Non-asymptotic distributions of water extremes: Superlative or superfluous?

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Reply on RC2 (Dr. F. Marra's report)

(Note: In the text below, Referees' comments were copied verbatim in **black**.)

We thank the Reviewer for the constructive feedback. In the following, we provide point-by-point responses in **blue**.

This manuscript critics the use of non-asymptotic (NA) distributions of block maxima. The authors bring two main arguments to support their critic. The first is that NA require knowledge of the parent distribution and that, when this is known, there would be no need for deriving distributions of block maxima. The second is that the presence of serial correlation in the observations would decrease the potential advantage of NA. The manuscript then follows with some targeted comments to specific studies. The paper addresses a relevant topic, but the manuscript falls short at supporting its main conclusions. This is not due to technical errors, rather to a narrow (mono-disciplinary) vision of the problems at hand (see main comments) and to an erroneous generalization of case-specific objections. Since all statistical models present advantages and disadvantages, which depend on how much the underlying assumptions are met/not met (or, to quote the statistician George Box: "all models are wrong, but some are useful"), I believe that the presented critics cannot be generalized to NA methods as a whole. Rather, they should be targeted to highlight specific aspects of NA methods that need attention and/or the specific NA approaches that need attention.

Considering the main comments below, I believe the manuscript should be deeply revised before reconsideration. I am not sure this can be handled within a major revision, because the take-home messages would need important adjustments. The title should be revised to be more pertinent with the actual outcomes. This also pertains the title of some sections/subsections which border disrespectfulness (e.g., section 3, 4, 5). The manuscript is long and contains numerous repetitions. It includes unclear and/or incorrect reasoning in some sections, which prevent from fully understanding some parts (see comments 6 and 7 below). It could be halved in length without changing the message.

All the references in this document can be found in the preprint of the manuscript.

Given my lack of specific expertise, comment 6 was written with the help of a colleague expert in Bayesian statistics.

I hope my comments are helpful.

Kind regards,

Francesco Marra

Response We thank the Reviewer for the time devoted to our paper. We are also grateful to the Reviewer who acknowledges that the paper is technically correct, thus he necessarily agrees that our conclusions are also technically correct. As discussed below, we believe that such conclusions are also fully general, as they apply to any NA model, including the ones introduced by the Authors of this paper (see e.g., Serinaldi et al., 2020b and Lombardo et al., 2019).

We agree that some concepts are sometimes repeated in the paper text, because we understand that they have been usually neglected in a huge amount of the literature dealing with NA models of block maxima (BM). Thus, we decided to follow the statement by the renowned physicist Arthur Leonard Schawlow: "Anything worth doing is worth doing twice", or even repeated more times when it comes to statistical analyses.

Main comments

1. My main comment turns out to be a citation from the manuscript itself (line 694): "models and methods should be thought and used in the right context and for suitable purposes". This sentence in the conclusions contradicts several of the arguments presented in the manuscript. The authors proceed for 30 pages (pages 1-30) repeatedly claiming that NA methods are 'superfluous'. Only in lines 685-694 they contradict this argument stating that what they report in the manuscript "does not mean that they are not useful at all". They then proceed listing what, in their view, are potentially useful applications of NA approaches and finish off with the citation I started with. I may add that the reported list of 'useful' applications is limited by the imagination of the authors (as shown by the sole presence of self-references in here) and, mostly, by the perspective they adopt. In this, it seems they forget the gap that there exists between theory and practice, between advancements in theory and practical use of extreme value distributions of any kind by hydrologists, risk modelers and end users in general.

Response We kindly invite the Reviewer to read our paper more carefully. Indeed, it seems here that the Reviewer has extrapolated the meaning of some incomplete sentences out of their context giving rise to misunderstandings and wrong interpretations of the Authors' statements.

Actually, highlighting the difference between theory and practice is the very aim of our paper. Indeed, throughout the text, we always and purposely use periphrases like "*little usefulness for* *practical applications*", "*usefulness of NA models in practical applications*", "*the problems concerning the use of NA models of BM for practical applications*", "*call into question the practical use and usefulness of NA*", etc., etc., whereas L685-694 refer to usefulness of NA models in *theoretical* context, as should be obvious to everyone.

