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

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We are grateful for the comment regarding a potential confusion concerning probable maximum precipitation, which is different to the maximum systematic effect that a temperature change may have on the precipitation. The use of an upper limit, however, was inspired from use in physics where problem solving sometimes involves the estimation of upper and lower limits if the most likely estimate is difficult to derive.

The Greek letter μ was used to represent the wet-day mean precipitation which is also related to the parameter of the exponential distribution f(X)= λ exp(-λx), where λ=1/μ. The best fit to this distribution is sought for various data samples, such as on an annual or monthly basis. Hence, there will be different estimates of μ for different calendar months. This is one of the new aspects of this strategy, and we are grateful for the

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reviewer pointing out how this may cause confusion. We will try to explain this more carefully in a revised version.

Another aspect is the relationship between μ and percentiles; it is easy to show mathematically that for the exponential distribution, any percentile can be written $q_p = -\ln(1-p)\,\mu$ (this expression is derived in http://dx.doi.org/10.3402/tellusa.v67.25954). We think that the use of three different symbols for the same quantity would be more confusing than using one, as the Greek letter μ refers to the wet-day mean precipitation for all these instances.

We disagree with the comment of the strategy being ad-hoc: with climate change, we must expect a change in the probability distribution function (pdf) and that changes in the tail of the pdf must follow the change in the bulk of the pdf as the area under its curve must equal to one by definition. The use of an exponential distribution does not give a precise representation of the tail of the distribution, but since it only has one parameter, it gives a constrained description of the change in the bulk part of the probabilities.

We agree that GEV would be a better choice for describing extremes for a stationary variable, but it is more questionable if it is the best method to quantify changes in the extremes for a non-stationary situation with short data records. Hence, there is a trade-off between using the less precise but more robust one-parameter pdf that takes changes in the entire pdf into account or the three-parameter GEV that only considers the tail of the distribution for fitting.

We used the exponential distribution to quantify the percentile for highest annual precipitation, which is not so far out in the tail. This percentile is then used together with a 95-percentile (1-in-20 year) of the annual aggregate for the wet-day mean precipitation μ to estimate the 20-year return value, inspired by Bayes equation. However, we realise that this must be explained more carefully to avoid confusion.

Our method does indeed assume that the wet-day frequency is not changing, and this is of course debatable. Analysis of past trends suggest that there has been mixed long-term changes (Benestad et al., 2016; ERL-102170.R2 - supporting material).

The upper bound is taken to be the assumption that all of the mean seasonal variability in μ is due to the seasonal variability in the temperature in parts of the North Atlantic which is a likely source of the atmospheric moisture.

This paper uses both physics and statistics, and there are sometimes different conventions in these two disciplines. We are not pretending that this paper is a pure statistics-study, but try to draw on common practices from statistics as best as possible, but not to the extent that it interferes with common practices in physics. We are also aware of past criticism that statisticians have made about physics and vice versa. We think that our use of "robust" meets both that of statistics (our estimates are not sensitive to outliers) and physics (they rely on a high signal-to-noise ratio since we make use of the mean seasonal cycle). This will be explained more carefully in a revised version.

We are aware of the fact that a multi-model ensemble like CMIP5 is not ideal, but is in reality "an ensemble of opportunity". We also acknowledge that sampling strategy is key to statistical reasoning, however, this is not such a critical point in this work. We do not use the ensemble to assess model-related uncertainty, but we find that the ensemble spread corresponds well with the interannual variability. We choose to adopt a common practice in physics, to be practical and make use of what information that is available. We choose to make use of a rough "back-of-the-envelope" approximation to be able to get a reasonable - although not perfect - estimate of the largest effect a temperature increase may have on extreme precipitation.

We appreciate the specific comments. To be rephrased: number of loss events, related to weather. Can be rephrased to 'has been', but still with a larger ensemble, such as Euro-CORDEX, it is much smaller than for the ensembles used in ESD as done here with hundred simulations for the most popular emission scenarios. This is more a physics way of addressing a problem. NCEP/NCAR 1 is used because it has longer data records and it has some similarities to a GCM being output from an atmospheric model. The predictor is the area mean temperature and such an aggregated quantity reduces the effect of a bias. The precise description is given in the R-markdown in the supporting material. The ensemble is used to gauge the interannual variability to estimate a typical 1-in-20 year warm annual wet-day mean precipitation μ. We will explain this more carefully. We are not sure what is the problem here. Pr(x >0) = fw, which we expect. Pr(X=0) = 1- fw. The threshold is a bit arbitrary, but it was chosen to highlight the locations with a good match between the mean seasonal variations in μ and in the temperature over the North Atlantic. This is a matter of debate, and I hope that our paper can be considered as one contribution. We make use of conditional probabilities and the assumption that all of the mean seasonal variations in μ are due to the mean seasonal variations in the temperature over the North Atlantic. Comparison with independent data give some support for this (supporting material). Good question. The outer rims indicate more geographical variance, and are probably subject to stronger statistical fluctuations connected to the stronger model response. These may depend on local geographical conditions, and since the results are based on rain gauge data, differences may be due to instrumentation and surrounding obstacles. Here, robust refers to the use of a "cleaner" signal compared to noise, in the use of the mean seasonal cycle. It also downplays the effect of outliers in terms of single events. The paper does indeed try to quantify a "worst-case" estimate based on the assumption that all of the mean seasonal cycle in μ that matches the mean seasonal cycle in the temperature is due to the change in temperature. This needs to be explained more carefully. Atmospheric rivers are phenomena taking place in the upper part of the troposphere. They transport humidity from low latitude regions and cause heavy precipitation at higher latitudes. They may of course be part of the equation here, but we do not see any reason why they should play a special role and we are not aware of a direct effect between these and the coasts. This is something that should be looked into further, but is outside the scope of our paper. This paper is more from a physics approach, and we disagree that anything that is not statistical is "ad-hoc". GEV-based work is pure statistics, but our strategy made use of the information contained in the mean seasonal cycle and an upper-limit estimate based on a physics problem solving strategy that has been part of the physics training at University of Manchester Institute of Science and Technology (UMIST).

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