Review of “Linking European droughts to year-round weather regimes” by Savary et al. submitted to Weather and Climate Dynamics

General comments

This study investigates the contribution of year-round weather regime occurrence frequency anomalies to the occurrence of drought events in Europe defined based on 3-months precipitation deficits (SPI3). They find that weather regimes can explain droughts particularly in Western Europe and during winter, but less so in the rest of Europe and during other seasons. These results are very insightful and useful, and will help to better understand drought in Europe. The paper is very carefully written from a methodological point of view, as it thoroughly and objectively justifies most of the involved steps (trends in data, definition of regimes, definition of drought regions, usability of SPI3, seasonality issues). In my opinion, however, it lacks a bit of a deeper meteorological interpretation of the results as well as a better justification of some specific methodological choices, which is why I suggest major revisions. I think, improving on these aspects will make this paper a great contribution to the community.

Major comments

Interpretation of the key result: You focus a lot on how important the weather regime frequency anomaly term, alpha_1 in Eq. 11, is for drought events, which makes sense as it’s the key question of this paper. However, you leave (at least) me with the open but relevant question, what that other, more important term, alpha_2 in Eq. 11, actually is from an intuitive point of view and why it shapes the drought events to first order. Can you improve your discussion and conclusion with respect to this aspect? What are the “processes” hidden behind this term? Is it also related to smaller-scale/synoptic anomalies that do not appear in the low-frequency regime definition? Maybe it would be helpful for a better understanding to look into the regime evolution ahead of some individual events such as the one of 1960-07-04 in Fig. 8, for which regime frequency anomalies had no importance at all. In previous studies (I think some of them include one of your co-authors), this decomposition is applied to understand climate change signals. As far as I understand, in these studies your second term (alpha_2 in Eq. 11) can be interpreted as a more thermodynamically driven term. However, in the drought context here, this does not really make sense, which is why I struggle to understand it…

Role of weather regime persistence and intensity: The frequency of daily regime occurrence 90d prior to a drought – as you define it here – does not tell us anything about regime persistence or regime intensity. For example, two drought events could be preceded by the same frequency of a regime, but in one case it could be one single, long-lasting regime event that is very strong, while in the other case, the regime could appear multiple times but in shorter and maybe weaker spells. I think this could make a meteorological difference. Did or could you make some statistics over the regime duration before drought events, to understand if duration is an aspect that makes regime occurrence “more important” for droughts (for instance you could do your analysis in Fig. 8 but somehow split between longer-lasting and shorter-lasting regimes)? For regime intensity it might be harder to test this though, because you would have to define a regime index similar to other studies you cited. And a thought related to my first comment: Is it possible that your second term, alpha_2 in Eq. 11, is somehow also dependent on regime intensity, since the strength of a precipitation anomaly delta_C_k when being in regime k might be larger in a stronger regime period?

Sensitivity to drought definition: SPI3 droughts are often used as proxies for agricultural droughts due to the relatively long timescale that starts to affect soil moisture anomalies. As far as I know, SPI1 are rather the ones used as proxy for the classic meteorological droughts. Did you apply the same analysis

to SPI1 and find out if regime frequency anomalies play the same (or a more important) role for this shorter, monthly timescale?

Role of “no regime”: Where exactly did and where did you not include the “no regime” in your analysis? For instance, how do the “no regime” frequency anomalies before drought events look like in Fig. E1? Are there any interesting signals there? In principle, even in “no regime” periods you can have short-term extremes (for instance cut-off lows) that can yield significant amounts of precipitation and thus affect the occurrence or non-occurrence of droughts.

Link to similar decomposition studies: A bit related to my first comment, have you considered to further decompose the weather regime frequency anomaly term as Fischer et al. 2025 have done (https://doi.org/10.5194/egusphere-2024-1253; see their Eqs. 3 and then 6), and from there, compute that ratio gamma that relates the contribution of the regime frequency anomaly/changes to the contribution from the intensity anomaly/changes? I haven’t thought this through, so I’m not sure if it makes sense to apply this in your drought context (rather than in the climate change context), but it seems like it should be applicable too. Any thoughts on this?

