

Interactive comment on “Effects of seasonality on the distribution of hydrological extremes” by P. Allamano et al.

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Referee 2 is acknowledged for his/her insightful comments. In the following are reported our responses (plain text) to his/her highlighted comments (italic).

Frankly, I am a bit puzzled by this paper, and I think it is much is oversold

We are also a bit puzzled by this review: the tones are rather harsh compared to the relevance of the comments, and some of these comments are not fully supported by objective reasoning. Still, we did our best to take constructively all the reviewer's comments, using these comments to identify the parts of the paper where we have not been clear enough in our explanations.

1) *The investigated problem is not as big a problem as suggested by the authors. It is common in climate and hydrological science to investigate seasons separately - such a model already reduces the violation of the iid assumption made by a stationary model. Just some few examples for such studies are, e.g., Coles and Pericchi, J. Hydrol., 2003; Kendon et al., J. Climate, 2008; Friederichs, Extremes, 2010. Recently, many studies have explicitly accounted for seasonality, e.g., Furrer and Katz, Wat. Res. Res., 2008, Maraun et al., Int. J. Climatol., 2009 in addition to the ones mentioned by the authors. Indeed, there are examples for stationary POT models fitted to data across the whole year - so there might be some justification for the manuscript at hand, but this approach is no longer standard as claimed by the authors (line 25, see also my comment below). So the authors should at least tone down everything.*

We are certainly aware that accounting for seasonality is not new in statistical hydrology (and we will integrate our reference list with the studies suggested by Referee 2). The aim of our study, in fact, is not to propose a new statistical model accounting for seasonality. However, we believe that showing the effects of (neglecting) seasonality could make an interesting contribution to support the hydrological and engineering practice. Very often, in an attempt to preserve model simplicity, data seasonality is not accounted for in design value estimation procedures. Also the lack of data could force researchers to adopt an Annual Maxima approach, as specified below. In our opinion this reviewer may be biased by his/her own experience (and overly optimistic in his/her view of the advancement of the scientific community) if he/she believes this is not an open problem. To support our point, a list of papers published in 2010-2011 where seasonality is not accounted for within a POT approach is reported below. As for the annual maxima method, this Referee will agree that the non-seasonal approach is standard, but still there is no published paper, to the best of our knowledge, clearly demonstrating that neglecting seasonality in an annual maxima approach does not produce severe errors in extreme-value frequency analysis. We will better clarify our point of view in the revised manuscript.

- Cantet, P; Bacro, JN; Arnaud, P (2011), Using a rainfall stochastic generator to detect trends in extreme rainfall, *STOCHASTIC ENVIRONMENTAL RESEARCH AND RISK ASSESSMENT*, 25 (3): 429-441

- Bacova-Mitkova, V; Onderka, M (2010), Analysis of extreme hydrological events on the Danube using the peak over threshold method, *JOURNAL OF HYDROLOGY AND HYDROMECHANICS*, 58 (2): 88-101

- Fruh, B; Feldmann, H; Panitz, HJ; et al. (2010), Determination of Precipitation Return Values in Complex Terrain and Their Evaluation, *JOURNAL OF CLIMATE*, 23 (9): 2257-2274

- Floris, M; D'Alpaos, A; Squarzoni, C; et al. (2010), Recent changes in rainfall characteristics and their influence on thresholds for debris flow triggering in the Dolomitic area of Cortina d'Ampezzo, north-eastern Italian Alps, *NATURAL HAZARDS AND EARTH SYSTEM SCIENCES*, 10 (3): 571-580

- Schindler, N; Tranckner, J; Krebs, P (2010), Extreme value statistics for river quality simulations, *WATER SCIENCE AND TECHNOLOGY*, 61 (2): 397-406

2a) I find figure 2 and the corresponding analysis a bit too exaggerated. The ratio of 7, which appears prominently in the conclusions, occurs only for large lambda 0 and in particular very large alpha. An alpha of 0.9, however, signifies that the amplitude of seasonality is 90% of the average value - something which might be relevant for Monsoon regions, but not in most climatic regions at least in the mid-latitudes.