The Reviewer is pointed to the distinction between practical and theoretical usage of NA models of BM, which is anticipated in L. 174-176, where we clearly state "*This explains why NA have not received much attention and why the recently proposed compound NA models are of little **practical** usefulness, if any. Their usefulness is mainly **theoretical**, as they help explain the inherent differences between parent processes Z and BM processes Y, thus avoiding misconceptions and misinterpretation of different model outputs (see Serinaldi et al., 2020b).*"

Thus, it is quite evident that there is no contradiction at all.

Furthermore, Reviewer's remark does not consider the key point, that is, the argument supporting our conclusion about the practical uselessness of NA models of BM: "*NA models of BM imply the preliminary definition of their conditional parent distributions, which explicitly appears in their expression. However, when such conditional parent distributions are known or estimated also the unconditional parent distribution is readily available, and the corresponding NA distribution of BM is no longer needed, as it is just an approximation of the upper tail of the parent*".

In fact, several applications of NA methods follow the directions accepted as 'useful' by the authors, and other applications of NA methods follow directions that are useful, although not within the directions imagined by the authors.

Response We disagree here with the Reviewer, as he seems to confuse practice with theory, namely theoretical and applied statistics. A clear understanding of such difference can be derived by reading (and comparing) for example Shao (2003; Mathematical Statistics) and Kottegoda and Rosso (2008; Applied Statistics for Civil and Environmental Engineers), among others.

One notable example is the connection between physical processes and statistics, which can only exist in a NA model, given that real world physics is not asymptotic. It is my believe that physical processes should direct the statistics we use. The physics of the processes we are dealing with is not asymptotic.

At this concern I must cite again the authors (lines 623- 624): "Historically, the main scientific progresses occurred when some one called into question widely accepted mainstream theories using arguments more solid than those of the superseded theories". It almost seems we think alike on this point, although with different concepts for 'mainstream'.

Response We are sure we think alike with the Reviewer on several points to such an extent that he previously stated that our paper is technically correct. In other words, he says our work is correct according to a strict interpretation of the rules. That means that the Reviewer agrees on all the scientific rules we have extensively pointed out in the paper, where we also showed that such rules are unclear, misinterpreted or even neglected in a huge part of the literature. Then, we are more confident in our vision that we can claim we are providing a valuable contribution to the scientific community. Other than this, we are afraid we did not understand the first sentence of Reviewer's remark, because:

- "Physical" (natural, or manmade) processes are what we observe around us.
- Physics (intended as a body of theories) and statistics are just modeling frameworks.
- Concepts like "asymptotic" or "non-asymptotic", "deterministic" or "non-deterministic", can only refer to models not to "*real world physics*", whatever it means.
- Observation records are yet another thing, and they have finite size.

If we say that "*real world physics is not asymptotic*", we can also say, for instance, that "real world physics is not *deterministic*": should we discard Newton's classic mechanics because "*real world physics*" is not uncertainty-free?

As stated by Morrison (2008) "*The next hurdle* [to get over in undergraduate mathematics] *is the differences among observed reality, mathematical models, and computational realizations of mathematical models. Even a lot of accomplished scientists are not clear on these points... learning to cope with three things makes up the basics of a liberal scientific education: facts, abstractions, and the comparison of facts with abstractions... Understanding and ultimately research occurs only when facts are reduced to abstraction, the abstractions manipulated to make predictions, and the prediction compared with new facts*".

Nowadays, it seems that there is a big confusion about basic epistemological and semiotic concepts, which are fundamental to make meaningful statements.

2. One argument is that NA methods require the knowledge of the parent distribution and that when this is known, there would be no need for deriving a block maxima (BM) distribution. This is technically true but seems to neglect situations in which a BM distribution is helpful (even though, I agree, not technically essential). Some examples: empirical comparison with observations of BM only; fair comparison with estimates from EVT distributions; providing information that practitioners can use without changing habits. Doing these directly from the compound parent, although possible, would be troublesome and possibly confusing for non-experts.

