EGUSPHERE-2024-2594
Detailed Responses to Reviewer 1's Comments

Anne Martin, Élyse Fournier and Jonathan Jalbert

February 16, 2025

First, we would like to thank the three reviewers for their thorough review and relevant comments and suggestions. Based on their feedback and recommendations, we notably adjusted the structure of the paper and added numerous clarifications. The modifications in the revised version of the manuscript are highlighted in blue. Below are detailed responses to all of Reviewer 1's comments.

I agree with the authors when they state that "translating the definition of PMP into a statistical model is interesting" (line 350). They could also say "estimating PMP is a really hard problem". To the authors' credit, they begin with the commonly accepted definition of PMP as an upper bound, and then construct a statistical model which fits this definition. Fitting a model with a finite upper bound is challenging because precipitation data usually suggests that the distribution is unbounded, and further has a heavy tail. It is probably not surprising that the authors ultimately find their approach to be unsuitable for implementing in practice, and conclude that the best statistical approach is to eschew the upper bound requirement and instead implement extreme value (EV) methods.

Thank you. This is a very good description of the proposed contribution.

Major concerns

1. Unfortunately, I think the manuscript's structure does not tell its story well. Primarily I view the paper as an interesting way to discuss the challenges of PMP estimation, and talking about their particular model is one part of this larger story. It strikes me that the take away message does not appear in the abstract or in the body until Section 6. I think it would be better to move these messages up front. The story I imagine is something like this:

   (i) Statistically estimating PMP is hard because its definition assumes a bounded tail, but precipitation data suggests the tail is unbounded. Because statistical estimation is hard, other methods like moisture maximization and Herschfield's scaling get used. Uncertainty and climate change are hard to incorporate into these non-statistical methods and frequently-used moisture maximization approaches involve several subjective judgements.

(ii) Starting with the ideas which underlie moisture maximization, we develop a sensible statistical model which assumes an upper bound.

(iii) We perform simulation studies and use the method to fit PMP at two locations in Quebec, but find that estimates for the upper bound have unsuitable uncertainty.

(iv) We conclude with a discussion and offer our suggestion for best practices.

I think all the pieces of this story are in the paper, but I do not think the current focus of the paper gets the essential message across very well.

This is true, and we fully agree with your suggestion. In the revised version of the manuscript, the sections have been rearranged as you suggested to more effectively convey the conclusions.

2. I find the notation in the paper to be inconsistent. In Equation (1), Y_i denotes precipitation of storm i, but in Equation (2) I believe Y_i has been replaced by P_i. Equation (4) supposedly comes from Eq. (1), but has quantities EP_i and EP_{max}, which are presumably PW_i and PW_{max} in Equation (1)?

   This was a mistake; thank you for pointing it out. In the revised manuscript, we consistently used "PW" for precipitable water. "EP" was the equivalent acronym used in the French version.

3. The ratio EP_i/EP_{max} is known/assumed to be less than 1, correct? If so, please say this explicitly.

   Yes, it is assumed to be less than or equal to 1. This is now explicitly mentioned in the revised version (L174).

4. I believe Equation (6) is used as the basis for the statistical model: $Y_i = EP_i/EP_{max} \times r_i \times PMP$. If I am following correctly, Y_i is random and observed. I think EP_i and r_i which underlie Y_i are random, but unobserved. EP_{max} is a parameter but not known, and PMP is the parameter we wish to estimate. So in the end, the authors propose a model for the observed precipitation Y_i, but use moisture maximization logic to include PMP as a parameter. They choose a beta/Pearson 1 as their distribution to fit. A cynical comment could be "the authors use a data-independent argument to conclude the data arise from a distribution, but which fits the data poorly". I think the story to be told here is that if one begins with a supposition of an upper tail, and one tries to then fit a model based on that assumption, things are really hard.

   Exactly. We indeed "propose a model for the observed precipitation Y_i, but use moisture maximization logic to include PMP as a parameter". Again, we agree with your comment that if one assumes an upper bound, things become quite challenging. We wanted to show that the issue might not lie with the Pearson Type I model itself but with the hypothesis of PMP existing as the upper bound. Elements of this discussion have been added to the revised version (L117 and L368).

5. If I understand correctly, the authors propose a beta/Pearson 1 distribution and fit **all** of the nonzero rainfall data to it. There is talk of thresholding on page 8, but it seems to be more tied to the discrete nature of the measurements rather than to thresholding for focusing on extremes.