We agree that this part can be a bit toned down, so we will avoid to explicitly refer to the maximum value assumed by the R_T ratio both here and in the conclusion. This does not imply that our results are less relevant. First because it is untrue that the model is tailored to mid-latitude regions (nothing would prevent, as skeptically suggested by the Referee, to transfer it to Monsoon regions); and second because an $R_T = 3$ (as it is found in the real case, Fig. 6) would already entail a return period underestimation

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of 3 times (i.e. from 100 to 30-year), which of course would be relevant in practice.

2b) *Also the choice of the Anderson-Darling test is a bit unrealistic. The distribution Eq. 6 and an exponential distribution are probably of very similar shape and therefore trivially undistinguishable. Instead, as a first diagnostic one would rather check the empirical annual cycle of threshold exceedances and then easily see whether the annual cycle is strong or not. Such a test would have much stronger power than the proposed test, and would thus help to avoid running into the serious problems discussed by the authors.*

We fully agree with this Referee that the exponential and seasonal distribution may be indistinguishable in some cases, and in fact this was exactly our starting point when tackling this problem (the two distributions are indistinguishable for low return periods but they diverge in the upper tail, producing contrasting estimators of the design values). The Anderson-Darling goodness-of-fit test is especially well suited to recognize deviations in the tails of the hypothetical and operational distributions, and its failure to recognize the difference between the two distributions is precisely a demonstration that there might be a serious problem of model selection in the presence of seasonality. We will better specify this concept in the revised text. As for the suggestion to check the empirical annual cycle of threshold exceedances to recognize seasonality, we disagree that this approach would allow to *easily see whether the annual cycle is strong or not*. Extreme value statistics cannot be based on subjective impressions about the behaviour of some data points on a graph (in this case, the time series of the exceedances), while unsupervised and automatic criteria are needed. Unfortunately, to the best of our knowledge there are no statistical tests of the seasonal behaviour of a time series of exceedances which are powerful enough to outperform the verification procedure we have adopted. For example, standard approaches based on plotting the monthly data with their confidence bands (e.g., Maraun et al. 2009) suffer of the fact that geophysical extremes are typically highly skewed, which implies that the assumption of normality of the average values, which is implicit in confidence band drawing,

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is seldom acceptable. Similar considerations applies to testing procedures based on verifying if the monthly averages are all equal or not (ANOVA approach, see Salas, 1993 or Kottegoda and Rosso, 1997).

3a) *The conclusions regarding the annual maxima approach are probably wrong. On page 4801, lines 15ff, the authors state that the reason for the Gumbel distribution to fit better is that it is more flexible than the exponential distribution. In fact, the annual maxima approach considers only annual maxima - if the seasonality is weak, the annual maxima should be evenly distributed across the year and be iid. If the seasonality is strong, annual maxima should stem only from a specific season - in this season, the iid assumption is satisfied again. Overall, the annual maxima approach effectively ignores seasonality (to the cost of using just one value per year).*

As already suggested by Referee 1 this is a more intuitive explanation for the AM approach performance, that we will add in the revised manuscript. However, according to this reasoning it would be difficult to explain the (low) sensitivity of the AM method also in the presence of out-of-phase regimes of the intensity and rate (i.e. $\delta = \pi$, as in Fig. 4). We believe the distribution flexibility (i.e. to the estimation of two -rather than one- parameters) is responsible for this second(ary) effect. In this sense, we do not agree that our conclusion (“The reason behind this better performance probably lies in the greater flexibility. . .”) is wrong, but we agree that other mechanisms can concur to produce this result. We will clarify this point in the revised manuscript.

3b) *Maraun et al., Int. J. Climatol., 2009, have shown that explicitly accounting for seasonality by a parametric model reduces the uncertainty compared to the annual maxima approach - but the annual maxima approach does not introduce any substantial bias*

Thanks for pointing us to this interesting paper. However, we carefully read the paper mentioned by this Referee, but found no clue about the fact that “the parametric model reduces the estimation uncertainty compared to the annual maxima approach”. In the

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paper there are some comparisons (in Fig. 8) of the 10-year and 100-year extreme rainfalls estimated with different methods, but there are no comparisons of the associated estimation uncertainties. In fact, it would have been surprising that this kind of conclusion was drawn from a data-based analysis, wherein the real design values are unknown and the estimation uncertainty cannot be rigorously evaluated.