Response Again thanks to the Reviewer who thinks that our arguments are technically correct. Reviewer's fairness is indeed greatly appreciated. Concerning the situations where models of BM (asymptotic or not) would be helpful, we refer to our paper's Section 4, which already addresses Reviewer's remarks:

1) Rescaling compound parent distributions has a degree of complexity that is always less or equal to deriving the corresponding NA models for BM (see Section 4.1.1).
2) Why should one compare (superpose) the distribution of a given process (e.g. streamflow) with the empirical (or a theoretical) distribution of annual maxima in a practical situation? Once we know the probability of (non)exceedance of a given streamflow value, this is all we need, indeed:
    a. If BM (annual maxima) are the only available data, NA models cannot be built.

b. If we have enough data to build NA compound parent models, BM models are irrelevant.

BM datasets are not of interest *per se* in any real-world application. They are only functional to rebuild the upper tail of the distribution of the parent process via distributions that hopefully do not require the (detailed) knowledge of the parent distribution (under suitable conditions). Other than that, BMs have no special purpose in practical applications, engineering design, management, etc.

3) "*fair comparison with estimates from EVT distributions*": Figures in the paper show fair comparisons among NA models of BM, NA parent models, and EVT models. More importantly, from a practical standpoint, once the parent model is available (and assumed to be reliable), any other model of BM (asymptotic or not) is just an approximation of its upper tail: as an approximation, it is always less accurate/correct than the parent models. Why should we build and compare two models of BM, when we already have a distribution that is superior by construction? Models of BM have no longer place once we decide to build/recover (compound) F_Z.
4) "*providing information that practitioners can use without changing habits*": using parent distribution has at most the same degree of difficulty as using a POT distribution (such as the classic GP) in terms of derivation of return period or other summary statistics. Moreover, how can a practitioner be more comfortable with models that are more convoluted than their parent models?
5) In our experience, "non-expert" and "practitioners" should be trained to properly use models and methods rather than providing them with more and more convoluted models that they do not know/understand and likely misuse due to apparent user-friendliness. On the other hand, if practitioners are well trained and can understand the nature and structure of compound NA models of BM, they will recognize that the parent models are the most straightforward option.

3. The issue with serial correlation is important and could affect some applications of NA methods. I believe future NA applications (either for block maxima or directly from the parent distribution) should keep this in mind. In this, the paper is a relevant addition to the literature. Still, it falls short at supporting the adjective 'superfluous' that accompanies the reader. The importance of serial correlation depends on the type of variable one wants to examine and on how the variable is used in the model. It cannot be generalized to the application of NA methods as a whole. Incidentally, serial correlation also negates the assumptions of extreme value theory (EVT), with the effect of making the convergence much slower. Slow convergence actually suggests that NA methods should be used, making the reasoning circular and thus highlighting once again the complexity of the problem.

Response We kindly reply to the Reviewer with the following points that are already included in our manuscript:

1) We do not vaguely talk about serial correlation and dependence: we specifically show that "*when declustering procedures are used to remove autocorrelation characterizing hydro-climatic records, NA distributions of BM devised for independent data are strongly biased even if the original process exhibits low/moderate autocorrelation. On the other hand, NA distributions of BM accounting for autocorrelation are less biased but still of little practical usefulness*" because they are yet approximations of the already available compound parent distributions. The NA models of BM are redundant in any condition (dependence, independence, stationarity, non-stationarity, or anything else).
2) EVT can take some kind of dependence into account by e.g. extremal index. In other cases, such as the presence of strong dependence, EVT just says that the asymptotes can be different from GEV/GP.
3) We do not say anywhere that NA methods should not be used: we say that NA models of BM are redundant in practical applications as they just approximate the upper tail of the already available NA parent distributions (Figs. 1 and 3), and both are biased under dependence and independence (Figs. 4 and 5).

   We do not even contrast asymptotic and NA distributions of BM: we show that the NA models of BM miss the main point that justifies the use of BM models, that is, no need of information about the precise nature of the parent distributions.

4) There is no circular reasoning affecting asymptotic and NA models: if we have only BM, NA models cannot be defined. If we have complete data sets, we can build (compound) parent distributions and we do not need any NA model of BM. Alternatively, we can use EVT (asymptotic) models if we do not want (or cannot) fit a suitable parent model for some reason.
   Circular reasoning only affects NA models of BM ("children") when compared to their compound parent ("parent"). Indeed, we generate the "parents" to give birth to "children", then we use the "children" to recover part of the "DNA" of the parents, which is already known! This is circular reasoning.
   Conversely, "dependence" (and slow convergence) is just a technicality that can be addressed asymptotically or not. Moreover, EVT goes far beyond GEV and GP distributions. Circular reasoning concerns logical arguments rather than technicalities.