   Exactly. The measurement precision is 0.1 mm with the lowest non-zero value of 0.2 mm. With the thresholding, we wanted to assess whether this discretization, which has a greater impact on small precipitation amounts, would affect the overall fit of the model. It turns out that the discretization does not have a noticeable effect on the overall fit (L230).

6. An EV approach would pick a high threshold or take block maxima and fit an EV model, presumably a reverse-Weibull guaranteeing an upper bound. Would such a method be better suited for estimating an upper bound than fitting a beta to the entire distribution?

   Thank for the suggestion. As mentioned in a previous comment, the goal was to propose a statistical model for the PMP based on the moisture maximization logic. While it is true that imposing a negative shape parameter on an extreme value distribution will result in an upper bound, this choice is difficult to justify beyond the fact that it produces an upper bound. Moreover, we are concerned that using an extreme value distribution in an inappropriate context, such as by imposing a negative shape parameter, could give practitioners a false sense of security. They might believe they are operating within the extreme value framework when they are not. Elements of this discussion have been added to the revised version of the manuscript (L397).

7. The authors show QQ plots for the EV models in Figure 6. QQ plots for the beta fit are noticeably absent.

   QQ plots of the Pearson Type I fit are provided in Figure 7(b) of the revised manuscript. Thank you for the suggestion.

8. l173. Why is β known to be greater than 1?

   Typically, precipitation has a monotonic decreasing density. This behavior is achieved with the beta distribution when $\alpha < 1$ and $\beta > 1$. This clarification has been included in the revised version (L182).

9. l304: Figure??

   Thank you for pointing that out.

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Detailed Responses to Reviewer 2's Comments

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The paper under consideration presents a statistical approach using the Pearson Type I distribution to estimate the upper bound of historical rainfall, incorporating uncertainty bounds. The authors aim to quantify uncertainty and address subjectivity inherent in the various stages of the World Meteorological Organization (WMO)-recommended Probable Maximum Precipitation (PMP) estimation methods. However, the WMO-recommended moisture maximization method focuses on maximizing highly efficient storms based on the physical mechanisms of those storms. From a statistical perspective, precipitation's tendency to exhibit a heavy-tailed distribution poses challenges in defining an upper bound, and this study similarly encounters this issue.

Major concerns

1. Previous studies demonstrated that precipitation naturally often exhibits a heavy-tailed distribution (shape parameter greater than 0) that brings rare storms over global scale, the proposed method struggled to estimate upper bound of those places by majority of parameter estimation methods. In this study, the method of moments partially quantifies the range with limited data, but the range is unrealistically large, for example, the range estimated for St-Hubert station varies between 165 to 9006.

   It is also the case here that precipitation is heavy-tailed and unbounded, as shown through the classical extreme value analysis in Section 6.2. In this paper, we proposed a statistical model for the PMP with a finite upper bound based on the moisture maximization logic. However, since this model does not fit the data well and is highly sensitive to it, as shown by the non-parametric bootstrap confidence intervals, it suggests that the PMP definition based on the moisture maximization logic may not be appropriate. We therefore recommend using extreme value theory instead.

2. A simulation study is conducted with distribution assumed convex density and found more than 40,000 sample size (in arid/semi-arid region that equivalent to more than 1000 years wet days) is required to stabilize the estimate. Given this, it is surprising that the authors did not attempt to expand the sample size for the two stations by incorporating numerical model ensemble precipitation products. Doing so could have supported their findings.

   This is a good suggestion to augment the actual precipitation data with simulated precipitation from a climate model or a weather generator. It is indeed something we could consider if we were to use this proposed model to estimate the PMP, although this is not our final recommendation in this paper. However, such data augmentation should be carefully implemented to avoid overconfidence. For instance, if 40,000 daily precipitation data points generated from a weather generator are used to estimate the model, do these 40,000 data points contain 400 times more information than a recorded series of size 100? At this point, this is beyond the scope of the present paper, but it could be an interesting avenue for future investigation. Elements of this discussion has been added in the Discussion of the revised version (L350).

