Reply to Comments by Referee #2

Comment 1: The paper is well-written and clearly mentions the research gap. It would be more interesting if the authors kept the objective as a point order (**consistent**) before concluding the introduction, so that the reader remains clear about the objective before moving on to the methodology.

Reply 1: We thank the reviewer for the careful reading and for the constructive suggestion regarding the structure of the introduction. We agree that listing the objectives in a clear point-by-point format before concluding the introduction will improve reader's clarity about the work before they move ahead. We will include the point-wise research objectives in the last part of introduction and make sure that the following points are incorporated:

In summary, the objectives of this study are as follows:

1. To investigate the projected changes in key hydrological drought characteristics (duration, severity, and intensity) across 200 UK catchments under three future warming scenarios.
2. To apply and compare results from nonstationary and stationary EVA using a Bayesian framework to quantify the role of nonstationarity in governing future hydrological drought risks.
3. Understanding the future evolution of hydrological drought characteristics in UK, specifically for rare events with robust estimation of uncertainty.

Comment 2: The author highlighted the role of seasonal controls in future drought. In lines 45 and the abstract, it has been highlighted, but it is unclear whether it is also considered an objective of the study. However, it's clear from the results section onwards, and this is indeed very interesting. However, the authors have not highlighted the importance of seasonality in relation to drought in the research gap and have **not cited any relevant literature**.

Reply 2: Thank you for the raising the important point and mentioning that seasonal results are interesting and worth highlighting. We have tried to discuss the season-based results subsequently in detail as we have set the whole analysis on seasonal scale. As mentioned in response to your previous comment, we agree that this point will be highlighted in the revised manuscript as one of the objectives of the study and relevant literature will also be cited at **Line 45-46** as follows:

…drought management strategies (Tanguy et al., 2023b). River-flow projections in the UK are known to be sensitive to seasonal variations in precipitation and potential evapotranspiration, owing to their influence on the seasonal wetting and drying cycles of the soil (Parry et al., 2024). Chan et al. (2024) further highlighted that the likelihood of experiencing a summer month drier than the historically driest recorded month is expected to rise with future warming in certain regions of UK. Although these studies highlight the importance of seasonal controls on UK droughts, a comprehensive probabilistic analysis of drought return levels across characteristics and warming levels is still needed.

Comment 3: Can the author **justify why they have selected the 90th percentile threshold**? A suggestion to check with an 80th percentile threshold.

Reply 3: We selected the 90th percentile (Q90) threshold to ensure that the analysis captures instances characterised by extremely low historical flows. This choice allows us to focus on severe low-flow anomalies that are hydrologically meaningful, rather than relatively normal variations in streamflow. The Q90 threshold has also been widely used in previous hydrological drought assessments, providing both consistency and comparability with earlier studies (e.g., Hasan et al., 2020; Prudhomme et al., 2014; Janicka-Kubiak 2025). Furthermore, Q90 is sufficiently stringent to minimise the influence of short-term fluctuations, ensuring that the identified drought events represent genuine low-flow conditions rather than transient anomalies. An additional motivation for adopting the Q90 threshold is our emphasis on addressing uncertainties associated with estimating rare drought characteristics. Using a high-percentile threshold such as Q90 demonstrates that the methodology is robust for detecting extremely low-occurrence drought events, thereby supporting the reliability of our drought characterisation approach.

In the revised version of the manuscript, we will incorporate these clarifications in Section 2.4 (**Line 235**) to make our rationale for selecting the 90th percentile threshold clearer and more transparent. Further, as per your suggestion we will also check an additional percentile threshold as suggested to demonstrate the consistency in analysis and include the results.

Comment 4: The majority of studies have considered the drought identification (threshold) on the total observation period, so using a baseline period here is really questionable. Please **justify**. For future data, the threshold should also vary, so the entire period should be considered instead of just the baseline.

Reply 4: Thank you for the comment. A major aim of this study is to assess nonstationary changes in drought characteristics relative to a baseline period under climate change. Using a baseline threshold and calculating future statistics against it allows us to capture changes due to shifts in the mean, whereas calculating thresholds over the entire period would dampen these changes and risk underestimating the future state of drought. In this study, we identify drought events using a variable-threshold approach, where flow deficits are measured relative to the reference-period Q90 values which results in drought characteristics naturally varying in the future warming level periods. Thus, although many studies compute thresholds over the full period, here we think using a baseline is justified when the goal is to detect absolute changes in drought behaviour relative to historical conditions. The suitability of this approach has been demonstrated in several studies for UK and globally, for e.g., in Parry et al., 2024; Satoh et al., 2022 and Araujo et a., 2025.

Comment 5: Line 146 Repetition about the number of catchments may be deleted, as it has been clearly stated beforehand.

Reply 5: We thank the reviewer for noticing this. The repeated mention of the number of catchments will be removed in the revised version of manuscript.

Comment 6: Line 186: Please check if the **null hypothesis** is rejected for a P-value exceeding 0.05; is this the case? Please clarify the exact null hypothesis in accordance with the study and cite the relevant literature. **Present a table which is significant, which is not in the supplementary.**

Reply 6: We propose to correct Line 186 and add the following lines including a table in the revised manuscript for more clarification:

The null hypothesis in our study assumes that drought characteristics extremes are stationary, meaning their statistical properties do not change over time or with temperature. Using the likelihood ratio test, this hypothesis is evaluated by comparing the fit of stationary and nonstationary GEV models. The null hypothesis is rejected when the p-value falls below 0.05, indicating that including temperature as a covariate significantly improves the model. Such an approach is consistent with standard methods in extreme value analysis for hydrological data (Salas and Obeysekera, 2014; Das and Umamahesh, 2017).

Comment 7: Line 198, there is an **error in the terminology** used in the expression and in the description.

Reply 7: We thank the reviewer for pointing this out. The terminology in **Line 198** will be corrected in the revised manuscript as follows:

$$p(\Theta\,|y) \propto p(y|\Theta)\,p(\Theta) \tag{4}$$

Here, $p(\Theta\,|y)$ denotes the posterior distribution of the parameter vector $\Theta = (\mu, \sigma, \xi)$, $p(\Theta)$ represents the prior distribution, and $p(y|\Theta)$ denotes the likelihood function corresponding to the GEV distribution evaluated at $y_{i \ldots n}$ where n is the number of observations.

Comment 8: Line 232: What is the moving window size with respect to ± how many days?

Reply 8: As mentioned in **Line 233**, a 30-day moving window centred on each day of the year was applied.

Comment 9: Line 240: Cite literature

Reply 9: Line 238-241 discusses the pooling procedure applied in the analysis to ensure that closely spaced drought events are treated as a single continuous event, reflecting their cumulative hydrological impact. In the revised manuscript, we propose to cite Parry et al., 2024 and Van loon and Van Lanen 2012 which utilise similar method for the analysis.

Comment 10: Line 382-385: Also, the intensity is a function of both duration and severity, so that might be the reason behind the smaller variability?

Reply 10: Yes, this difference is smaller for drought intensity as intensity is a derived metric from duration and severity. This results in possible partial compensation of variability due to simultaneous increase/decrease in duration and severity as compared to the case when they are looked at individually.

Comment 11: Figure 2: Mention unit for duration, intensity and severity

Reply 11: Thank you for the suggestion. We will mention the units for duration, intensity and severity in the revised manuscript.

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