We believe the relevance and importance of such concepts described above justifies their various repetitions throughout the text of our manuscript.

4. Relatively large portions of the manuscript are dedicated to commenting specific works (sections 4.2, 5.3; 10 pages in total). Given that previous work, also by some of the authors (Serinaldi et al., 2020), and this work itself confirm that NA methods are formally correct, it is not fully clear how objections to specific works should affect NA methods in general.

Response The Reviewer is kindly pointed to Section 4.2 of our manuscript, which clearly shows the redundancy and practical uselessness of NA models of BM: "*being formally correct/incorrect*", "*being biased/unbiased*", and "*being useful/useless*" are different things. For example:

- Under serial dependence, the classical estimator of the correlation coefficient is (i) formally correct, (ii) biased, and (iii) only partially useful (as there are better estimators of linear dependence).
- NA models of BM are (i) formally correct, (ii) biased, and (iii) practically useless in real world applications (data analysis).

On the other hand, our Section 5.3 shows that the simulations used in the literature to support MEV models are formally incorrect as they confuse expected quantile functions and expected probability functions.

While we refer to specific papers, our remarks are fully general: all NA models are redundant when compared with the corresponding (and known) NA parent models (Section 4.2), and all NA models (parent and BM) are biased because of their compound nature (Sections 5.1, 5.2 and 5.3).

We are sure that the Reviewer is fully aware of the difference between general conclusions and case-specific conclusions. If he thinks that our statements are not general and/or not supported by our analyses, we will be glad to discuss possible counter examples that he can provide if he wants.

Specific comments

5. In section 3, the authors treat EVT as if it was the truth. Statistically it is, provided that the underlying assumptions are met. Among these the asymptotic assumption. In some relevant cases, convergence to the asymptote is (very) slow, such as the case of the powered exponential family of distributions. Notably, this is the case most relevant for precipitation, and precipitation is the main variable on which NA methods are confidently used (due to the relatively simpler relation with the underlying physics). In fact, in the case of precipitation tails from EVT are too heavy. This becomes clear when one tries to generate stochastic time series from a EVT distribution, and led to the development of a family of powered-exponential distributions for the generation of stochastic precipitation series (Papalexiou, 2022). These tails explain well the statistics of observed extremes, as shown by Marra et al. (2023) (more on this later). Overall, in that paper, we showed that GP tails from EVT and powered exponential tails from NA models can be indistinguishable, with the difference that the former are asymptotic distributions fitted to NA data. The message is once again that no model is perfect, and that different models may lead to similar answers, thus advancing our understanding of nature.

Response We regret to say we disagree with the Reviewer here for the following two reason:

- Section 3 does not treat EVT as the truth, whatever "truth" means.
- "*Statistically it is*"? What does it mean that EVT is a statistical truth?

We do not think that such comment is related to the content of Section 3 of our paper. Let us summarize Section 3. Its title is "*Modeling extreme values: asking 'why' before looking for 'how'*" because we noted that the literature on NA models of BM is so focused on convoluted transformations of the parent distributions F_Z that it seems to miss the key point: for a process Z,

F_Z already provides all information about the probability of every quantile (extreme or not). We do not need any other distribution, which is necessarily less informative of F_Z.

Even the Reviewer's remark seems to talk about "how" (convergence, Weibull distributions, simulations, etc.), and it does not focus on "why" we develop such models, which is instead the topic of Section 3.

Distributions of BM have been studied as one hopes to get insights into the tails of F_Z **when F_Z is not available** for some reason (e.g., lack of data, or difficulty to reliably identify F_Z).

When F_Z is available, we do not need any distributions of BM (either asymptotic or non-asymptotic) to define the probability of any quantile. Indeed, in real world applications, we need the probability of z (discharge, rainfall, etc.), not the probability of BMs (streamflow AM, or rainfall AM).

"*One might wonder why we should be interested in an asymptotic distribution of Y when the exact distribution, which is given by $F_Y(z) = F^m{}_Z(z)$, where F_Z is the c.d.f. [cumulative distribution function] sampled from, is known.* ***The hope is that we will find an asymptotic distribution which does not depend on the sampled c.d.f. F_Z***." (Mood et al., 1974, p. 258).