3. This study compares their estimated upper limit with moisture maximization based PMP value. The PMP values using moisture maximization were found to be 282 mm for Montréal and 436 mm for St-Hubert, whereas the observed 24-hour maximum precipitation for these stations was 81.9 mm and 106.5 mm, respectively. Thus, the maximization ratio will be 3.44 and 4.09. The reason behind the exceptionally high maximization ratio may be due to the selection of storms and/or estimation of storm associated precipitable water. This study includes low magnitude storms (0.9 quantile might give more than 500 samples but previous studies mostly consider the highest 50 or less storms) and did not separate those storms that could lead to higher maximization ratio. Previous studies mostly limit the maximization ratio 2.0 (that only for orographic storms). Imposing a similar limit could provide some physically possible value around 200mm that aligns with 10,000-year return level (POT based) and PMP value would not much different within 26 km distance. Since the moisture maximization method provides an unrealistically high PMP value, comparing with this value to validate the method is questionable. It is recommended to use multiple study sites and consider those sites where maximization ratio lies below 2.0 and compare within those sites.

   PMP estimates using the moisture maximization method in Section 2.2 are based on the top 10% of storms, as suggested by Clavet-Gaumont *et al.* (2017). The reference has been added in the revised version. A sensitivity analysis was performed, and regardless of the quantile used for storm selection (10%, 1%, or 0.1%), the PMP result remained unchanged. This is because the event of September 20, 1989, with 63.8 mm at the Montreal station, is maximized regardless of the storm threshold. The same applies to the St-Hubert station, where the precipitation of 73.4 mm on July 5, 1958, is maximized. The maximization ratios are 4.4 and 4.9, respectively. Since precipitable water was not directly observed, it was estimated using the dew point, which may indeed affect the quality of the PMP estimates (L143).

The approach you suggested to improve PMP estimates using the usual moisture maximization method would be appropriate if that were the goal of the paper. However, we applied the simple moisture maximization method with the usual formulas at these stations primarily to highlight, to some extent, the flaws of the methodology and the need for great care in obtaining sensible PMP estimates. While the approach you recommend, choosing sites where the maximization ratio is below 2.0, may work for those specific sites, it would not be directly replicable to other locations. Therefore, although the elements you propose to improve the PMP estimates have been added to the manuscript, we believe this is beyond the scope of the present paper, as our focus is on the developed model for statistical PMP estimation.

4. National Academies of Sciences, Engineering, and Medicine (2024) recommends for risk-informed extreme value analysis methods that account for low exceedance probabilities and provide robust uncertainty and nonstationary quantification. It remains unclear how the proposed method offers advantages over or resolves issues better than these recommended approaches.

   In this paper, we provide additional support for using extreme value theory, as recommended by the National Academies of Sciences, Engineering, and Medicine (2024). The newly proposed statistical approach for PMP estimation does not perform well, as it requires an extensive amount of data and is highly sensitive to the data. In the revised version of the manuscript, we emphasized more clearly that we align with the conclusions of the National Academies of Sciences, Engineering, and Medicine.

5. The choice of Pearson Type I distribution over other distributions is missing.

   We have added more details in Section 3.1 explaining why the Pearson Type I distribution is the natural choice for the moisture maximization logic.

Minor comment:

- Line 304: The placeholder "Figure??" needs Figure number.

  Thank you for pointing that out. It is now corrected.

- Additionally, there is inconsistent notation in Equation 4 compared to Equation 1.

  Yes indeed, thank you.

- The term "EP" and "EPmax" should be clearly defined to maintain consistency and avoid confusion.

  This is a translation mistake from our part. Thank you for pointing that out. It is now corrected.

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Detailed Responses to Reviewer 3's Comments

Anne Martin, Élyse Fournier and Jonathan Jalbert

February 16, 2025

First, we would like to thank the three reviewers for their thorough review and relevant comments and suggestions. Based on their feedback and recommendations, we notably adjusted the structure of the paper and added numerous clarifications. The modifications in the revised version of the manuscript are highlighted in blue. Below are detailed responses to all of Reviewer 3's comments.

Interesting paper. However, I feel it may become more of use with additional investigations of refinements needed to the method that has been proposed. See my comments below.

1. l125: Is $EP_{\max}$ defined for a calendar day for nearby regions or defined as the maximum at that point location? Equation 1 referred to $PW_{\max}$ which is the atmospheric moisture. $EP_{\max}$ is I suspect the same but should be consistent or else defined. If non-seasonal, please refer to WMO guidelines for seasonal variations in PMP.

   Precipitable water should be written as $PW_{\max}$ and not $EP_{\max}$ throughout the paper; this has now been corrected. To answer your question, $PW_{\max}$ is defined as the maximum at that specific location. This has been clarified in the revised version, and we have also referred to the WMO guidelines regarding seasonal variations. Thank you for the suggestion.

