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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Simple and approximate upper-limit estimation of future precipitation return-values" *by* Rasmus E. Benestad et al.

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Benestad et al. present a methodology for statistical-empirical downscaling of precipitation time series to estimate upper limits for future return levels. The proposed methodology provides a considerable alternative in cases where more explicit estimates are not available. In general, the manuscript is carefully written and accompanied by very detailed supplementary material. In fact, my impression is that in some cases, the reader finds relevant information regarding the motivation and methodological details only in this supplement, and one might discuss if some of the supplementary material might fit better into the main paper.

In general, I recommend publication of this manuscript in NHESS after certain revisions have been made. Below, I provide a list of specific recommendations that the authors

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Discussion paper

might wish to consider when preparing their final manuscript.

1. I acknowledge that the authors use well-studied data from the CMIP5 ensemble. In the context of the present work dealing with estimating future return levels, it would be advantageous if the authors could briefly summarize some information on potential known biases (if there are any) of the considered projections.

2. Referring to the statement that "heavy precipitation will become more severe in already wet areas in the future " (p.1, ll.25-26), I was wondering if this holds globally in all such regions.

3. The proposed method relies on inferred statistical relationships between different climate variables. A few more words on possible limitations of these relationships (from both physical principles and empirical observations) would be useful.

4. The authors state several times that their estimates provide upper limits. From the presented material, I did not fully understand why this is the case. The argument seems to refer to the relationship between the variables used for empirical-statistical downscaling; however, the results would only be an upper limit if other (unconsidered) covariates would have exclusively opposite effects and could not enhance the considered relationship. Is it possible to rule out (from physical principles) possible "positive interferences" between different variables possibly influencing precipitation?

5. The proposed method is based on an exponential distribution of 24-hours precipitation sums, whereas I would naively expect a gamma distribution being a more common statistical model (even though the simple scaling from the behavior of the mean to that of arbitrary quantiles would not apply anymore in such case). I would be interested in some additional details on why the exponential distribution is justified here.

6. The choice of the reference region in the North Atlantic appears to be motivated by general climatological considerations rather than statistical optimization. Could the results of the empirical-statistical downscaling be further improved by explicitly seek-

ing for the strongest statistical relationships between predictor and predictand fields? Specifically, as the authors recognize, their predictions are not very convincing in regions with complex orography – could this be because the predictors are not appropriately chosen for these locations in terms of their geographical spread? Can the considered relationship be assumed to be essentially homogeneous over entire Europe?

7. In Eq. (1), is the considered noise term white or serially correlated?

8. The PCA in Section 2.4 predetermines mutually orthogonal annual cycle shapes in PC1 and PC2. It is not clear if and why this is desirable in the present case. Specifically, what the authors consider here in terms of the coefficients of PC1 (PC2) is closely related to the phase of the annual cycle, since both components essentially generalize the role of sine and cosine functions in case of a fully harmonic oscillation (PCA is commonly based on normalized time series, so amplitudes do not matter that much). It might be useful to directly refer to some corresponding phase variable to parameterize the shape of the seasonal cycle for each considered location.

9. In a few figures (in both main manuscript and supplementary material), axis labels and labels/units to color bars are missing. This should be carefully revised. In Fig. 3, it is not clear if the inset shows relative or absolute changes.

Technical comments:

* p.3, l.3: "Our approach. . ." would rather call for using present tense.

* p.4, l.18: mathematical symbol missing after "referred to as"

* p.4, ll.25: "the ration between explained variation. . . and the total variation. . ."

* p.4, l.25: "var() with the noise term is taken to be zero" is not quite understandable, please rephrase

* p.4, l.25: "Principal component analysis"

* p.7, l.13: “constant value of the. . .”

* p.8, l.1: “dependency. . . on temperature”

There are also a few typos in the supplementary material that are not listed here for brevity. In general, in some of the R outputs embedded in the SM text, the meaning of the individual variables is not fully clear without consulting the full R code; at least identifying the variables in the corresponding text boxes would facilitate the reading. Also, I did not find a caption for Fig. SM14.

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