To summarize:

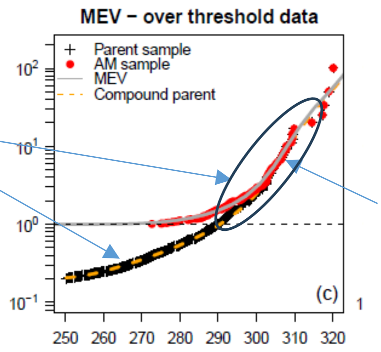
1) The process of interest (rainfall, streamflow, etc.) is Z.
2) The probability of any quantile is described by F_Z.
3) When F_Z is known, we do not need anything else to calculate the probability of any z.
4) When F_Z is unknown:
    a. Asymptotic models of BM provide an approximation of the tails of F_Z that **does not require** the (precise) knowledge of F_Z.
    b. NA models of BM cannot be derived as they **require** the preliminary knowledge of F_Z, which explicitly enters in their expression.

Then, we highlight once again that searching for an approximation of the upper tail of F_Z makes no sense if the whole F_Z is already known: why should one build an inferior approximate model of a sub-process, when one already has a superior model describing the whole process?

Supporters of NA models of BM always contrast these models with asymptotic models, without recognizing that the true competitors of NA models of BM are the parent distributions that need to be preliminarily identified for the derivation of NA models of BM. And parent models are always superior to any model of BM as they describe the whole state space of process Z, whereas models of BM (asymptotic or not) describe just a subset.

For clarity, we further summarize the key points graphically in the figure below.

(Compound) parent distributions of Z (orange line) are more informative than any and every asymptotic or non-asymptotic model of BM (grey line) as they describe the whole state space.



Asymptotic or non-asymptotic models of BM (grey line) are just approximations of the upper tails of parent models (orange line).

NA models of BM are redundant, as they require the preliminary identification of the parent distributions, which are more informative than their surrogate NA models of BM.

The "querelle" between supporters of asymptotic models and NA models of BM is ill-posed: the competitors of NA models of BM are not the EVT models, but the more informative parent distributions required for their derivation (and appearing in their expression).

6. Some concepts in Section 5.3.1 are misused. Montecarlo simulations (both in its standard term and in its Markov Chain variant) are numerical methods to compute integrals and expectation, sampling from a target distribution numerically and approximating the expectations via empirical average. However, the description provided by the authors is confused and, at least for what concern the different approaches to statistical inference (here the frequentist or classical paradigm and the Bayesian one), wrong. Montecarlo simulations in frequentist inference and Markov Chain Montecarlo (MCMC) in Bayesian inference target totally different objects. The authors correctly assess the role of Montecarlo simulations under a frequentist approach to statistics. Under this point of view there exists a true population's characteristic (or statistics, using the authors term) that is estimated (intrinsically with some uncertainty) from a finite sample. The variability of the estimator (and not of the parameter that the estimator is targeting) can be assessed in many ways, e.g. exploiting Montecarlo sampling to mimic the repeated sampling principle thus allowing to construct frequentist confidence sets. In Bayesian inference, instead, do not exist a 'true' parameter of the population as this is consider a random variable itself. Consistently with this, the posterior distribution of any unknown, which is often approximated via MCMC sampling is the target of inference. While posterior summaries like the posterior mean are common, they represent fundamentally different entities from frequentist estimators. In Bayesian inference, MCMC draws are used to construct credible sets, intrinsically different from the notion of frequentist confidence sets. The uncertainty that the posterior is describing is not the same uncertainty that the estimator variance in frequentist inference (obtained in any way, including Montecarlo sampling) is describing.

Response We thank the Bayesian statistician involved by the Reviewer for his/her overview about the scope of MC simulations, and the difference between frequentist confidence intervals and Bayesian credible intervals. We are fully aware of such concepts, being already familiar with the explanations provided by e.g. Nicholas Metropolis, Arianna and Marshall Rosenbluth, Stanisław Ulam, and Edward Teller in their original papers on MC, or Bernardo and Smith (2000), Gelman et al. (2003), or Robert (2007) in their Bayesian books, etc., being left alone the original De Finetti's works on subjective probability.

