



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

### **Anonymous Referee #2**

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Report for "Effects of seasonality on the distribution of hydrological extremes" by Allamano et al.

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

Apart from several minor comments listed below, I would like to raise the following four points:

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;

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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.

2. 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. 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 excesses 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.

3. 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). 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

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annual maxima approach does not introduce any substantial bias.

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.

Minor comments:

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

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.

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.

p 4791, l 11f: the reasoning is wrong. The reason for analysing monthly maxima is that the 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.

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 dail values - so also monthly maxima should be available. In fact, the author's argument contradicts their resoning regarding the POT approach. How should one apply the POT if only annual maxima are avail-

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able? 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!

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

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

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

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.

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!

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.

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..."

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.

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

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