Quality of the text: Some specific parts of the manuscript need to be improved with respect to language / phrasing. Also, there are quite many typos (particularly in the second half) that can be removed when carefully reading the manuscript. As you will see in the minor comments, I corrected quite many of these little issues, but at some point, I stopped. One example is Section 4.1, which I find very hard to read and it took me quite some time to understand what the actual goal of this section is (especially the first paragraph there needs rephrasing).

Minor comments

L15: … consequences for society …

L16: Maybe replace classically with typically

L17: Maybe better “depending on the physical variables they are associated with”?

L20: Maybe mention that this is mainly the case for the extratropics (the example with the winter drought).

L26: Rainfall where?

L29: … which began between March and May and continued into June …

L34-36: I feel like there are two things mixed here and I would rephrase a bit: Yes, tropical teleconnections influence Europe less than, e.g., North America, but still, even in North America, rainfall anomalies are modulated by weather regimes (but these weather regimes are then modulated by teleconnections). So, I would not say it’s either the teleconnections from the tropics or the local weather regimes, because it’s rather both, but in one case, regimes are more driven by remote processes than in the other case. Furthermore, also European regimes are driven by teleconnections, but for less from the tropics but for instance from the stratosphere etc.

L42: Maybe better “… are often defined separately for each season …”?

L43: … including the transitional …

L62: I would add “daily mean precipitation and 500 hPa…”

L68: … covers …; also, isn’t there a word missing after AR6?

L71: … data at 500 hPa is determined …; also, it sounds a bit odd, because it’s not you “determining” this field – it’s just a field in ERA5; do you refer to computing the anomalies?

L71-72: Is this the same domain as in Grams et al. 2017? Then maybe write this.

L83: Maybe add “… in terms of its climatological probability of occurrence …” or something like this (i.e., the relation to the climatological distribution)?

L86: … the SPI is calculated as the cumulative … which is then subtracted from the long-term median cumulative precipitation … -> I’m not sure if my suggestion is proper, but I think you should somehow mention the fact that it’s an anomaly.

L86: Does the cumulative 3-month window end on that day or is it centered over that day of reference? I think that’s an important detail.

Section 2.2.2: This is a quite nice way of proving the usability of the SPI in different regions. But given that your conclusion is that it’s usable over most of Europe (i.e., your region of interest), I wonder if this section really needs to be in the main manuscript or could be moved to the supplement? I suggest this also because the manuscript is quite long already and includes many different methodological steps the reader has to get familiar with…

L105: I wouldn’t say you use “two distinct methods” – you rather compute two things but look at them together, so it’s rather one approach you use.

L106: … consists of …

Fig. 1: The hatching is too strong such that the underlying colors can hardly be seen – please change this.

L119: … SPI3 time series …

L120: Make sure you don’t mix the tense in writing. In the previous sentence you write in present tense, and in the following in past tense.

L134: Section number is missing in brackets.

L136: What is AR6? Is that a region?

L141: Maybe write something like “spatio-temporal simultaneity” – which is the case, right?

L155-156: I’m not sure I understand this last step with the seasonal variance. What exactly do you mean about judging the “relevance of this decision”? Which decision? Is this step the one that decides when to stop the creation of further regions? And aren’t there various objective metrics to decide how many clusters to retain after a clustering (such as similarity indices etc.)? Couldn’t you apply one of these?

L162: “significantly equal to 1” – can you really say that? At least I never heard it like this.

L163: Are there examples of other regions / clusters in the literature, either related to drought or something related? If yes, it would be nice if you compared yours to them in a few sentences. I am not aware of any, but are your regions, for instance, similar to the heatwave clusters Stefanon et al. 2012 (https://doi.org/10.1088/1748-9326/7/1/014023) or Pyrina and Domeisen 2023 (https://doi.org/10.1002/qj.4394) found?

