Response to the Reviewer

Thank you very much for the thorough comments. We truly appreciate the time the reviewer invested in reviewing our manuscript. Here, we provide our preliminary response, including our plan to address the issues raised. Our responses are in blue.

Major comments:

1) Structure of the paper
The section 2.1 introducing the soil moisture datasets is overly long and excessively detailed. The authors should focus on essential and objective information, such as the datasets used, their main differences, spatial and temporal resolutions, and the analysis period. Additional complementary details (Table 1), while relevant, are not strictly necessary for the analysis and could be moved to the Supplementary Material. Consider this as a suggestion intended to help reduce the overall length of the manuscript.

Thank you for the point. We will shorten the text by removing the extensive data explanation. However, we will keep Table 1, as it better visualizes the differences among the datasets.

2) Datasets
A new version of GLEAM (GLEAM4.2) has been made available and with important updates relative to GLEAM version 3. Authors should consider using this updated version and recomputing the results accordingly.

We obtained the updated GLEAM and will update the manuscript accordingly.

Methods:

3) There is some lack of consistency and clear information on the time period and datasets chosen for the analysis. For example, in Section 4.1, part of the analysis is conducted using all available datasets but considering two different time periods (2000–2019 and 2000–2020), whereas the last section presents results based on only three datasets and over an extended period (1980–2020). I know the authors struggled to find a common temporal coverage among the six datasets. Nevertheless, I would prefer to define a single and longer time period with a consistent spatial resolution among datasets, so that a coherent and robust analysis is employed (even if this requires reducing the number of datasets considered).

We partially agree with the reviewer's point regarding the lack of consistency in the analyzed time periods. However, we want to point out that five gridded soil moisture datasets are used to compare D&O characteristics only in the first part of Section 4.1. For this part, our results show that D&O characteristics vary noticeably across datasets, which we consider an important finding. Restricting the analysis to datasets with the same temporal coverage throughout the entire manuscript, which would limit us to three datasets, would not be sufficient to support this conclusion.

Nevertheless, for the first part of the analysis, where the periods 2000–2019 and 2000–2020 are used, we will adopt a consistent period of 2000–2019 across all datasets. For the rest of the results, we use three datasets that provide longer temporal coverage and restrict the analysis to their common period (1980–2020).

We will clarify why we use five datasets in Section 4.1 and three datasets for the rest of the revised manuscript.

4) I find little justification for prioritizing the presentation of transition periods to drought onset and termination based on precipitation, when drought identification and the corresponding DO and DT are defined entirely using soil moisture—a variable that responds partially to precipitation. While I understand that the results may appear more "appealing" from a statistical perspective (due to a larger number of grid points showing significant trends in Figure5), unless there is a strong and proper justification (which is not presented by the authors), conceptually, I find no clear rationale for defining the onset and termination of a soil moisture deficit while neglecting the role of temperature, land cover, and soil characteristics. This issue is particularly relevant given the strong spatial variability in soil moisture sensitivity to precipitation and temperature, as well as regional differences in the relative contribution of precipitation.

Thank you for this point. To justify our approach, we should first assess the relationship between precipitation and soil moisture, and examine whether precipitation can be used as an indicator of soil moisture droughts across regions with different climatic, land, and soil characteristics. We will include this analysis, along with a more detailed discussion of the complex interactions between soil moisture and atmospheric variables.

Regarding drought indices, we attempted to use more than one definition of drought and drought transitions to assess the sensitivity of drought characteristics to the definition. In doing so, there is a possibility that we may be comparing different types of droughts (meteorological versus soil moisture droughts), which could be misleading. We will discuss this more clearly in the revised manuscript.

5) It would be interesting to assess the sensitivity of the DO&T transition periods to the drought duration. Basically, is the time required to end a drought proportional to the duration of the event itself? If so, could this be linked to changes in land cover and soil properties (porosity/ hydrophobic)? This could be explored by conducting the analysis for distinct groups of droughts categorized by event duration.

We will include scatter plots that link two characteristics (DO&T periods versus Drought durations) in the revised manuscript.

