Authors' response to Referee #2

For clarity, authors' responses are presented by blue colour.

We have answered all the comments of the reviewer 2. Answers are attached to this revision note. Along with the answers we are also explaining all the changes we have done.

Review of "Exploring the Long-Term Reanalysis of Precipitation and the Contribution of Bias Correction to the Reduction of Uncertainty over South Korea: A Composite Gamma-Pareto Distribution Approach to the Bias Correction" by Kim et al.

The authors present and evaluate a bias correction of the ECMWF ERA-20c reanalysis for South Korea. The correction is based on a parametric quantile mapping and calibrated between reanalysis grid-box and observed station precipitation, and extended to the full field by interpolating the transfer function parameters in space.

I cannot recommend this manuscript for publication. At least major parts should be substantially revised, and the spatial model should be fully omitted. My major concerns are as follows:

1. Deterministic bias correction of precipitation cannot be used for downscaling, and in particular not to create spatial fields. Maraun (2013) has demonstrated that bias correction suffers from the same conceptual flaw as inflated regression. Differences between reanalysis and station observations (in particular the magnitudes of summer extremes and wet day frequencies) are not necessarily biases, but to a substantial degree due to the scale gap between the area average of the reanalysis and the pointscale of the observations. Local-scale variability is not fully determined by the grid box average, a deterministic rescaling as done by quantile mapping cannot create the missing local variability. Instead the large-scale variability is inflated. Thus, the corrected time series have similar marginal properties as the local observations, but do not have the correct spatial-temporal properties. This is a problem in particular for spatial fields, as the spatial distribution of the corrected field is still that of the reanalysis (apart from the wet-day correction), but only inflated. It does not represent the smallscale variability of summer thunderstorms, e.g. The problem is severe for extreme events: dry areas as well as the magnitude of precipitation falling over a certain area are substantially overestimated (Maraun 2013). Thus, using these data for hydrological modeling would likely result in dangerously misleading results. This issue is rather irritating, given that the authors cite Volosciuk et al. (2017) who discuss this issue in depth. In fact, the only correct solution would be a stochastic bias correction that bias corrects (if needed at all in this case) and

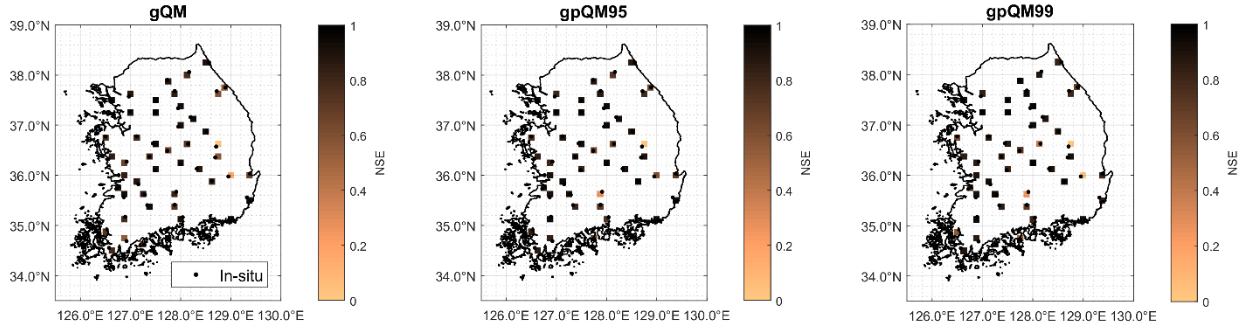
additionally adds random small-scale variability (either in a single-site approach as suggested by Volosciuk et al. (2013) or with a fully spatial model. If only single locations are considered (without using time series at multiple sites), a quantile mapping to the point-scale might be justified based on pragmatic reasoning.

A major problem of the manuscript is that the evaluation is essentially blind to these problems. They are mostly visible in the spatial and interannual variability. None of these aspects have been evaluated.

(**Response**) Thank you for the comments and the explanations. We agree that there may exist the spatial bias between local station observation and gridded reanalysis, and the bias corrected values could misrepresent spatial-temporal pattern. However, a primary objective of this study is to statistically extend the sample size, especially for extreme values, in a certain area with spatio-temporally sparse observation network. Therefore, specific day-to-day variation or trend analysis was not our main concern in this study. In these perspectives, we further investigated the IM-PCM method within a leave-one-out cross validation framework.

First, we evaluated the IM-PCM method by employing a leave-one-out cross validation framework over 48 weather stations for the reference period and the overall error estimation results were described in the manuscript for both the extreme and mean. For a more specific analysis in each weather station in the context of cross validation, we generated a map showing the spatial errors in both annual maximum series (AMS) rainfalls and mean. The AMS errors were illustrated by root-mean-square-error (RMSE) and Nash-Sutcliffe efficiency (NSE) in Figure S1. For the mean, we further evaluated the IM-PCM method by estimating the relative error between the observed and modelled in Figure S2. As shown in the figures, for the AMS rainfalls, gpQM95 and gpQM99 generally perform well except for a few stations. Most stations showed NSE over 0.8 and RMSE less than 30mm. For the mean daily rainfall, the relative errors are generally below 10%.