4) Part two of the manuscript needs considerable work. Sometimes, motivations are missing (e.g., page 4792, line 24: which integral? Here, equation 6, first part, should be given already), sometimes there is too much technical detail (e.g., the derivation of the parameters of F'). I will give more details below

All the specific points raised by this Referee are addressed below. The more generic suggestions for the style revision will be accounted for if we will have the chance to revise our manuscript.

Minor comments:

- Abstract: "This paper focuses" - this is wrong. It does not focus on seasonality, it exclusively deals with seasonality.

We do not fully get the point of the remark, but we trust this reviewer and will rephrase as suggested.

- p 4790, l 19: the citation of Rust is somewhat misplaced here. This paper deals with extremes, not precipitation in general, and should only be mentioned later.

OK

- p 4791, l 1ff: the discussion of possible solutions is a bit incomplete. The authors do not cite any papers having occurred after 2003. I strongly urge to add the references given above (point 1) and additionally do a thorough literature review.

A list of recent publications is reported above. The most pertinent of these papers will be included as references in the revised manuscript.

*- p 4791, l 11f: the reasoning is wrong. The reason for analysing monthly maxima is that the number of data points is increased 12fold (see also point 3 above). But then, as a drawback, seasonality has to be modelled explicitly - otherwise the iid assumption would not be fulfilled at all, which is exactly the opposite of what the authors claim. Also contrary to the statement of the authors, the increase of available data by a factor of 12 also *decreases* the uncertainty, as long as a parametric model is chosen. If the months are modelled independently, the uncertainty should stay the same (12x as many data points, 12x as many parameters, see also Maraun et al, 2009). All in all, this paragraph needs substantial corrections.*

We disagree. The need for identically distributed variables is the main motivation for extracting seasonal/monthly maxima (see Carter and Challenor, 1981 and Buishand and Demaré (p. 90, l. 9-11, 1990). In fact, the “identically distributed” hypothesis better applies to shorter, within-year, time intervals. Of course the collateral effect of subdividing the year in shorter intervals is an increase in the number of data points, but this does not automatically entail an increase in the information content of the available sample. In fact, extreme value analyses are aimed at representing the upper tails of a distribution, and the 11 additional values per year added to the sample when using monthly maxima will all be lower than the annual maximum, and will therefore provide few information about the behaviour of the upper tail. In a POT framework, it has been demonstrated that a 5-fold increase of the data induces a (less than) 1.5-fold reduction in the estimation error, which in turn corresponds to increasing the AM sample size of less than 1.5 times (e.g., Madsen et al., 1997; Martins and Stedinger, 2001). 100 POT (or monthly maxima) data are therefore far less informative than 100 AM for design event estimation. This reduces the possible favourable effect on uncertainty of using a parametric model with less than 12x parameters. Moreover, the use of a parametric model introduces the need for assessing the uncertainty in the model parameters estimation. It is not easy to demonstrate that the overall uncertainty reduces in this case.

- p 4791, l 14ff: *I don't see the point regarding data availability. If annual maxima are available, they are usually taken from a series of daily values - so also monthly maxima should be available...*

This is not the case. For example, flood data (we are referring to instantaneous discharge measurements, not to daily data) before the 70s are often available as annual maxima, while the continuous series are not available.

- *...In fact, the author's argument contradicts their reasoning regarding the POT approach. How should one apply the POT if only annual maxima are available?...*

Here the reviewer is right, our writing seems to suggest that POT does not suffer of the data availability problem. We will amend this in the revised manuscript.

- *... Also (line 20) it is wrong that POT considers seasonal or monthly maxima. POT considers threshold excesses and therefore in general different data than maxima!*

Right again, the error is the result of an incautious rephrasing in the pre-submission phase. We will amend it.

- p 4791, l 25ff: *"standard approaches". These papers are extremely old! This is no longer standard - update the references please (see comment above)*

OK, see above

- p 4792, l 5; *indicate that lambda is a function of time, otherwise the process would be homogeneous*

OK

- p 4792, l 24: *which integral? Please state the first part of equation 6 here.*

The Bayes integral. We will better clarify this point.

- p 4793, l 5ff: *the derivation of the parameters is just a technical issue. Eq. 4 could be deleted, the text shortened. It distracts the reader from the actual analysis.*

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OK, we will consider to simplify this part in the revised manuscript or to move this paragraph.