6) I've found some misleading and contradictory descriptions of the drought onset and termination concepts that are undermining the clarity of the methods. In lines 204-205, it is unclear what the authors mean by "after a continuous period of positive SM3". This description does not align to what is shown in Figure 2a, where the onset is marked as the

first month when SM3 drops to values below -1σ, following a sequence of months with SM3> -1σ (including positive and negative values).

This was our mistake. It should be "A drought onset (DO) is the first time step (monthly) in which SM3 reaches below -1σ after a continuous period of **negative** SM3."
We will correct it.

7) Furthermore, in line 205, the authors mention the following: "On the contrary, a drought termination (DT) is the first monthly time step that SM3 reaches above -1σ from the minimum SM3". However, this raises an important question: if SM3 drops below −1σ again in the subsequent month, is it still appropriate to consider the drought as terminated?

In the following sentence, we state that "a drought is fully terminated when SM3 exceeds 0 for two consecutive time steps." Accordingly, a drought is considered ongoing if this criterion is not met, and SM3 remains negative. Given the persistent nature of soil moisture, abrupt transitions from strongly negative to positive values are also unlikely. This definition is adapted from Coats et al. (2015).

We will also modify Line 200: "A drought is a period in which SM3 stays below one negative standard deviation (-1σ)." as this can be misleading.
We are aware that the explanation is not clear, and we will correct it in the revised version.

8) Finally, how sensitive are the results to the choice of the SM threshold? Do the spatial patterns and inter-dataset differences change substantially when SM thresholds other than −1σ are considered?

We have checked that the spatial patterns and the number of droughts are not noticeably sensitive to the chosen threshold, although the drought duration is significantly affected, particularly for lower thresholds.
We decided to use −1σ because this threshold is commonly used to classify moderate droughts in standardized drought indices (e.g., Lloyd-Hughes and Saunders, 2002), which also capture intense droughts.
We will include an explanation of why this threshold was chosen in the revised version.

9) Analysis of the results:
Regarding the analysis focused on the impact of the several modes of variability on DO&T, the authors tend to explain this interlink mainly via precipitation anomalies. Knowing that DO&T are identified through soil moisture anomalies and that precipitation deficits explain only partially fluctuation in soil water content, I feel like the analysis is missing other key aspects. Similarly to Fig. 8, it would be good to show the spatial distribution of the correlation coefficients between the modes of variability and temperature + VPD (perhaps as Supplementary Material). Information about these variables would complement the explanation for the causal link between atmospheric dynamics and soil moisture variability.

We agree with the reviewer's point. To better assess the relationships between precipitation and soil moisture, and soil moisture (and VPD) and circulations, we will perform correlation analysis between these variables and include them in the revised version.

Minor comments:

Line 55: Change accordingly: "Due to the complex and multifaceted nature of droughts (Cook et., 2018), determining the precise timing of initiation and termination of droughts is challenging, which may explain the lack of studies addressing this topic."

Line 60: Change accordingly: "(...) are typically slowly-evolving events, with impacts on ecosystems becoming mainly noticeable in latter stages, after progressed accumulated periods of precipitation and/or soil moisture deficits (...)"

We will modify them.

Line 62: It is unclear what is meant by "dry" or wet conditions". In this context, this terminology is vague and lacks scientific rigor.

It refers to a hydroclimate condition that helps to determine the exact initiation or termination of droughts. We will simply modify the sentence as "These factors collectively make it challenging to define when droughts have initiated or ended."

Line 74: " Parry et al. (2016) reviewed drought terminations in the British Isles (..)". It reads a bit weird. Please rephrase it.

We will modify the sentence as "Parry et al. (2016) examined drought terminations in the British Isles and identified distinct synoptic events associated with changes in streamflow and drought demise."

Line 110: Remove the following: "(...) and many solely observation-based soil moisture datasets are generally short and not continuous in time and space".

We will remove it.

Line 120: What do the authors mean by "present"? For instance, GLEAM4.2a only goes until December 2023 Anyway, this information is unnecessary. The important thing is to mention what was the time period defined to conduct the analysis which is something I don't find mentioned explicitly in the manuscript.

