Response to comment 2

We would like to thank the reviewer for their time spent on our work and positive review. We will respond to their comments directly below, with our responses in blue.

This manuscript investigates the temperature dependence of extreme short-duration precipitation using the TENAX framework, with a particular focus on the behaviour of the Weibull shape parameter. Overall, I find the study to be clearly structured, theoretically well motivated, and addressing a topic that is both important and timely. Although the results do not yet provide a fully conclusive answer regarding how the shape parameter should be treated in practice, the analysis offers a very useful and careful reference for further work in this direction.

I only have a few points where I think some clarification or further discussion would strengthen the paper.

1. First, the results seem to strongly suggest that the estimated values of b are influenced by different precipitation mechanisms, as implied by the contrast between Germany (mostly insignificant and seemingly stochastic) and Japan or the UK (more significant values with clear spatial structure). This appears physically reasonable, since the impact of temperature on short-duration rainfall is expected to differ between convective, orographic, or mixed precipitation regimes. However, I am not entirely convinced that a single parameter b can adequately represent such diverse rainfall mechanisms. Some clarification on how the authors interpret b in relation to different precipitation types, or a discussion of the limitations of using a single parameter for this purpose, would be helpful.

We agree, and there is ongoing work trying to specifically split TENAX-like models by mechanism and precipitation regime. We will add some discussion of the limitations and potential interpretations of *b* to the discussion section: "*The apparent dependence of b on location and orography implies that it is accounting in some way for the dynamical changes associated with temperature changes. For example, in some locations, climate change is associated with more intense circulation patterns in convective precipitation and shifts from stratiform to convective regimes, and super Clausius–Clapeyron scaling is observed (Da Silva and Haerter, 2025). Hence,* using only *the b parameter to represent a vast array of precipitation mechanisms in some locations may be an oversimplification.*"

2. The title refers explicitly to "precipitation tail heaviness", while the main focus of the analysis and results is on the significance, uncertainty, and practical implications of estimating the parameter b. Although the influence of b on tail behaviour can be inferred indirectly through the Weibull shape parameter, the manuscript does not directly

quantify or visualise changes in tail heaviness itself. The authors may wish to consider whether a clearer alignment between the title and the analytical focus is needed, or alternatively, whether some additional discussion or diagnostics could more explicitly link the estimated parameters to observable tail behaviour.

We will add further description of the implications of b on tail behaviour in the introduction and discussion, including the example figure (see below).

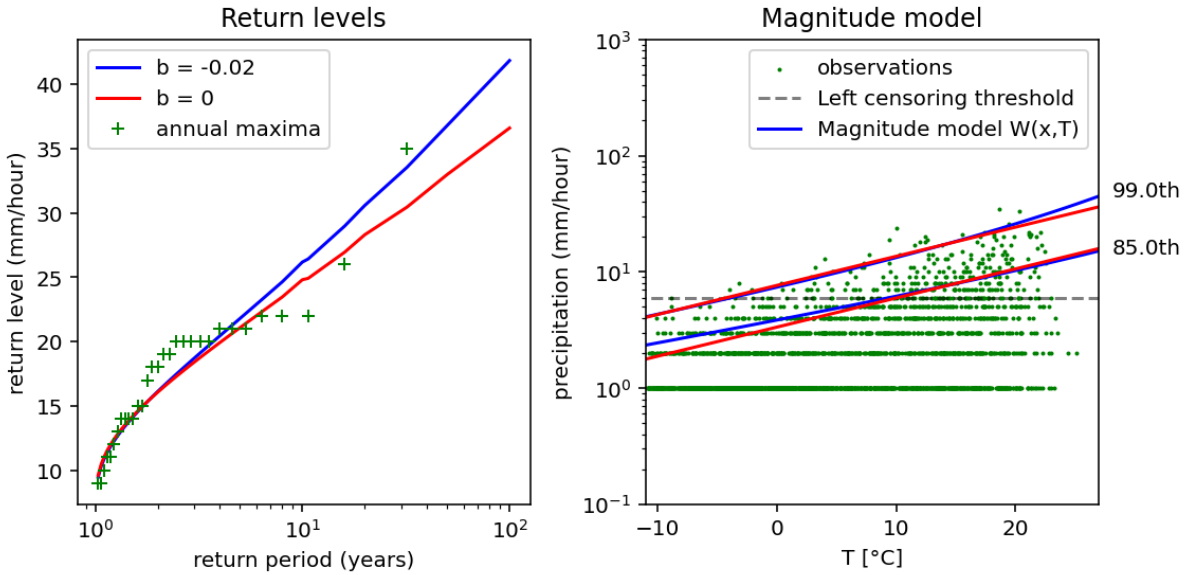

Figure R1: Example to show the impact the shape parameter has on tail heaviness and the resulting return levels from a station in north Japan (45.25, 141.85). Panel (a) shows the return levels, with shape parameter dependence on temperature in blue. No dependence on temperature in the shape parameter is in red. Panel (b) shows the magnitude model, the relationship between temperature and extreme precipitation.

3. The four analysed countries show quite different behaviour in terms of the significance, spatial structure, and uncertainty of b, yet these differences are not discussed in much depth. Given that these regions form a central part of the study, I believe the manuscript would benefit from at least a qualitative discussion of why such contrasts arise, for example in terms of climatic setting, dominant rainfall processes, or data characteristics. Even a speculative interpretation would help readers better understand the broader implications of the results.

Thank you for the recommendation. While we agree with your suggestion, it is difficult for us to give interpretations that are not merely speculative. As mentioned in the text: the pattern "… seems to be linked to a range of factors, including latitude, elevation, climatic conditions, and prevalent synoptic forcing". Disentangling such factors would require specific analyses which fall beyond the scope of our technical note. Indeed, this is something we plan to explore as soon as possible.

Overall, I like this study and these points are meant as suggestions for clarification, and I believe addressing them would further improve the impact and interpretability of this study.