Review of *Non-stationary low-flow frequency analysis with mixed Weibull components and Copula-based dependence framework* (https://doi.org/10.5194/egusphere-2026-1820) submitted to HESS.

Reviewer: Chuck Kroll, SUNY ESF, Syracuse, NY USA

Scientific significance: Good
Scientific quality: Good
Presentation quality Good

I liked this manuscript, and recommend it be accepted for publication in HESS with minor revisions. It is well-written and generally clear. The manuscript provides an interesting framework to address both seasonality and trends in low flow frequency analysis. My primarily suggestions are to improve some of the clarity of the manuscript and to further discuss the context of the analysis.

Line 93. Do any of the ROBIN sites you are employing contain intermittent flows (zeros)? If so, how does this impact your analysis?

Lines 112-113. The sentence reads awkwardly.

Lines 136-138. What is the importance of this clustering to your analysis. Is it to group sites for interpreting results? Would interpreting histograms of annual minimum 7-day flows produce similar groupings?

Lines 152-155. These ranges of Kendall's tau seem arbitrary. Could you instead relate them to a p-values for the Mann-Kendall test?

Line 168. I believe the Sen slope assesses the trend in the median and not the mean.

Line 173. While it's somewhat semantics, it's not the probability of a low flow event, but a probability of having a low flow event equal or smaller (a non-exceedance probability).

Line 181. While theoretically annual minimum flow series should follow a Weibull distribution, sometimes it's been shown to provide a poor fit to these series. Might you be able to show (or at least discuss) this distributional choice and it's use at these catchments for these series? Could an L-moment diagram of these series to motivate its use?

Line 219. Could you give further evidence of whether model parameters are stationary or not? You could split the record and fit the WEI3 to both halves and observe whether there are common trends in the parameters. While it's beyond the scope of this manuscript to consider other non-stationary moments, it might be good to assess this assumption.

Line 224. What motivates this model? Why linear in real space? The location parameter is essentially a lower bound. Have you looked at whether the lower bounds are increasing (they seem to be in Fig 4a)? You might do a quantile regression to assess this.

Line 251. It would be helpful to either include the likelihood function or provide a reference to where this could be obtained. Are you assuming your observations are independent?

Line 260. Suggest replace “occurrence” with “non-exceedance”

Line 268. The rdT statistic is a measure of a relatively change in a return period at the quantile estimate defined by the non-stationary model. Is this really a performance gain? I don’t think you can really assess performance of these methods unless you have a controlled experiment (e.g., Monte Carlo) where the true statistic is known. If you fit the WEI3 to the first ½ and second ½ of the record, you’d also see some shifts in rdT, but it wouldn’t necessarily be a performance gain. Also related to this, the lower tail location and shape of the Weibull distribution will probably have a large impact on this statistic (indicating largest or smaller values of rdT). Ultimately, I think this is better presented as a distributional shift than a performance gain.

Line 300. What about precipitation patterns? It appears that southern Europe is experiencing decreasing precipitation (Zeder D., and Fischer, E.M., 2020, Observed extreme precipitation trends and scaling in Central Europe, Weather and Climate Extremes, 10.1016/j.wace.2020.100266).

Lines 308-310. This is a great motivation for the rest of the study. This might benefit from reporting a field significance test (regionally).

Line 314. What do you mean by succeeded? Converged? DId you assess distributional fit/significance? Something else? Please explain.

Line 320. State significance level of LR test (5%?).

Line 321. This seems to be evidence that a stationary model may be adequate except in unique situations (~10% of your sites). Sometimes sites classified as undisturbed are disturbed when analyzing low flows, which are more greatly impacted by small disturbances to the system. Could there be other drivers to these systems?

Line 326. I would remove the words “of course also”

Line 338. I would soften the statement “is both relevant and informative” as only 10% of your sites appear to fit this situation.

Line 356. Figure 4b shows return periods up to 1000 years. Does this make practical sense? In US, we rarely look at return periods greater than 10 years for design purposes (usually 7Q10 and 7Q2). Is a 50, 100 or 1000 year return period used in practice? Given the 3-parameter Weibull was used, the lower tail behavior will be strongly influenced by the location parameter (lower bound). A lot of your discussion is about the changes in “rarer events”. Is that practical?

Line 362. Is “strongly correlated” the correct term? Is this an increased seasonal dependence?

Line 363. Only 1 parameter is varying, not all parameters.

Line 395. Table 1 appears to represent the variation at different quartiles and not at the beginning and end of the observation period.

Line 445. What is a “considerable subset?” Suggest you provide the statistics here (~10%). Also, I would mention these are unregulated sites.

Line 447. Is the location parameter change a shift in the central tendency or the lower bound?

Aside: While this analysis is interesting, it is complex. Might you get a similar result by just fitting the WEI3 to the last 15-20 years of the record? Vogel and Kroll (2020, A comparison of estimators of the conditional mean under non-stationary conditions, Advances in Water Resources, 10.1016/j.advwatres.2020.103672) tried to address this issue looking at non-stationary conditional means.