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

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Reply on CC1 (S. Han's report)

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

Response We thank the Reviewer for the interesting feedback. In the following, we provide responses in **blue**.

I think that this manuscript is very dispersive. I suggest to insert (in the first part) at least one flow chart and one figure, in order to facilitate the understanding of all the steps for a common reader.

This is the difference between a very good scientific paper and a common one; in the latter case there could be the risk that only the authors and a very small set of readers can deeply understand the work!

Font size of Figures 2 and 3 seems very small! A very good dissemination of results also requests suitable figures. I suggest to enlarge the dimensions and (mainly for Figure 3) to create two figures, aimed at a better visualization (and understanding) of the plots.

Figures 4, 5 and 6: is logarithmic the scale of the vertical axes? I suppose it, but it is better if Author specify it along the text (or in the captions). This is always for a clear presentation of the results to all the readers in the scientific community.

Sections 5 and 6: my opinion about the "dispersion" is also confirmed by the presence of mathematical formulas in the second part of this manuscript. Indeed, the whole methodology description should be placed in the first part of a scientific paper, while the second part should be only dedicated to the discussion of the results.

Overall, this manuscript seems to suffer from two issues:

1. a not so clear (for all) presentation of the methodology;

2. self-referentiality: in the references part I counted twenty papers of Serinaldi, and this seems not so elegant in the scientific community…

Sincerely

Response Again thanks for your time spent reading our manuscript. We will surely improve figures' font size and presentation in the revised version.

In the meantime, we would like to provide a response about the two key points raised by the Discussant:

- "Dispersion" (paper structure and materials' organization): As clearly stated in the introduction, our paper falls in the class of neutral validation papers, implying the analysis of the foundations of a given methodology. This requires an investigation of each stage of the considered methodology, bearing in mind that the conclusions/remarks/criticisms about each stage have consequences on the next one. Therefore, this type of papers should be read 'front-to-back' as an evolving story, and formulas are introduced when they are needed to discuss each specific point (other examples of this approach can be found in our previous papers cited in the text and references therein).
  In this respect, we purposedly introduce equations concerning the synthesis of multi-model ensembles in Section 5.3.1 so that the reader could benefit from fresh exposure to the concepts required to follow the discussion in the subsequent Section 5.3.2. Reporting those equations elsewhere (e.g., after Section 2 or in an appendix) would not allow a precise understanding of the inconsistencies discussed in Section 5.3.2. This understanding is of paramount importance for this type of papers, as their aim is precisely to show that neglecting theoretical aspects has direct/immediate (negative) consequences on the interpretation of numerical analysis (from real-world or synthetic datasets).
  We are sorry that the Discussant felt uncomfortable with the paper structure, which, however, cannot be of the type "methods-data-application-results", characterizing common papers. Papers' structure is not a dogma or an untouchable constraint. It generally varies according to journals, disciplines, and topics. Material organization is just the way we use to reach the aim, that is, communicating a message. Therefore, it should be chosen and adapted according to the aim and nature of the message we want to deliver, not vice versa.
  Moreover, the nature and structure of the paper are clearly stated in the introduction (L15-57 and L57-66, respectively). Therefore, the readers can be aware of the kind of paper they are going to read.
  Furthermore, our paper discusses technical inconsistencies of existing methods. This means that it explicitly assumes that the interested reader is familiar with the discussed methodologies.
  To summarize, the paper was purposely organized according to its nature and aim.
  In contrast to what stated by the Discussant, we think that the difference between 'a very good scientific paper and a common one' is not the materials' organization, but the rigour of the arguments. Presentation should adapt and follow accordingly.

- "Self-referentiality": This is an interesting topic; we are aware about the use, abuse, and misuse of self-references in a large amount of literature over the last twenty years or so. However, we just would like to provide the Discussant with the following food for thought:
    o We cite 20 papers of ours. However, the bibliography includes 107 references, and we are planning to add a few more (from other Authors) in the revised version. Thus, our papers cover less than the 19% of citations.
    o More importantly, references have (or should have) a purpose; thus, the actual point is not how many works of ours we cite, but if the references are appropriate and justified to support our statements. Constructive remarks should be: "these references do not deal with the topic or are inconsistent with the statements, please remove them", or "this topic is not introduced by these references for the first time, please use original references ('this' and 'that')", or again "this is a general concept presented in whatever handbook. Please, avoid citing yourself and use well-established references such as 'this' and 'that'".

Thus, the point is not "elegance" or "quantity" (which are always subjective and relative), but "how" and "why" we use citations. In this respect, we cite our own papers throughout the manuscript in two specific circumstances: (i) when we talk about technicalities concerning NA models because, as far as we know, Serinaldi et al. (2020) is the only paper providing a general/unified picture of NA models and a discussion of their unique/common nature based on the theory of order statistics; and (ii) when we talk about statistical inconsistencies widespread in the hydro-climatic literature and neutral validations. Also in the latter case, as far as we know, there are not many papers attempting independent validations of existing (but questionable) methods in hydro-climatology. We would be happy if more researchers were devoted to this activity, as happens in other disciplines, such as physics and medicine. Unfortunately, we believe this is not the case, and new methods are (too) often applied without the necessary validation of their theoretical foundations. Confusing iterative application with proper independent validation is detrimental for scientific progress, and this concept is one the main messages conveyed by our paper and previous papers of ours cited in the text.

Therefore, focusing on "quality", if the Discussant spotted inappropriate and/or missing citations we would be happy to remove incorrect references and add relevant ones that we may have missed.

That said, we report below our personal policy about referencing/citing our own work:

1) When we write a paper, we mention previous papers of ours only if we think that they can strengthen our message about very specific topics/issues that cannot be found elsewhere with similar flavor/point of view.

2) We avoid citing our own papers when referring to general topics (e.g., EVT theory, order statistics, etc.). There is a huge amount of good books that can (and should) be cited, as they can absolutely be trusted.

3) Our bibliographies are always quite extensive, and our works are always just a small fraction of the total number of cited references.

4) When we act as reviewers and editors of other manuscripts, we mention our own works only to support "rejection", which means that the suggested references are expected not to be cited anywhere. Hardly ever, we suggest our own papers in review/editor reports implying "acceptance/revision". In those very rare cases, no more than 2-3 titles are suggested, and they are usually within a wider list of references from other Authors (thus, just a fraction of the total suggested references).

We hope that the above remarks clarified the rationale behind the paper structure/organization and its relationship with the paper message/aim, as well as our ethical policy concerning the use of references/citations and self-advertising methods.