

Review Response HESS-egusphere-2025-3073

We would like to thank both the reviewer and the editor for their constructive comments. We paste the comments here, and give a summary reply below.

Reviewer comments

Major Comments

To my regret, the authors have preferred to estimate yearly return levels from annual maxima, rather than derive them from the seasonal distributions, despite agreeing with my considerations on “purely theoretical grounds”. They argue that it is uncertain how well the derived return levels perform (presumably in terms of closeness to the empirical estimators), use the relative influence of different weather patterns to infer that the seasonal maxima come from the same distribution, invoke error propagation and high computational costs, and present trials in which the difference in return levels (stationary) and trends (non-stationary) between the two methods are essentially not very large.

I do not agree with the authors’ presentation of the numerical derivation of the yearly return levels as an “alternative method”. On the contrary, it is the theoretically robust method, and in their particular case, perhaps because they opted to work with a relatively low return period of 20 years, the results happen to be similar.

Unless the authors can show that the hypothesis that the seasonal maxima of different seasons come from the same distribution cannot be rejected (which seems unlikely given the difference in dominant precipitation processes between winter and summer), the yearly maxima cannot be assumed to follow a GEV. The readers should explicitly be made aware of this. In my view, the methods section should present the theoretically robust method, and the author’s reason to depart from it, given that they in fact have the means to derive these yearly return levels from the seasonal distributions. Possible consequences for the interpretation of the results should be discussed in the discussion section.

To accept this paper for publication, I insist on the following:

- In section 3.2.1

- explain that if the monthly (seasonal) maxima are not from the same distribution, the seasonal (yearly) maxima cannot be assumed to follow a GEV distribution. This should be mentioned explicitly. It must be clear to the readers that the

authors must justify their assumption that the seasonal (yearly) maxima follow a GEV.

- explain that if the seasonal (yearly) maxima do not follow a GEV, the seasonal (yearly) return levels should be derived numerically from the monthly (seasonal) distributions rather than fitting a GEV to the seasonal (yearly) maxima.
- Indicate that this method was not pursued, and the authors' reasons for this choice. Indicate that the possible consequences for the interpretation of the results will be examined in the discussion.

- In section 5.1.3 the following points, at least, need to be explained and/or discussed.

- Deriving yearly return levels from the seasonal distributions is theoretically correct and is the more robust approach.
- Not deriving the return levels for the stationary analysis is probably of minor importance, as the uncertainties are large. On the other hand, it can be critical for the long-term trends. Indeed, not only do these rely on the exact value of the return level without taking into account its uncertainty, but the position of the return value on the timeline is relevant. In addition, the significance of the trends can be affected.
- According to Extreme Value Theory, the maxima must be taken over a large enough number of iid values to justify the use of the GEV approximation. Therefore, confidence may be greater in assuming that the seasonal maxima (based on approximately 90 values) follow a GEV than the monthly maxima (based on approximately 30 values). This supports the authors' choice to estimate seasonal return levels from seasonal maxima, rather than deriving them from monthly GEV distributions.
- While the month-to-month distribution of weather types does not negate the assumption that precipitation measurements in months of the same season, characterized by similar physical processes, follow the same distribution, the same cannot convincingly be argued for different seasons in which precipitation processes are known to be different. Thus, it is doubtful that, for instance, winter and summer hourly or daily precipitation are iid.
- The results are similar when estimating yearly return levels from yearly maxima and when deriving them from the seasonal distributions for a return period of 20 years. The readers should be made aware that this similarity may no longer hold for higher return periods or a change in analysis period.

Minor Comments

A few of the changes to the manuscript seem to have been hastily added, as they disrupt the logical thread of the text, or deprive it from its support in the literature (see lines 340 and 422).

Editor comment

I have now read the revised manuscript again, together with the reviewer's report. On the one hand, I believe that the analysis you conducted and the results reported are scientifically robust. On the other hand, I also see the reviewer's point that, given seasonal changes in large-scale circulation, there is a rationale for analyzing at a seasonal (or monthly) rather than annual scale. That said, based on the reviewer's report, I believe that their suggestion to comment explicitly on this issue and to explain to readers the implications of deriving yearly return levels from annual maxima rather than from seasonal distributions should be addressed more clearly in the manuscript. This should require only relatively minor textual amendments to the methods and discussion sections, along the lines suggested by the reviewer. I look forward to reading the revised manuscript and seeing how these points have been addressed.

Author's reply

We very much appreciate the time and effort invested by the reviewer, which in times of increasing numbers of GenAI-written reports really is worth highlighting. Unfortunately we still do not agree with the reviewer on their concerns. As we have laid out in our previous replies, the two methods should not differ on theoretical grounds, but will give different results when applied to real observations which are far from perfect due to, e.g. measurement errors, sampling bias, changes in instrumentation etc. Further, given the limitations of the data, notably the length of the time series, we also believe that a return period of 20 years is, in fact, relatively high, especially for trend analysis.

While we do agree that the fact that annual maxima can come from different seasons (and thus drivers) could be a problem (Carter and Challenor, 1981), we believe that this is actually not relevant in our case: The patterns for daily return levels, both stationary and non-stationary, as well as for the dominant weather patterns are very similar between annual and autumn analysis, which indicates that the sample of maxima is very similar between these two. Thus, if the analysis for autumn is robust (and the reviewer does not seem to doubt that), the annual analysis is robust as well. The same is true for hourly intensities, where the annual patterns resemble a lot those for the spring & summer seasons.

Although not widely discussed, the reviewer's concerns have been addressed in the literature. One of the suggested solutions is to make the GEV parameters month-dependent (with sine or spline functions, e.g. Maraun et al., 2009, Fischer et al., 2016). However, this requires a smooth seasonality of the GEV parameters, which we do not find in our study region. Also, these models remain stationary in the sense that no evolution with the years is taken into account (only a seasonal cycle). Only one study that we are aware of included a possible long-term evolution (Raillard et al., 2026),

however, they analysed sea ice states in climate models, which unlike our dataset are not expected to contain extreme singular events (which could either be real or due to measurement errors in our case). For the stationary case, Maraun et al. (2009) compared three approaches (direct fit on annual maxima, derived from individual monthly fits, and derived from linked monthly fits) and did not find that any of these approaches performs significantly better or worse than the others. This conclusion is similar to what we found in the previous review round. However, we have also found that the problem of error propagation from poor monthly fits is much more important when we allow for a long-term trend. We are not aware of any study that, similar to Maraun et al. (2009) has compared the different approaches for such a case. Also, many published articles actually base their analysis on annual maxima, including highly-cited papers such as Papalexiou and Koutsoyiannis (2013), Ribes et al. (2019) and also Ben Alaya et al. (2020), Jayaweera et al. (2023).

Finally, our main interest and thus most of our comments in the manuscript relate to the monthly and seasonal scales, and we show annual return levels mainly for the purpose of comparison. In that sense we argue for the importance of a detailed analysis beyond annual maxima, and the theoretical concerns raised by the reviewer are one more argument in the same direction.

We have amended Sections 3.1.1 (Methods), 5.1.3 (Discussion) and the Conclusion accordingly.

References

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