This manuscript evaluates the applicability of the Dual Gamma Generalized Extreme Value (GGEV) distribution for flood frequency analysis using data from a large set of catchments in Poland. While the study addresses a relevant topic and applies a technically interesting distribution, major revisions are needed before publication—particularly regarding the inconsistent use of estimation methods (Bayesian vs. MLE), limited justification for the choice of GGEV, excessive detail in the redundancy analysis, and recurring issues with redundancy, figure quality, and sentence clarity, among others. Detailed comments are provided below.

Mayor comments

1) The current title, "The application of new distribution in determining extreme hydrologic events such as floods", is somewhat vague and potentially misleading, as it implies that the distribution was developed or derived as part of this study. However, the manuscript focuses on applying an existing distribution—the Dual Gamma Generalized Extreme Value Distribution (GGEV)—to flood frequency analysis.
   I suggest revising the title to name the distribution, improving clarity and precision explicitly. For example, "Application of the Dual Gamma Generalized Extreme Value Distribution to Extreme Hydrologic Events" would more accurately reflect the scope and contribution of the paper.
2) The manuscript adopts the GGEV distribution as a flexible alternative to traditional three-parameter models for flood frequency analysis under non-stationary conditions. However, the rationale for selecting this distribution and the hydrologic interpretation of its parameters—particularly the additional shape parameter (delta)—could be more clearly articulated.
   It would strengthen the manuscript if the authors provided a more detailed explanation of how the GGEV distribution improves the modeling of extreme events, for example, by showing how delta influences tail behavior or skewness in a hydrologically meaningful way. Comparative visualizations (e.g., PDFs or return level plots) could also help clarify the added value of this distribution in the context of practical flood risk applications.
3) The study employs Bayesian MCMC estimation for the GGEV parameters, while using maximum likelihood estimation (MLE) for the GEV, LN3, and P3 distributions. This inconsistency introduces potential biases in model comparison and undermines the objectivity of the goodness-of-fit evaluation.
   It is strongly recommended that the authors either apply a uniform estimation framework across all distributions (e.g., MLE or Bayesian) or explicitly justify using Bayesian inference for the GGEV distribution alone. If the Bayesian approach is retained for GGEV, it would be important to include appropriate posterior diagnostics (e.g., trace plots, convergence statistics) and sensitivity analyses regarding prior assumptions.
   Additionally, in the context of Bayesian modeling, it may be more appropriate to incorporate probabilistic performance metrics that reflect both accuracy and reliability —such as the Continuous Ranked Probability Score (CRPS) or its skill score version (CRPSS) (Hersbach, 2000; Ossandón et al., 2022)— rather than relying solely on point-estimate-based metrics like MAE or RMSE. This would provide a more robust basis for comparing predictive performance across models.
4) While the authors apply the Mann-Kendall test to identify monotonic trends in the data, the modeling framework does not appear to explicitly incorporate non-stationarity in the distribution parameters. This methodological gap could limit the study's relevance in the context of climate change, where the assumption of stationarity is increasingly untenable.
   If the authors choose not to implement non-stationary models (e.g., distributions with time-varying parameters such as mu(t) or sigma(t)), it would be important to include a discussion justifying this decision. Clarifying whether the approach is intentionally based on stratified modeling (e.g., trend-

based grouping) rather than a fully process-based framework would help readers understand the scope and limitations of the results, particularly in terms of their applicability to future climate-influenced extremes.