2. l140 - interesting. However, the sampled r_i are non-iid, which complicates their use in defining the Beta distribution I think. Plus, there is an assumption that the sampled r_i has an upper limit of 1. Given this limiting value will dictate/influence the PMP estimate, the uncertainty associated with this assumption is important to characterize.

   It is true that the sampled r_i are not iid. Typically, for precipitation in the considered location, autocorrelation exists in daily non-zero precipitation series, but it is very weak and short-range. We believe that the impact of this very small dependence is quite limited compared to the overall sampling uncertainty, which already results in very large uncertainty in the parameter estimates. For example, the autocorrelation estimate for the non-zero precipitation series recorded in Montréal is 0.0092 for a lag of one day, and 0.0095 for the St-Hubert station. These values have been added to Table 1 to demonstrate that dependence is weak for the data considered. However, in regions where autocorrelation is stronger, it should be accounted for.

In this paper, the analysis is restricted to summer precipitation (from May to October inclusive) to minimize the effects of seasonality. While some seasonality may still exist, it appears negligible compared to the natural variability of precipitation and precipitable water as shown in Figure 1. We have added these figures to the revised version of the manuscript (Figure 1 in the revised version).

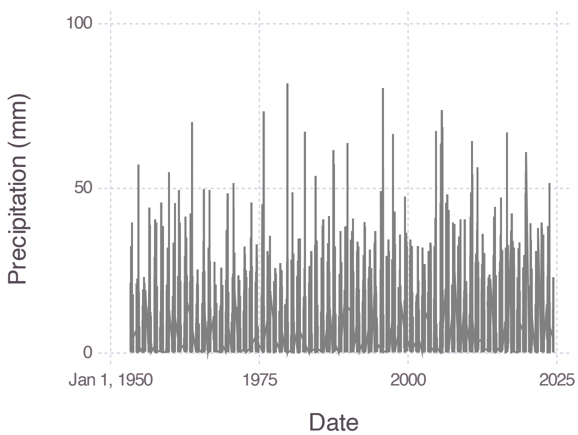
3. Also, how can stationarity be assumed given there is a clear temporal trend in precipitable water time series. Would violation of the stationarity assumption distort the beta distribution parameters?

   Yes, non-stationarity in precipitable water and/or in precipitation would distort the Pearson Type I distribution. For the considered observed data, there is no evidence of a trend in either the precipitable water and precipitation, as shown in Figure 1. This is due to the natural variability of these variables.

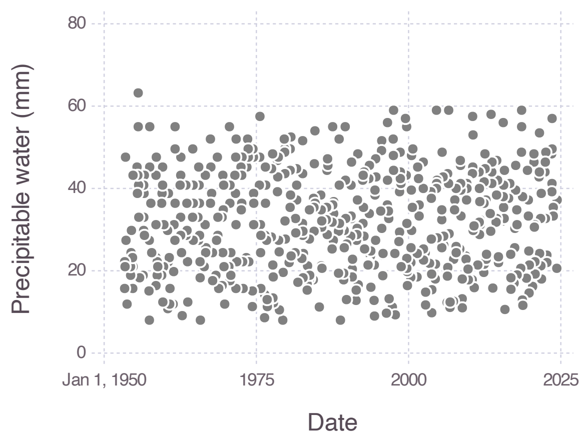
   Non-stationarity might be present in long series of simulated data from a climate model. In such cases, Eq. (6) should be extended to account for non-stationarity by allowing either or both the PMP and the ratio to evolve over time. In the context of the Pearson Type I distribution, this involves allowing the upper bound and the shape parameters to vary with time. This discussion has been added to the revised version of the manuscript (L404).

4. l375 - The authors recommendation makes sense. In addition to the issues they have mentioned, I also feel that the lack of independence (unless they parameters are being fitted using iid data above a threshold) and the presence of a trend are limiting factors. I wonder if using a nonstationary model and regional data can help overcome these limitations.

   Thank you. We agree that a regional model could potentially help in the estimation of the PMP. Such an approach would involve modelling the spatial dependence between precipitation at multiple sites. In future work, we plan instead to focus on spatial modelling of extreme precipitation within the framework of extreme value theory (L354).



(a) precipitation



(b) precipitable water

Figure 1: Time series of (a) daily precipitation and (b) precipitable water for the top 10% of storms recorded at the Montréal station.