Revision of the manuscript egusphere-2025-6282

REVIEWER #2

We thank the Reviewer for the positive comments and constructive criticism on the submitted manuscript. Following the concerns expressed by this and Reviewer #1, we revised the manuscript to provide the requested clarifications and changes.
Minor amendments to the text have been added as well, according to the suggestions of both Reviewers. All the changes are highlighted in the submitted track-changes version of the manuscript. Hereafter, we reply to the suggestions and recommendations expressed by the Reviewer #2. For the sake of clarity, the comments are reported in **bold**, and our replies are reported in *italics*. Moreover, the added or changed text is reported in normal font after the reply.

We believe that the manuscript has substantially improved, and we look forward to receiving this Reviewer's feedback.

Best regards,

Giulio Calvani and Paolo Perona

The definition of a "weakly non-stationary" process appears somewhat arbitrary, as it depends on the subjectively defined parameter a, which ultimately controls all results. There is no objective or physically grounded criterion provided for selecting a. A process is, in principle, either stationary or non-stationary. Introducing an intermediate category based on a threshold for the rate of change of the return period requires stronger justification. The authors should clarify how should this parameter be selected in practice. Should it relate to acceptable design risk?

We agree that a process with time-changing statistics is definitely non-stationarity, but we also argue that, even for stationary processes, there are some distinctions, i.e., they can be K^{th}-order stationary when not all the pdf moments are constant. This leads to the definition of weakly stationary process (Katz, 2013). From a different point of view, such processes can be defined $(K+1)^{th}$ non-stationary. In a similar fashion, we termed "weakly non-stationary" those processes exhibiting slight changes in the process parameters, as measured by the parameter a. The definition may be quite arbitrary if the parameter a is assumed a priori. However, as highlighted in the workflow of the proposed framework (added following the suggestion of Reviewer #1), the parameter a is determined at the end of the analysis from the calculated or forecast trends in the process variables and the analyzed timeframe t. Indeed, trends and timeframe of analysis are somehow imposed by (maybe of limited duration, see figure 1) recognized alterations of the system's statistical behaviour (see later comments) and their relevance with respect to engineering application or other type of practical considerations involving design risk, i.e., as this Reviewer has correctly pointed out. For such systems, the value of a satisfying the conditions on the change of return period determines the definition of weak non-stationarity.

In addition, the relationship between the proposed "weakly non-stationary" framework and established time-varying GEV models (e.g., GEV with covariates, or other trend-based nonstationary EVA methods) should be clarified. Since these models already allow the parameters of the GEV distribution to vary in time, it would be helpful to explain how the proposed framework relates to such approaches. Is the primary contribution a diagnostic criterion for admissible trend magnitudes, or does it offer advantages in estimation or design that cannot be achieved with existing nonstationary EVA methods?

As stated in the text, the proposed framework aims at estimating neither the GEV parameters, their change in time, nor the detection of potential trends/shifts in the system dynamics. These are assumed to be given or somehow known. It is rather a tool to calculate the maximum allowed trend values in the process parameters, to compare them to the influencing trends in the system dynamics, to retrieve the maximum value of the parameter a and to calculate the approximation error in the calculation of the return period for such process using a simple formulation (the one for stationary process) in comparison to the more complicated and computational demanding relationship for non-stationary processes (Salas and Obeysekera, 2014). According to the comment, the framework may be defined as a diagnostic criterion for admissible trend magnitudes.

In the discussion of trends and non-stationarity, an important nuance seems to be missing. The presence of a statistically significant trend in hydroclimatic data does not automatically imply non-stationarity of the underlying process for at least two reasons: (a) trends may emerge as manifestations of natural long-term variability or persistence, without implying a structural change in the generating mechanism, (b) stationarity and non-stationarity are modeling assumptions about the system's dynamics, not purely empirical properties to be inferred from finite data samples. As argued in the literature (Montanari and Koutsoyiannis, 2014; Koutsoyiannis and Montanari, 2015), invoking non-stationarity presupposes strong physical evidence of systematic alteration of the system's dynamics (e.g., land-use change, regulation, urbanization). Therefore, the authors should clarify whether their framework is intended for cases with physically justified structural change or for any statistically detected trend. This distinction is crucial.

We thank the Reviewer for pointing out this important distinction with which we mostly agree! In the Introduction, we have better explained that a phenomenon of local non-stationarity, yet of practical relevance may arise from different sources, including natural variability of climate conditions or simply stochastic periods of accumulation or decharge, to which the Reviewer refers to as "natural long-term variability or persistence", and anthropogenic activities (e.g., land-use change), which the Reviewer refers to as "alteration of the system's

dynamics". According to the comment, we modified the sentence to include the broader definition stated by the Reviewer, with the suggested references, and prepared a new Figure to combine with Figure 1. Additionally, we added some lines in the Introduction to mention the potential temporary behaviour of the non-stationarity, as well as the long-term structural stationarity of the system. Based on that, we therefore changed Figure 1 in order to highlight these different behaviours.
The changed text now reads (lines 29-33):

The alterations in hydrological statistics are typically attributed to several factors, including natural variability of climate conditions, and systematic changes of the system's dynamics due to, for instance, anthropogenic activities (e.g., urbanization, land-use change, greenhouse gas emissions) which may impact the precipitation patterns, air temperature, and sea levels (Montanari and Koutsoyiannis, 2014; Koutsoyiannis and Montanari, 2015; Lee et al., 2023).