However, we must stress here that section 5.3.1 and the whole paper have nothing to do with Bayesianism or the "*46656 varieties of Bayesians*" (Good, 1971, Am Stat 25:62–63).

Our paper does not report any credible interval or Bayesian analysis. In the first paragraph of Section 5.3.1, we just state that simulation methods have several applications, and one of them is "*to obtain posterior distributions of model parameters with unknown mathematical form*", that is, when closed form of posterior distribution is not available, which is the most common case in data analysis.

Summarizing a multi-model ensemble is a general problem that is fully independent of the inferential strategy, as multiple models can result from sampling uncertainty analysis in frequentist fashion, from posterior distributions in Bayesian inference, or just from multiple physical models (with different model structure) without involving any statistical inference. The third paragraph in Section 5.3.1 states that the problem of summarizing multiple models is well known for instance in Bayesian literature just because the typical output is a set of models corresponding to parameter sets usually sampled via MCMC. And this has nothing to do with the difference between credible intervals and confidence intervals, which should be indeed well known to anyone who uses applied statistics to "play" with data.

Finally, the third and last entry of the term "Bayesian" is in L. 646, where we discuss "predictive distribution". However, also in this case, Bayesian inference/theorem does not apply whatsoever, as predictive distributions are just an application of the total probability theorem and marginalization.

In this respect, we endorse the following statement by Christakos (2010): "*when an investigator was asked if he is a "Bayesian" or a "non-Bayesian," he responded that he is an "opportunist," meaning that he would use whatever approach works best for the given in situ conditions*".

Additionally, Bayesian model averaging is a well-known and successful concept that is not related to the summarization of the (MCMC approximated) posterior distribution of any kind.

Response Indeed, we do not apply and do not even mention "Bayesian model averaging" anywhere in the paper.

Despite stemming from confusing arguments about basic concept of frequentist and Bayesian inference, the discussion starting from eq. (10) to the end of Section 5.3.1 is correct. However, it is a mere consequence of eq. (17) and deserves less space. Perhaps lines 453-485 can be removed and the subsequent text rearranged.

Response Thanks indeed to the Reviewer and his Bayesian colleague for stressing once again the correctness of our paper. We believe indeed that the discussion after Equation 10 is correct because its premises are correct, as they are fully general, and do not refer and are not limited to frequentist or Bayesian inference: they refer to how one can summarize multiple models, and this has nothing to do with the specific inferential procedure (often, involved models are not even statistical).

Lines 453-485 are necessary to introduce predictive and median quantile/probability functions that play a key role in the interpretation of results in Section 5.3.2 (as explicitly stated in L. 450-451).

Clearly, Section 5.3.1 must be read in the context as clearly recommended in L. 449-451.

7. Section 5.3.2 is not clear. Specifically, I could not grasp whether the objection concerns (a) the average from the synthetic timeseries of the Montecarlo samples, or (b) the average in the MEV formulation. Are the authors claiming that the figure in Marani and Ignaccolo draws something different from what is claimed, or that the MEV framework is incorrect? The suggested changes in Fig. 7 indicate that we are in case (a). Should this be the case, the entire section 5.3 would be a direct comment to Marani and Ignaccolo (2015) that not necessarily pertain NA methods in general, but only the Montecarlo sampling in here. Should (b) be the case, it is not clear why section 5.3.1 is there and why all the distributions (not only MEV) change in figure 7. Even in this case, the comment would not pertain NA methods in general.

Response The justification of the analysis in Section 5.3.2 is explicitly and clearly stated at the beginning of Section 5.3: "*Therefore, we re-run Monte Carlo simulations described by Marani and Ignaccolo (2015) to understand the reason of such a disagreement* [with simulations in Section 5.1 (reproducing those of Marra et al. (2018))]. *We anticipate that the foregoing discrepancies depend on the misuse of methods used to summarize multi-model ensembles. Thus, before describing Monte Carlo experiments and their outcome, we need to recall some theoretical concepts that are required to correctly interpret numerical results.*"

The message of results in Section 5.3.2 is indeed very simple:

- NA models (BM and parent) are biased thus confirming results in sections 5.1 and 5.2.

- These results contrast with those of Marani and Ignaccolo (2015), which are affected by incorrect use of multi-model averaging over S and $\Omega_{\boldsymbol{\theta}_S}$.