(a)



(b)

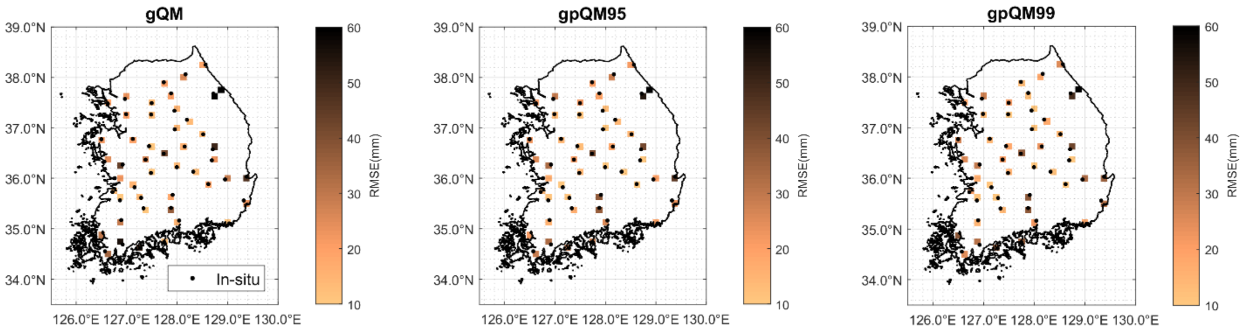


Figure S1. Cross validation results of the IM-PCM for the annual maximum series rainfall of the bias corrected data by QM approaches (gQM, gpQM95 and gpQM99) over 48 grid points. (a) Nash-Sutcliffe efficiency (NSE) and (b) root-mean-square-error (RMSE).

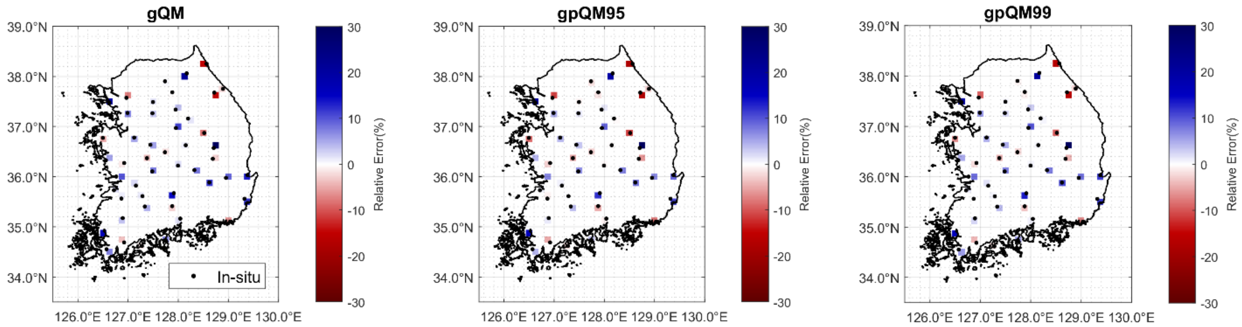


Figure S2.Relative error of the bias-corrected mean rainfall by QM approaches (gQM, gpQM95 and gpQM99) in 48 grid points compared with the corresponding in-situs.

Second, one generally collects annual maximum rainfall series or extreme values over a certain threshold from historical records to derive Intensity-Duration-Frequency (IDF) relationships. In many regions including South Korea, the long-term meteorological record for a given catchment is largely limited to evaluate the reliable IDF relationships. Thus, the uncertainty of the estimated design rainfalls could be affected by uncertainty associated with the sampling error (Coles et al., 2003; Huard et al., 2010; Overeem et al., 2008; Tung and Wong, 2014; Van de Vyver, 2015). We further explored the uncertainty range of design rainfalls based on GEV distribution for a given return period and a given data length within a Bayesian modelling framework. As illustrated in Figure S3, the uncertainty range of design rainfall is significantly reduced with increasing data.

