**Non-asymptotic distributions of water extremes: much ado about what?**

**Previously: "*Non-asymptotic distributions of water extremes: superlative or superflous!*"**

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## REPLY ON EDITOR'S REPORT

(Note: In the text below, Editor's comments were copied verbatim in **black**, whereas responses and changes in the text are in **blue**.)

Thanks to the authors and reviewers for detailed and thoughtful comments. While the paper primarily focusses on demonstrating the "redundancy and practical uselessness" of the Non-Asymptotic models of Block Maxima, however, in the conclusions section (lines 685-695), the authors adopt a more moderate and positive tone about the criticized approach. This is somehow contrasting with most of the paper's tone. The second referee (RC2 Report) also expressed a similar opinion about this topic.

***Response***

Dear Editor,

Thanks for giving us the opportunity to revise the manuscript and reply to Reviewers.
We understand that the tone of our paper is the only significant criticism to our work. In fact, all reviews we received acknowledged that our paper is technically correct, which is extremely encouraging for us.
Nevertheless, concerning the supposed contrast between the content reported in the text and the sentence extrapolated from the conclusions, we already discussed this issue in depth in our reply on RC2, which we report for convenience below:

"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*"."

Therefore, we believe that "contradiction" may arise from (i) superficial reading (possibly neglecting entire sections), (ii) improper extrapolation of sentences out of the context, and/or (iii) biased reading. Anyway, in the revised paper we implemented some modification according to the editor suggestions.

Providing constructive criticism in a positive manner would enhance the paper's contribution to the scientific community in general. I recommend maintaining a positive tone throughout and avoiding terms as "useless" or "superfluous". Additionally consider a subtle title change to balance appeal and accuracy. While I understand that a more provocative title might capture a wider audience, I would suggest a subtle title change avoiding those terms.

***Response***
We understand Editor's point of view; therefore, we changed the paper title accordingly and eliminated the terms "superfluous" and "useless" throughout the revised text.

Regarding your statement in the RC1 document, specifically: "Under iid assumption, order statistics have a binomial distribution (which is equivalent to a beta distribution)". Please revisit these statements during your revised manuscript preparation and check for correctness.

***Response***
Thanks. We have clarified this point in the revised text as follows "*and the latter is described by a generalized beta distribution (see Eq. 1 as well as Eugene et al., 2002; Tahir and Cordeiro, 2016)*".

In fact, as mentioned in our reply on RC1:

"*Under i.i.d. assumption, order statistics have a binomial distribution (which is equivalent to a beta) in the sense described by Equation 1 (see also David and Nagarajah 2003, pp.9-10). In other words, the distribution of the order statistics is a beta distribution of the variable $F_z(Z)$, or equivalently a so-called beta-extended distribution of Z (Eugene et al. 2002), which is also known as generalized beta-*

*G distribution, where "G" denotes generalized classes, such as exponentiated-G or Kumaraswamy-G (e.g., Tahir and Cordeiro, 2016)."*

The RC2 report extensively discusses the MCMC concept and the Bayesian approach. I recommend removing the example from section 5.3.1 that involves Monte Carlo methods within the Bayesian framework (i.e. MCMC). Instead consider providing an example more relevant to your actual research since the Bayesian approach is not utilized here.

***Response***

Thanks, we removed references to the Bayesian approach in the revised paper.

The RC2 discussion about the connection between physical processes and statistics is intriguing. However, it might be less relevant than considering the impact of the autocorrelation in the NA BM estimation methods described in section 5.2.

***Response***

To give some more food for thought about this issue, in the following we quote a recent miscellaneous work by Prof. Demetris Koutsoyiannis (2024; https://www.itia.ntua.gr/en/docinfo/2468/), who thoroughly addressed the distinction among real-world observations, models, and model outputs, as well as a vision of the relationship between physics and statistics that dates back to the 19th century:

*"… when applied to physical problems, statistical methods become parts of physics. Clockwise physics, without using probability and statistics, has been conventional wisdom for a couple of centuries but has proved to be weak and inadequate. Hence, stochastics has long ago been incorporated into physics. This occurred one century and a half ago, but admittedly, many of us… are not updated on this fact yet and continue to contrast physics and statistics. Therefore, I am providing the following information in bulleted form (along with my apology for being didactic):*

- *Statistical physics (cf. Boltzmann, Gibbs, Planck) used the probabilistic concept of entropy (which is nothing other than a quantified measure of uncertainty defined within the probability theory) to explain fundamental physical laws (most notably the Second Law of thermodynamics), thus leading to a new understanding of natural behaviors and to powerful predictions of macroscopic phenomena. Atmospheric processes are explained by statistical physics in all respects (thermodynamic equilibrium, blackbody radiation, transport processes)*

- *Quantum theory (cf. Heisenberg) has emphasized the intrinsic character of uncertainty and the necessity of probability in the description of nature.*