- Since results reported by Marani and Ignaccolo (2015), including apparent lack of bias, are routinely used to justify the goodness of NA models of BM (due to supposed better performance with respect to EVT models), Section 5.3.2 shows that such arguments are not valid.

To conclude, our concerns refer to both options (a) and (b), which are not mutually exclusive, even though we must rephrase them for the sake of correctness. We state that the figures in Marani and Ignaccolo show something different from what is claimed, <u>and</u> that the MEV framework and <u>any NA model</u> (BM or parent) is <u>biased</u> (… not "incorrect"). Indeed, in statistical modelling there is no "free lunch": what we gain in reduced variance, we lose in increased bias, and vice versa. However, NA models are routinely described in the literature missing their bias, which disappeared in the incorrect diagrams reported in a paper that is usually cited as a starting point for these NA models of BM (neglecting numerical errors, lack of correspondence between figures and text, etc.).

8. In section 6, the authors briefly comment on a paper of mine in which NA (Weibull) and asymptotic (GP) tails are compared for the case of precipitation. They quickly dismiss our study claiming that we used a low threshold "out of its range of validity". We reported results for threshold equal to the 95-th percentile for consistence with the Weibull model, but we clearly stated that "Results derived from higher thresholds such as the 98-th percentile used by Serinaldi and Kilsby (2014) are qualitatively analogous but characterized by larger uncertainties" (Marra et al., 2023). For reference, I report here the same as figure 3 in Marra et al. (2023) as it was obtained using a threshold equal to the 98-th percentile (Figure 1 below). As it can be seen, the instances in which GP provide too heavy or too light tails are even increased when using the 98-th percentile with respect to the 95-th percentile case (please refer to Marra et al., 2023). This is because in addition to theoretical convergence issues (what the authors focus on), there are important (practical) issues with stochastic (sampling) uncertainty.

Response We disagree with the Reviewer here, because the foregoing remark starts from premises or statements that are attributed to our paper even though they do not appear anywhere.

Furthermore, we do not dismiss any paper: we call into question the interpretation of Fig. 5 (not Fig. 3!) in Marra et al. (2023), which has practical consequences as clearly stated in L. 571-592. The Reviewer interpreted that figure (reported below for convenience) as follows:

"*<u>the errors for maxima sampled from GP tails strongly depend on the left-censoring threshold and tend to be too heavy-tailed for ϑ ≤ 0.90</u>. <u>The accuracy of GP tails in reproducing the statistics of observed maxima is comparable to the one of Weibull tails only for thresholds ϑ > 0.9.</u> Second, GP* tails estimated from synthetic Weibull-distributed data, are virtually indistinguishable from the GP tails estimated from real observations (dashed blue). <u>As predicted by EVT, GP tails tend to provide similar estimates upon asymptotic conditions</u> (here represented by ϑ_{GP} = 0.95; see also Serinaldi and Kilsby, 2014) <u>and the difference in L-moment ratios between non-asymptotic Weibull tails and GP tails decreases with increasing threshold</u> (Fig. 5c). Crucially, the difference between L-moment ratios of annual maxima emerging from GP and GP* tails (dashed) are virtually indistinguishable also for high thresholds such as ϑ = 0.95, and smaller than the differences between L-moment ratios of annual maxima emerging from GP and Weibull tails (Fig. 5c). Estimating GP tails from observations is equivalent to estimating GP tails from Weibull data.*"