We will update our analysis with GLEAM4.2a, and modify the employed time period. But as we commented in (1), this data description section will be shortened in the revised version, leaving some descriptive information in Table 1.

Line 126: Change accordingly to keep consistency: "The spatial resolution of SoMo is 0.25° × 0.25°.
Line 130: Remove the following: "The output from GLDAS version 2.1 is used taking the period 2000–2020".

Same as the comment above.

Line 140: GLEAM4 uses precipitation from MSWEP v2.8 (https://www.nature.com/articles/s41597-025-04610-y) that can be downloaded here (https://www.gloh2o.org/).

Thank you for the information. We will update the dataset accordingly.

Line 147: "Modes of circulation patterns". Typically in the literature these are referred to as "Modes of atmospheric circulation variability". Please due the necessary changes over the whole manuscript.

We will replace the term with "Modes of atmospheric circulation variability", and some also with "Modes of Variability".

Line 151: Change accordingly: The NAO index was retrieved from the NCAR climate data guide (https://climatedataguide.ucar.edu/type/climate-indices/circulation/nao), while the other indexes were obtained from KNMI climate explorer (https://climexp.knmi.nl/).

We will change the sentence.

Line 153-156: "the index is calculated as the leading modes of Empirical Orthogonal Function (EOF)". This lacks scientific rigor. If I got it right the authors used the PC-based NAO index (Hurrel - NAO), which relies on the PC1 obtained from decomposing the SLP spatiotemporal field using a principal component analysis. The leading mode of the EOF gives the main spatial pattern of SLP variability, while the true "NAO index" is obtained from its time series (PC1). Check also the lines 161-162.

We will be clearer with that description by changing the text to: "The index corresponds to the first principal component (PC1) obtained by decomposing spatiotemporal fields of sea level pressure anomalies over the Atlantic sector (20°–80°N and 90°W–40°E) using Empirical Orthogonal Function (EOF) analysis."

Line 165: Change accordingly. "We considered Europe as our region of study, particularly the central (CEU) and Mediterranean Europe (MED)"

Lines 173-174: Change accordingly: Surface soil moisture levels over a layer of 10cm depth were used. While GLEAM, SoMo, Noah, and CLM-TRENDY provide information directly for this layer, the corresponding value for ERA5-Land is obtained by estimating how (...)"

We will change the sentences as suggested.

Line 192: Do you mean something like: "(...) where an individual time step t considers the *mean soil moisture conditions over the previous two months, from t-2 to t0.*

We will correct the sentence to: "The SM3 time series have a monthly temporal resolution, where each time step t represents soil moisture conditions accumulated over a three-month window consisting of the current month (t_0) and the two preceding months (t_{-1} and t_{-2})."

Why was a three-month time window chosen? Was a sensitivity analysis performed using different time windows, and if so, how sensitive are the results to this choice?

We chose a three-month window because it represents the typical mean seasonal timescale in our study region. This choice is consistent with our analysis, which focuses on the seasonality of DO&T timing and associated circulation patterns. The results would be sensitive to the choice of the time window, as different window lengths mean different cumulative periods and may therefore capture droughts operating on different timescales. We will include a more detailed discussion of these aspects in the revised manuscript.

Lines 197-198: Rephrase it please

We will rephrase the sentence to: "In addition, we compute monthly precipitation anomalies (ΔP) by subtracting the 2000–2014 climatological annual cycle from precipitation amounts P at each grid point."

Lines 200-201: "A drought is a period when SM3 stays below one negative standard deviation (-1σ)". By "period," do you mean one month, multiple months, and must the months be consecutive?

Also refer to our response in (7). A period can be a month or multiple months as long as they meet the definition of droughts and DO&T. However, a period within a drought must be consecutive. We will clarify the definition as we mentioned in (7).

"A threshold of -1σ in a standardized drought index corresponds approximately to the 15.9th percentile level". Considering that SM3 was computed pixel-wise, how was exactly this value derived? Can you clarify?

