authors write that WR defined by season hinders a systematic analysis of the drought circulation relationship throughout the year - this is not quite true, it is just that this analysis would have to be conducted by season. This statement should be explained in more detail to be included.

- Comments regarding the regime calculation:

  - There is no weighting of the data with the cosine of the latitude mentioned to account for the different area that grid cells at different latitudes cover. This is common practice when dimensionality reducing circulation, in particular when computing weather regimes. Is there a particular reason that this was omitted here? For the weather regimes shown in Figure 4, in particular MTr and ScBL, the center of action is quite far North - if the data was indeed not weighted, this might be a possible reason. This reviewer would suggest trying out the computation of regimes including the weighting, or include convincing reasoning for not doing so.

    - The authors write that the z500 data was detrended between 1991 and 2020 prior to computing the regimes; however, for the full period, 1960-2022, no detrending is mentioned. This would be inconsistent and likely lead to artificial trends in the regimes. Is there a particular reason that this was omitted?

- Section 2.2.2 - Is there a particular reason that a Gamma distribution is fit to the data, of which the skewness coefficient is then calculated? Why is the skewness not directly estimated from the data, circumventing the uncertainty around the distribution fit?

- Section 2.2.2 - The threshold of 7 for skewness coefficients is chosen to define regions where the SPI is a useful metric. This seems slightly arbitrary - why not 3 or 4?

- Appendix A: A robustness criterion, i.e. the robustness of the cluster centres to subsampling the data on which they are computed, is a reasonable metric to choose clusters, as implemented for example. The authors could consider investigating this metric.

- Section 4.1: This section could be improved by mentioning possible physical mechanisms that motivate the investigation of variations in weather regimes and drought frequencies. It should further at least be discussed that calculating a linear trend over the entire period cannot capture non-stationarity due to decadal variability. Furthermore, Non-stationarity or Stationarity would seem a more suitable section title compared to Representativity.

- Sentence starting with 'The Mediterranean area ...' (Lines 24-26). Given the rich literature on future projections of precipitation over the Mediterranean, there should be other citations here to complement the IPCC.

- The conclusions section would benefit from more detail, in particular the last paragraph on potential implications of this paper. In particular the wording 'Our findings could have substantial implications in the field of sub-seasonal to seasonal drought predictions [...] is a bit too generic.

- Abstract (Line 2), please specify the following part of the sentence: *'due to the complex interplay between regional climate variability and large-scale atmospheric circulation.'* This distinction is ambiguous, especially made without prior context in the first sentence of the abstract. Large-scale atmospheric circulation is mostly understood to be a part of regional climate variability. Do you mean the interplay of atmospheric, ocean and land drivers? Or the interplay of thermodynamic and dynamic processes?

- Line 34 and following: *'In this region, seasonal and subseasonal climatic variations are less dependent on tropical teleconnections than in other regions of the world (). Surface climate in this region is driven by more regional phenomena particularly the North Atlantic atmospheric circulation'*. These statements are quite ambiguous and not well evidenced by the cited literature. There is a wide range of papers evidencing the influence of tropical teleconnections in Europe - including the Shaman and Tziperman 2011 paper cited, the North Atlantic circulation itself is influenced by tropical teleconnections, as investigated for example in Cassou 2008 which is cited a few sentences later. The statement 'compared to other regions of the world' is ambiguous.

- Line 160: Why does the result that the variance of the SPI3 averaged over each region is not different from 1 at the 95% confidence level confirm the suitability of the regionalization for the purpose of this study?

- In a number of locations, for example Line 241, the authors write that the average frequency of each weather pattern during the 91 days preceding the droughts is calculated. This wording could be clearer, as the weather regime frequency is calculated precisely during those days over which the SPI is calculated. A clearer way of phrasing this would be 'during the 91 days over which the SPI3 is calculated'.

- Line 436: References should be added to this statement.

- Figure G1: Add axis labels.

- Page 33: More details on Author Contributions

References

Rouges, E., Ferranti, L., Kantz, H., and Pappenberger, F.: Pattern-based forecasting enhances the prediction skill of European heatwaves into the sub-seasonal range, Climate Dynamics, https://doi.org/10.1007/s00382-024-07390-0, 2024.

Spuler, F. R., Kretschmer, M., Kovalchuk, Y., Balmaseda, M. A., and Shepherd, T. G.: Identifying probabilistic weather regimes targeted to a local-scale impact variable, Environmental Data Science, 3, e25, https://doi.org/10.1017/eds.2024.29, 2024a.

Spuler, F. R., Kretschmer, M., Balmaseda, M. A., Kovalchuk, Y., and Shepherd, T. G.: Learning predictable and informative dynamical drivers of extreme precipitation using variational autoencoders, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2024-4115, 2025.

Bloomfield, H. C., Brayshaw, D. J., and Charlton-Perez, A. J.: Characterizing the winter meteorological drivers of the European elec470 tricity system using targeted circulation types, Meteorological Applications, 27, e1858, https://doi.org/10.1002/met.1858, _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1002/met.1858, 2020.