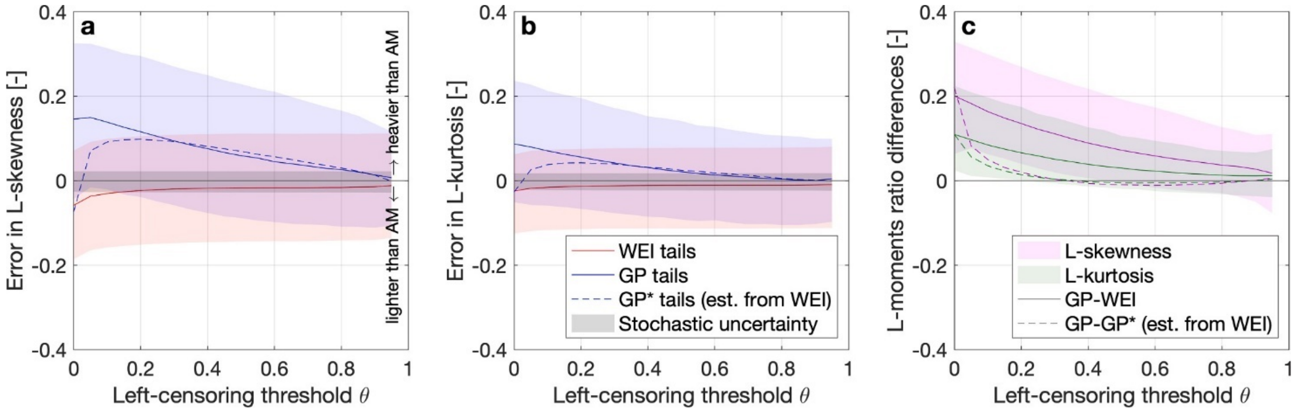


Fig. 5. Error in L-skewness (a) and L-kurtosis (b) of annual maxima estimated from MC samples of 10^3 years of non-asymptotic Weibull tails (WEI, red) and GP tails (blue) with respect to observed annual maxima; solid lines and shaded areas represent, respectively, median and 90% confidence interval across the stations; dashed blue lines show the median for the case of GP tail model estimated from the synthetic Weibull tails (GP*); shaded grey areas in (a) and (b) quantify the stochastic uncertainty due to the available data record in presence of non-asymptotic Weibull tails. (c) Difference between L-skewness (purple) and L-kurtosis (green) derived from GP and Weibull tails (solid lines for the median, shaded areas for the 90% confidence interval) and from GP tails and GP* estimated from the synthetic Weibull tails (dashed, only the median is shown).

This behavior is expected because, as we state in the paper, "*The natural interpretation of these results would be that the Weibull distribution is a good model F_Z for the parent process Z (positive rainfall or rainfall over low/moderate thresholds) confirming previous results reported in the literature, while GP model works well for exceedances over high thresholds (as postulated by EVT), and does not work well (as expected) for low/moderate thresholds, that is, outside its range of validity.*"

The poor performance of positive rainfall with GP behavior up to moderately high threshold is not a limitation of GP, but an effect of using GP for "the body of the distribution". These diagrams (and the overall results reported by Marra et al.) do not call for a renewed consideration of nonasymptotic statistics (NA models of BM) for the description of extremes. They just translate in the following conclusions: "*(i) use GEV if only BM are available (e.g., AM from hydrologic reports), and (ii) use F_Z (e.g., (compound) Weibull) if you have information on Z, which can be either the process of all positive rainfall or rainfall over arbitrary low/moderate thresholds if the latter is deemed easier to fit. In the latter case, calculate the T-year return levels as the $(1-\mu/T)\cdot 100\%$ quantiles of F_Z, where μ is the (mean) inter-arrival time (in years) between two observations of Z (e.g., Serinaldi, 2015; Volpi et al., 2019).*

Such a plain reasoning highlights that there is no need to build an additional distribution of BM (i.e., SMEV, MEV or whatever else), in the same way we do not need to define the GEV distribution of AM once we already inferred a GP model of POT."

Once (compound) Weibull (or anything else) is identified as an acceptable model for all positive rainfall or rainfall over arbitrary low/moderate thresholds, the behavior of any quantile (extreme or not) is completely described by this distribution. We do not need any other asymptotic or NA models of whatever surrogate process.

9. The manuscript presents numerous self-citations.

Response See our reply on CC1.

10.Incidentally, as a user of NA methods, I never claimed they are 'superlative'. They are as good as other models are: they offer advantages in some situations and disadvantages in others.

Response Thanks for this comment. We understand the Reviewer's point and in fact we never stated that the Reviewer claimed that NA models are superlative.

In our paper (and several previous papers of ours), we only criticize methods and/or other papers, not their Authors. However, we also understand that sometimes researchers tend to "fall in love" and/or identify themselves with models and methods that they use and promote, especially if they do that for a long time.

We are confident that the reader can distinguish the different roles played by tone, style, and content in a written text, and therefore the intended meaning of the question "*Superlative or superfluous?*" in the paper title.