In non-stationary extreme value analysis, the main challenge is estimation uncertainty. It could be argued that the computational burden emphasized by the authors is becoming less critical with modern computational resources. In contrast, uncertainty in parameter estimation, which is already substantial in stationary extreme value analysis, becomes amplified under non-stationarity due to additional trend parameters and temporal extrapolation. How does the proposed framework account for uncertainty in estimated time-varying parameters? If trend coefficients have confidence intervals, does the "weakly non-stationary" condition become probabilistic as well? At minimum, a discussion of uncertainty propagation and its interaction with the parameter a should be included.

We thank the Reviewer for pointing out this important aspect. Definitely yes. Uncertainty in the process parameters, their potential trends, and estimation may affect the outcomes of the framework. Specifically, considering the case of the linear trend (Eq.s (22)-(23)), one may find that some of the process trends (forecasted by statistical analysis, or climate scenarios) satisfy the limit conditions, and some do not. Additionally, for the same process trend, the uncertainty may change, for instance, the maximum timeframe satisfying the limit conditions. This is the typical case when the process parameters and trends are considered in a probabilistic way, and a pdf is therefore accounted for at each timeframe. Therefore, for each time frame, there exists a finite probability, P, that the trend satisfies the limit condition, and the complementary probability, $1 - P$, that the trend does not. Based on the Reviewer's comment, we added a figure (Figure 6) and explanatory text in the Discussion to highlight and discuss the deterministic and probabilistic approaches.
The added text reads (lines 279-289):

From a practical point of view, the limit solutions for the values of the GEV parameters (Eq.s (14) and (15) for the weibull-Fréchet type, and Eq.s (19) and (20) for the Gumbel distribution) have to be compared to forecasted trends from data analysis or modelled scenarios. As an example, figure 6 shows this comparison for the parameter λ and a sample modelled scenario of trend with decreasing return period. Due to the uncertainty in the estimation, at each future timeframe, t, the value of the forecasted parameter, $\lambda_F(t)$, is provided with a probability distribution function (light blue curves in figure 6) and the mean trend is represented by the continuous blue line. The shaded area of the pdf below the red curve (i.e., the limit solution) quantifies the probability that the forecasted trend satisfies the weakly non-stationary conditions in the proposed framework. In this regard, the deterministic timeframe limit of validity is defined by the point at which the forecasted trend (blue line) intersects the limit solution (red curve), i.e., $t \simeq 13y$ in the shown example (figure 6). Conversely, from a probabilistic point of view, even when the average trend is above the limit solution (e.g., at $t = 15$y in figure 6), a finite probability, although small, exists for a valid weakly non-stationary approach to the return period analysis.

The framework assumes local stationarity within $\Delta\tau$, implying that the time-varying distribution parameters are well defined and estimable from the observed record. This assumption deserves further clarification, as classical ergodicity presupposes stationarity. The relationship between stationarity and ergodicity within the proposed framework should therefore be explicitly discussed. Under what conditions can parameters of a time-varying distribution be consistently estimated from a single realization? Since ergodicity affects both return period interpretation and parameter estimation from observed data, it would be beneficial to introduce and clarify this conceptual link earlier in the manuscript.

The framework aims at forecasting the return period of non-stationary processes, when the trends are within *specific values, to satisfy the weak condition. The framework is formulated for future timeframes, and the trends are considered in such future timeframes. There are single realizations in the future, as there are no realizations at all. Rather, trends and changes in process parameters are forecasted from available data (in the past) or simulated scenarios. Indeed, as all the relationships for the limit conditions on the parameter trends are given as a function of* $\overline{T}_0(x)$*, the process is considered stationary up to time* $t = 0$.

The paper is highly theoretical. While the mathematical derivations are clear, it may not be obvious to the average HESS reader what the practical contribution is. The authors should more clearly articulate: What specific research question this framework answers? In which typical hydrological scenario it would be preferable over existing methods and how it would be implemented in practice? Could the authors provide empirical examples where the temporal evolution of the distribution parameters is sufficiently gradual, relative to typical design horizons (e.g., 20–100 years), to justify the weak non-stationarity assumption? At present, the practical usefulness is not sufficiently highlighted.

Based on this comment, other suggestions, and the Reviewer #2's comment, we added a simple description of a possible workflow of the proposed framework for practical purposes. The added figure (figure 6) also explains the comparison between the proposed limit solution and a hypothetically forecasted trend in one of the governing parameters of the Gumbel distribution (see also the reply to a previous comment).
The added text on the application of the proposed framework reads (lines 320-322):

As a result, once the GEV parameters are known from fitting models or forecasted scenarios, the maximum timeframe for the validity of the weakly non-stationarity analysis can be retrieved in the case of decreasing return period for specific values of the parameter a, which in turn drives the estimation error (e.g., Figure 7).

In Figures 2a, 2c, and 3a, the magenta solid line corresponding to the larger time horizon is not shown. Although the text mentions a limitation related to the asymptotic condition of Eq. (11), this is not immediately evident from the figures. I suggest clarifying this point in the caption or adding a brief explanation to improve readability.

Thanks for highlighting the unclearness in the figures. Indeed, the violet line is not shown as the corresponding timeframe (i.e., t=30 years) is greater than the limit condition. Although the explanation is given in text, and according to the suggestion, a brief description of the reason has been added to the caption of all the figures affected by this behaviour.
The added sentence in the caption of Figure 2 (similarly in Figure 3) reads:

In panels a and c, the continuous violet line does not satisfy the condition $t \leq \frac{\overline{T}_0(x)-\Delta\tau}{a}$, and therefore it is not shown.

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