L166: How relevant is this 10-day threshold for your conclusions? And aren't single days with SPI3 below the threshold already extreme as well (given that they are already based on long 3-month windows), so isn't this threshold a bit too strict? How often do these shorter-than-10d-events occur at all?

L168: Maybe replace "characteristic date" with "time stamp" or "event date"?

Section 2.2.5: What was the reason that you did not directly use the 7 regimes from Grams et al. (2017), but instead redefined them with a slightly adapted domain? Why did you use this different domain? Is it to include more of Eastern Europe (which would be good reason)? In any case, I think it would be good to briefly discuss these choices and also the differences of your regimes compared to the Grams regimes, because the methodological steps you perform seem to be very similar to Grams et al. (2017).

L173: Is there a reason you only used 1990-2020 for defining the regimes, rather than going further back to get a more robust picture? Also, wouldn't going back further make the regimes more similar to the ones in Grams et al. 2017 (which might be something desirable)?

L204-206: What exactly is the criterion for deciding if a regime is cyclonic or anticyclonic? Please specify. Also, it's a bit unfortunate that the Mediterranean Trough regime is a blocking regime, although the trough over the Mediterranean seems to be the key feature (according to your interpretation). Regarding that MTr regime: It's interesting that this appears so distinctly in your regime definition (compared to Grams et al.). Do you think it comes from extending your domain much more to the east, which allows this blocking in northeastern Europe to appear, probably favoring the stationarity of a trough (probably associated with cut-off lows or just Mediterranean cyclone activity) in the Mediterranean? Maybe you can discuss this "additional regime" briefly?

L207: I would mention Grams et al. first here, because this was the original definition.

L207-210: My gut feeling would tell me it is mainly the different domain you used, which made the difference to the Grams regimes, probably followed by the different time periods. I can hardly imagine though that ERA-Interim vs. ERA5 makes a difference, given that regimes are defined on this very coarsely smoothed field Z500.

L211-212: I would personally find it more useful if you sorted the weather regimes according to some similarity principles or according to cyclonic/anticyclonic, rather than frequency of occurrence. But I guess that's a matter of taste and I'll leave it up to you.

L223-225: You should be more specific here: the blocked regimes are not associate with negative P anomalies everywhere, but rather in the high-pressure regions, while there are important (and impactful) positive P anomalies as well! And vice versa for the cyclonic regimes.

Fig. C1: Interesting to see these patterns! Just to be sure, I assume you computed these seasonal anomalies with respect to seasonal climatologies? I guess that would be important as you observe and discuss stronger magnitudes in winter.

L235: “particularly in the December-January-February (DJF) season” is not really needed because you already say before that it’s the winter months in which the magnitudes are larger.

L240: I would stick to “weather regimes”, because “weather patterns” is sometimes used for shorter-lasting / higher-frequency circulation anomalies than weather regimes.

L241: What does this specifically mean, 91 days prior to the drought? Since you look at SPI3, i.e., 3-months events, this should be clarified.

L251: … shown in Fig. 5 …

L262: Better to write “Similarly, on the drought-preceding periods S” or something similar.

Eq. 9: Isn’t there the sum operator missing before the last term, f_k*C_k (or, alternatively, brackets around everything)?

L277: … characterizes …

L280: … illustrated in Fig. 6 …

L284: Which terms do you refer to? And over which regions are these spatial averages? Please specify in the text.

L291: I would not use the term “satisfactory” here, but something more objective…

Table 3: Can you also include the “no regime” here? Also, I personally like Fig. D1 better than this table here – could you consider just using the figure in the manuscript instead of this table?

L297: How do you get to this number of 50 drought events? Isn’t the number of events different from region to region, as shown in Table 2?

L301-302: I don’t understand this sentence about EMed. Do you mean it’s the region in which the highest number of “NS” appears in Table 3? However, is it not a bit misleading to count the number of NS? Because it still seems that EMed is influenced by weather regimes, but by chance just by one (the Zonal Regime), but relatively strongly. So, I don’t think the “number of NS” is a meaningful measure here for saying how strongly a region is influenced by weather regimes. You see my point?

