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

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December 20, 2024

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.

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.

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.

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.

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 Section 5 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.

9. l304: Figure??

   Thank you for pointing that out.