- *Developments in numerical mathematics for applications in physics (cf. Metropolis) highlighted the effectiveness of stochastic methods in solving physical problems that are even purely deterministic, such as numerical integration in high-dimensional spaces and global optimization of non-convex functions (where stochastic techniques, e.g., stochastics-based evolutionary algorithms and simulated annealing, are in effect the only feasible solution in complex problems that involve many local optima).*

*This extends even beyond physics. Thus,*

- *Genetics (cf. Mendel) and evolutionary biology have emphasized the importance of stochasticity (e.g., in gametes fusion, selection and mutation procedures, and environmental changes) as a driver of evolution.*

- *Developments in mathematical logic, and particularly Gödel's incompleteness theorem, challenged the almightiness of deduction (inference by mathematical proof). This necessitates the use of induction in physical problems, whose theoretical basis is offered by the field of stochastics*".

Thus, Reviewer#2's statement "*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*" makes little sense because:

- NA models are just tools devised for very specific finite-size-sample problems.
- Physics, statistical physics, quantum physics, etc., are neither asymptotic nor non-asymptotic; these properties only concern how models specialize for the problem, data, and spatio-temporal scales at hand.

A clearer statement about the role of physics and statistics in hydrological modeling is provided by Montanari and Koutsoyiannis (2014; https://doi.org/10.1002/2014WR016092), which we report verbatim for convenience:

"*We believe that there is a widespread misconception in the hydrologic community, related to the use of process-based versus statistical models. The prevailing view is that process-based deterministic models are deductive means that take advantage of the available knowledge of the process dynamics, while statistical models are inductive and therefore are useful when the above knowledge is limited. We believe that this view is inconsistent. In complex hydrological systems, both deterministic and stochastic models are necessarily inductive (as they rely on fitting on data), while any deductive component in a deterministic model can be conveyed also in a stochastic model [Montanari and Koutsoyiannis, 2012]. The actual difference between deterministic and statistical models is just that the former establish a precise relationship between input (including initial and boundary conditions) and output (including system state), while the latter examines the probabilities of events (or time evolution thereof) by admitting that randomness, and therefore uncertainty, is inescapable. A statistical or stochastic model is just not deterministic: it can be physically based, it can represent spatial and time variability and can take full advantage of the knowledge of the system. Because of this, stochastic models with an increasing content of physical reasoning have been gaining increasing attention over the last decades. In order to identify the appropriate model to use, one should simply decide whether one wants to represent the inherent randomness affecting hydrological processes, and whether or not one wants to take uncertainty into account. There is no doubt that process-based models are the most appropriate solution for solving many water related problems, but we do not see any reason not to formulate them in a stochastic context. In our opinion, stochastic-process-based models are the way forward to bridge the gap between physically-based models without statistics and statistical models without physics. There has been a lot of applications in hydrology that clarified the potential of stochastic process-based models*".

Building stochastic process-based models only concerns incorporating uncertainty and has nothing to do with asymptotic or non-asymptotic models. For example, are stochastic differential equations (SDEs) asymptotic or non-asymptotic? SDEs are stochastic process-based models including deterministic terms (describing the dominant evolution some dynamical system) and stochastic terms accounting fluctuations that cannot be described by the deterministic part.

In lieu of the two reviewers' reports and community comments I suggest a major revision of your manuscript along the lines suggested by the reviewers and my own recommendations.

***Response***
We thank once again the handling Editor and Reviewers for the time devoted to our paper. Concerning the specific remarks:

- As per Reviewer#1's request, <u>we revised the notation and some unclear sentences</u>. Please, note that HESS provides language editing at proofreading stage that will fix residual problems.
- Concerning Reviewer#2, he explicitly stated that there are no technical errors (apart from typos in some formulas spotted by Reviewer#1). On the other hand, our detailed responses clarify that Reviewer#2's remarks are just part of a classical scientific debate between a researcher who supports NA models for BM and researchers that wrote a technically correct manuscript that criticizes such models. In fact, recognizing that our results are technically correct implies that our work has some value for the hydrological community, and it possibly deserves publication in HESS.
- Concerning the Discussant's comments, <u>we updated the figures to make them more readable</u>.
- As far as the remarks about style, length, and presentation are concerned, we revised the text according to the suggestions of the Editor and Reviewers bearing in mind the importance of repeating some crucial piece of information to ensure reception by readers of various background.

  We also noted that the Reviewers mostly focused on Section 5.3.1. However, our work reports theoretical and conceptual reasoning that justify our point of view as well as data analysis (Section 4) and extensive Monte Carlo simulations (Sections 5.1. and 5.2). These simulations reproduce and extend the numerical experiments reported by Reviewer#2 in one of his papers. Moreover, some Reviewers' comments about Section 5.3.1 seem not to account for its premises and consequences reported in Section 5.3.2.

  Therefore, we stress again our belief that our work has some value for the hydrological community, and it possibly deserves publication in HESS.