L307: … presents … (instead of is presenting)

L309: What do you mean with “then the amplitude of the signal decreases with the distance”?

L311: Isn’t it better to write “climatological precipitation” than “canonical precipitation”?

L316: Replace “coast” with “North Atlantic”

L317-318: “accurate” with respect to what / comparing to what? Please specify.

L321: Remove “easily”

L341: You repeat yourself here with the statement that you did this analysis for all regions.

Table 4: Please specify in the caption what exactly the middle and right columns show.

L350: … are the ones in which droughts are the most influenced by … (and same for the sentence afterwards)

L351: … by the large-scale atmospheric circulation in the North Atlantic …

Fig. 8: Please explain this figure better in the caption / text, because it's very confusing. What do you mean in (a) with "for all cases where droughts cannot be explained with year-round weather-regimes"? Aren't just all drought events shown in (a), i.e., those that can and those that cannot be explained by weather regimes according to your definition? Also, the explanations for (b-d) are confusing. In the text you write "Panels a) and b) in Fig.8 show the mean of the precipitation anomalies and the mean of the reconstructed signal, in cases explained by anomalous WR frequencies. Figures c) and d) show the cases that are explained by anomalous WR frequencies." Isn't this twice the same?

L360: "faithfully" is a strange term to be used in this context, in my opinion.

Fig. E1: Since it's flipped, you should change "left" and "right" in the caption.

L370: What is the "WE contribution"?

Figure 9: You don't really introduce this in the text, but you suddenly discuss Fig. 9b. Probably you missed it?

L380: Can you refer to the figure again where one can see the frequency anomalies of all droughts (to which you compare)?

L386-387: What exactly do you mean here with "without taking into account the above-mentioned differences concerning the heterogeneity of the reconstruction quality"?

Figure 10: I think it's confusing that you now use red for those droughts that are not explained well by WRs, while red in the figures before was the term that stands for the importance of WR influence.

Section 4: I assume the importance of the regimes for droughts also depends a bit on the set/definition of regimes you use. I would assume that they become more important if you went to a higher number of regimes, because they would explain more of the surface weather variability. Do you agree and can you maybe discuss this briefly in the discussion? However, I don't say it would make sense to use a higher number, because in terms of predictability, a lower number of regimes is generally more useful.

L389: "in contrast to" rather than "with respect to"

L389-391: This is a cool result! Could convective activity (or non-activity) and maybe soil-moisture-related feedbacks on precipitation during summer also contribute to the fact that WRs explain the droughts less well in this season (since convective activity is not so directly linked to the low-frequency large-scale circulation)?

Figure H1: What exactly is the difference shown here? It's a bit hard to understand with the given explanations. Can you maybe refer to the other, previous figures that "flow into" this figure?

L427: Why exactly do you explain the AT-WMed example of Fig. H1? I don't see a very strong difference/signal in the Wester Mediterranean in this example, but much bigger magnitudes in other subpanels.

L433: The last sentence doesn't make sense – you just did explain the ScBL!

L439-440: "Hence, we can expect from this region to present the more heterogeneous pattern of droughts, thereby exhibiting a limitation of the choices to keep only 6 regions during the regionalization method." – This is a heavy sentence to read and I'm not sure I understand. Can you simplify?

L445: I would either directly write "weather regimes" here or then specify "low-frequency large-scale North Atlantic circulation", because you don't just look at the instantaneous large-scale circulation.

L448: What do you mean with "original regionalization method"?

L451-452: At various places in the manuscript – and here – you use "large-scale circulation" (or simply "circulation") as a synonym for weather regimes, but this is not true. The large-scale circulation likely influences *every* drought, but the regimes apparently don't (for instance, a single convective event can be strongly driven by the instantaneous large-scale circulation but not necessarily by a regime). This is because the regimes are a categorization of the lower-frequency circulation and are persistent states. Please rephrase this a bit to avoid mis-interpretation by the reader.

L460: Not sure you introduce ESM before…