The 15.9th percentile threshold corresponds to the probabilistic distribution below −1σ for all standardized indices. Because all indices are standardized, these percentiles are the same for all pixel-wise grid points, and this approach allows for comparisons across points in space. We will clarify this better in the revised version.

Lines 221-222: "The 12-month scale is chosen because it is the typical time scale of drought indices". This is misleading… The scale used truly depends on what type of drought you are interested in identifying (meteorological, hydrological, agricultural). Also the reference Mo, 2011 does not say anything regarding this issue.

We agree with the reviewer; therefore, we will remove this in the revised version.

Lines 250-251: How do the authors plan to count the number of occurrences of a mode of variability? The mode is always there, the only things that change are either the signal or the Intensity.

For the frequency, we counted the number of months with positive or negative signals. For the intensity, we averaged the values. We will clarify this in the revised version. We will add this detail in the revision.

Line 258: An equal time period should be used in order to ensure that any differences in the results are fully explained by biases across datasets.

Refer to our response in (3).

Line 261: I know what P-12 stands for… However this acronym is not defined in the manuscript.

We will include the definition in the new version.

Line 278-281: I understand the authors' point that certain regions exhibit longer drought onset and termination (DO&T) periods, consistently across datasets, drought phases, and transition definitions. However, this idea should be explained more clearly, and the sentence should be rewritten for better English and readability. "In general, CEU tends to show slightly longer DO&T duration than MED, in which this difference is more pronounced in the DT". This reads weird

We will add more discussion on this and correct the sentence.

Line 292: Is really the "study period" 1980-2020? I see no consistency here. So far the analysis was conducted for the periods 2000-2019 and 2000-2020…

We will change it to "the period 1980–2020". Also, refer to our response in (3).

Figure 5. I suggest depicting the statistically significant values with some kind of hatches (dots), contours or shades. The way it is represented right now makes it hard to identify which ones are really significant.

We will change the hatches to darker dots for a better visualization.

Figure 7. I would move this figure to Supp. Material. Although important, it shows results that are only a complement to the analysis and are not pivotal. Please take into account that this is just a suggestion aiming to shorten a bit the size of the manuscript.

We agree with the reviewer. We will implement this in the new version.

Lines 408-409: " A longer transition period to DO means that more cumulative P deficit is required to initiate DO than cumulative positive P anomalies needed for DT" . Poor English. Please rephrase it.

We will replace it with "The longer transition period associated with DO suggests that its initiation depends on a larger accumulation of precipitation deficits compared to the positive precipitation anomalies required for DT."

Line 412-413: "The longer transition periods to DO observed in their studies, as well as ours, suggest that DO is more predictable than DT". What is the causality between both things? Because it's easier to predict large-scale modes of variability than other transient eddies? This needs to be explored a bit more.

DT are more sensitive to small-scale (e.g., synoptic) events, also associated with transient eddies, that can bring moisture. We will add more discussion on this in the revised version.

Line 420-421: "We analyzed the potential origins of this discrepancy but did not identify specific influences of P or evapotranspiration." Maybe lower high-frequency (daily/weekly) P and E variability in ERA5?

We will change the sentence as: "We examined whether differences in precipitation or evapotranspiration could explain this discrepancy, but found no clear influence in standard diagnosis, although differences in higher-frequency variability (e.g., daily to weekly) may still play a role."

Line 436-442: Agree. I suspect that drought termination (DT) is more sensitive to short-lived precipitation events and, consequently, highly influenced by transient eddy systems, which are less constrained by seasonal patterns (?).

We agree with the comment. The same as for Line 412-413, we will extend the discussion on this.

References

Coats, S., Smerdon, J. E., Cook, B. I., & Seager, R. (2015). Are simulated megadroughts in the North American Southwest forced?. *Journal of Climate*, *28*(1), 124-142. https://doi.org/10.1175/JCLI-D-14-00071.1

Lloyd-Hughes, B., & Saunders, M. A. (2002). A drought climatology for Europe. International journal of climatology, 22(13), 1571-1592. https://doi.org/10.1175/0.1002/joc.846