- p 4794, l 1: *on the other hand, a motivation why the resulting integral contains a modified Bessel function would be helpful, the same holds for the mean of the distribution. Is this all calculated in Abramowitz and Stegun? Then state this, otherwise give references!*

Abramovitz and Stegun provide the reference for the modified Bessel function, which is part of the analytical solution of the Bayes integral.

- p 4794, l 13: *in the Coles book, this is shown for the extremal case (i.e. GEV, GP distributions) without seasonality - is it clear that these relationships can be transferred to the case at hand? Motivate this please.*

Right. The applicability of the relationships to the case at hand can be demonstrated but cannot be found in the Coles book. We decided to skip the description of this technical part for the sake of simplicity, but we will consider to add an appendix on this in the revised manuscript.

The distribution of the annual maxima from a non-homogeneous Poisson process can be determined with the standard relation (Bayes theorem)

$$F_{AM}(x) = \sum_{m=0}^{\infty} f_M(m) \cdot F_X(x|m) \quad (1)$$

where m is the number of events per year, $f_M(m)$ is its (discrete-valued) pdf, and $F_X(x|m)$ is the cumulative distribution of the maximum of m non identically distributed random variables, conditional on m . $F_X(x|m)$ can be determined taking the product of m different distributions with a form $F_X(x|t_i)$ (see Eq. 3 of the paper) where t_i is the time of occurrence of the i -th event, and integrating out the time of occurrence:

$$F_X(x|m) = \int dt_1 \int dt_2 \dots \int dt_k \prod_{i=1}^m F_X(x|t_i) f_T(t_1, t_2, \dots, t_m) \quad (2)$$

where $f_T(t_1, t_2, \dots, t_m)$ is the joint probability of the times of occurrences. Since the events are mutually independent in a Poisson setting, the joint distribution factorizes, i.e. $f_T(t_1, t_2, \dots, t_m) = \prod_{i=1}^m k(t_i)$, with $k(t_i)$ (as in Eq. 2 of the paper). The integrals in Eq. (2) can thus be sequentially solved for each t_i (as in Eq. 6 of the paper), obtaining

$$F_X(x|m) = [F(x)]^m \quad (3)$$

Since $f_M(m)$ is a Poisson distribution with parameter λ_0 even in a non-homogeneous setting (e.g. Ross, 1996), one obtains for Eq. (1)

$$F_{AM}(x) = \prod_{i=1}^m \exp[-\lambda_0] \frac{\lambda_0^m}{m!} [F(x)]^m = \exp[\lambda_0(1 - F(x))] \quad (4)$$

- p 4795, l 21f: the structure is misleading. it should be stated that "it has a return period T^* , given by $\lambda_0 T^* = 1/(1-F(XT(POT)))$, which is obtained by..."

OK

- p 4801 l 1ff: the conclusions should be rewritten following my comments above, especially the points about the 7-fold overestimation, the Anderson Darling test and the explanation why the annual maxima approach is better.

Specific comments to these points are reported above.

All in all, the manuscript requires substantial revisions. If published, it should be written in form of a cautionary note focussing on the POT approach. Therefore, one could even

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think of deleting the case study and the part on the annual maxima (although I leave this decision to the authors and the editor).

We do not fully understand the reason behind this suggestion. We gave ample explanation of the need to consider both the AM and the POT approaches, for the widespread use of AM estimation models and the non-trivial (though relieving) conclusions about seasonal effects on AM models. The case study demonstrates –again– that in the POT case results can differ by a factor of 3.

References:

- Kottegoda, N. and Rosso, R.: Statistics, probability, and reliability for civil and environmental engineers, McGraw-Hill, International Edition, 1998.
- Madsen, H., P. F. Rasmussen, and D. Rosbjerg, Comparison of annual maximum series and partial duration series methods for modeling extreme hydrologic events: 1. At-site modeling, *Water Resour. Res.*, 33(4), 747– 757, 1997
- Martins, E. S., and J. R. Stedinger, Generalized maximum likelihood Pareto-Poisson estimators for partial duration series, *Water Resour. Res.*, 37(10), 2551– 2557, 2001.
- Ross, S.: Stochastic Processes, John Wiley, New York, 1996.
- Salas, J.D., Analysis and Modeling of Hydrologic Time Series, Chapter 19 (72 p.) in *The McGraw Hill Handbook of Hydrology*, D.R. Maidment, Editor, 1993.

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