5) While the RDA section is methodologically comprehensive, it currently presents an excessive level of detail that overwhelms the reader and hinders the extraction of key insights. The narrative includes lengthy descriptions of biplots and correlation patterns for each distribution, much of which could be streamlined.
   I suggest briefly introducing how to interpret RDA results in the Methods section (3.5) and then making the Results section (4) more concise and targeted, focusing only on the most relevant and interpretable patterns. Analyzing each RDA result in detail within the main text is unnecessary. For example, the detailed plots and discussion related to the LN3 and P3 distributions could be moved to the Supplementary Material, especially since the primary focus of this study is the application of the GGEV distribution in flood frequency analysis.
   This would allow the main text to highlight the most critical findings while avoiding redundancy and improving readability.
6) The manuscript has recurring issues related to redundancy and incomplete or unclear sentence structure. In several instances, information is repeated across consecutive lines with minimal added value, affecting the text's overall conciseness and flow. Additionally, some sentences appear grammatically incomplete or are phrased in a way that makes their interpretation ambiguous. These issues may hinder reader comprehension and should be systematically addressed.
   For specific illustrations of these concerns, please refer to the minor comments. A thorough language and structural revision is recommended to enhance the manuscript's clarity and readability.
7) The overall quality and resolution of the figures should be improved to enhance their visual clarity and readability. For instance, in Figure 10, it is difficult to clearly discern the horizontal and vertical lines corresponding to zero on each axis. The axis tick labels, as well as any other textual elements within the figure, appear blurry and are challenging to read. This issue applies not only to Figure 10 but also affects several other figures throughout the manuscript. Enhancing the resolution and font clarity would significantly improve the interpretability of the graphical results and ensure they meet publication standards.

Specific comments:

1) L48: Lognormal is a two-parameter distribution, or do you mean LN3?. Please correct it if it corresponds.
2) L82: A linear trend means nonstationarity of the time series. Please check it.
3) L98: Please correct the citation. It should be (Silva and Do Nascimento, 2022).
4) L124-125: Please consider improving the phrasing in those lines for clarity.
5) L144: You already mentioned it in L140. Please avoid repetition.
6) L226: Canoco 5.12 is an R package? Please specify.
7) Figure 3. It should say "value of the shape." In the same figure, it should be "generating random samples." Please correct them.
8) L258: Include the reference for Brunner et al. (2018) in the reference list. Please check it for the rest of the references.
9) L287: Generalized is redundant in "the generalized GGEV". Please remove it.
10) L291: There is an extra comma; please remove it.
11) Figure 6. In the legend, it should be "P3" instead of "3P3"
12) Lines 324–325 appear to repeat information already stated in the preceding paragraph. Consider revising this section to avoid redundancy and improve the flow of the text.

13) L336-338: Please revise the phrasing in the sentence. The sentence appears incomplete or abruptly truncated, and the comparison between the distributions could be clarified for better readability.
14) Lines 338–340 appear to repeat information already stated in the preceding lines. Consider revising this section to avoid redundancy and improve the flow of the text.
15) L355-356: it is stated that the widest range of catchment area (A) is represented by samples fitted to the GGEV distribution. However, based on Figure 8, the GEV distribution spans the widest range of A (~50 - ~2,500 km^2). Please revise the description to reflect this more accurately.
16) L359: the same as 15). Redundant.
17) L370: Define the term "add. Shape."
18) L423: Add an space before (Fig9. c)
19) L456-457: The sentence "It exhibits a strong correlation with the scale and location parameters, whereas this relationship is not observed for the shape parameter (Tabari et al., 2021b)" is unclear. It is unclear what "It" refers to or which variable is being described as correlated. Please clarify the sentence and ensure that the subject and its relationship to the distribution parameters are explicitly stated.
20) Figures 9 and 10: It looks like for GEV, the shape parameter is less sensitive to the sample size (N) that the shape and add. shape for GGEV. If this is the case, it should be highlighted.
21) L531: repetition. You already said it in L532.
22) L627: In the sentence "the fitted LN2 distribution has a shape parameter value greater than 1 for only seven stations", there appears to be a conceptual error. As far as I understand, the two-parameter log-normal distribution (LN2) includes only a location (mu) and a scale (sigma) parameter and does not have an explicit shape parameter. Please clarify whether this refers to sigma being interpreted as a shape proxy or if there has been confusion with another distribution.

References

Hersbach, H. (2000). Decomposition of the continuous ranked probability score for ensemble prediction systems. *Weather and Forecasting*, *15*(5), 559–570. https://doi.org/10.1175/1520-0434(2000)015<0559:DOTCRP>2.0.CO;2

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