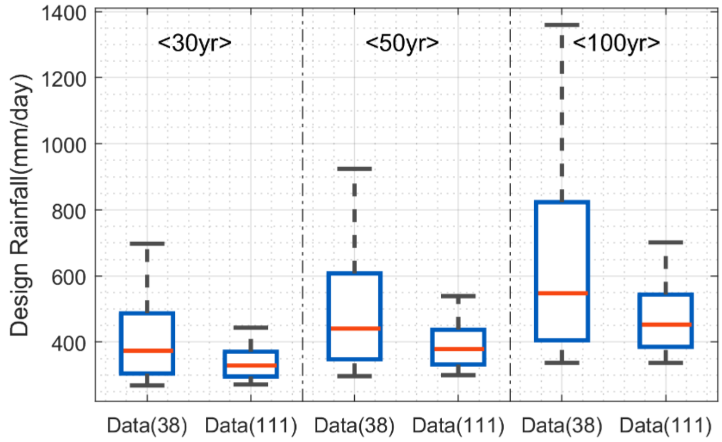


Figure S3. Boxplot for the uncertainty range of design rainfalls with 30-yr, 50-yr and 100-yr return period based on GEV distribution for 38 annual maximum series (AMS) (Data(38)) and 111 AMS data (Data(111)).

2. The discussion in the manuscript is rather naive and largely ignores problems of bias correction and reanalysis data. It also ignores much of the literature in the field. For instance, it is well known that at least the first versions of century-long reanalysis data strongly misrepresent long-term climatic trends, or that synoptic-scale variability in the Tropics is only weakly constraint in reanalysis data (Krueger et al, 2013; Befort et al., 2016; Brands et al., 2012). These issues are not discussed in the manuscript. Similarly, the downscaling issues discussed above have not been acknowledged, differences between biases and scale-gaps in the given example have not been discussed. In fact, the authors do not make any attempt to discuss which kind of biases can be corrected in their context. E.g., misrepresented long-term trends, spatial-temporal variability (apart from wet-day corrections) or a misrepresented tropical day-to-day variability will not be corrected by the bias correction. See, e.g., Maraun et al., 2017, for a discussion of several issues (many are relevant in a climate change context, but some apply also here).

(**Response**) Thank you for the valuable comments. As aforementioned above, this study aims to explore a composite distribution based bias correction approach for century-long ERA-20c daily precipitation data which could be possibly used to reduce the uncertainty associated with sampling error in rainfall frequency analysis. Thus, other issues related to the bias correction for daily rainfall series have not been fully considered. We agree that those discussions are valuable for this study, and the discussion has been included in the revised manuscript as follows:

"(Page 3, Line 19) However, although substantial improvements have been made in the modelling process, previous studies have shown that reanalysis datasets still have their own systematic errors which vary in space and time (Bao and Zhang, 2013; Bosilovich et al., 2008; Gao et al., 2016; Kim and Han, 2018; Ma et al., 2009) It is also clear that century-long reanalysis data may misrepresent long-term climatic trends or synoptic scale variability, especially for the first half of twentieth century, and there exists the difference in temporal variability between century-long reanalyses (Befort et al., 2016; Brands et al., 2012; Donat et al., 2016; Krueger et al., 2013; Poli et al., 2013)."

"(Page 21, Line 17) One major issue in QM approach is that the bias corrected values could suffer from inflation of the bias corrected value or implausible representation of temporal pattern (Bum Kim et al., 2016; Maraun, 2013, 2016; Maraun et al., 2017; Volosciuk et al., 2017; Vrac and Friederichs, 2015). However, as this study aims to statistically extend the sample size, especially for extreme values associated with the sampling error in rainfall frequency analysis, in a certain area with spatio-temporally sparse observation network, specific day-to-day variation or trend analysis has not been fully considered. One generally derives Intensity-Duration-Frequency (IDF) relationships for rainfall intensity analysis, but in many regions including South Korea, the long-term meteorological record for a given catchment is largely limited. Thus, the uncertainty of the estimated design rainfalls could be affected by uncertainty associated with the sampling error (Coles et al., 2003; Huard et al., 2010; Overeem et al., 2008; Tung and Wong, 2014; Van de Vyver, 2015). I.e., the more the reliable sample data, the less uncertainty exists in the estimated rainfall depth. In these perspectives, the bias correction methods developed in this study both statistically improved the quality of the data and extended daily precipitation over the 20th century in South Korea."

3. The language needs substantial revision, as well as the logic within several sentences. I will give some examples below.

(**Response**) We have carefully revised the manuscript. The English structure and grammar of the manuscript has been thoroughly reviewed through a specialized English editing office for proof reading. Thanks for the constructive comments again.

Further comments:

p2|11: this sentence makes no sense and does not logically link to the previous sentence.

(**Response**) In this paragraph, we first described the influence of climate change on a wide range of fields and moved the focus into the impact on water related hazards. We have modified this paragraph as follows:

"Recent studies have documented that long-term climate change has influenced a wide range of fields such as agriculture, environment, health, economy and water resources (IPCC, 2014; Nelson et al., 2009; Patz et al., 2005; Vörösmarty et al., 2000). An increase or decrease in climate variables such as precipitation and temperature can affect the growth of crops, ecosystem, human diseases, and water-related hazards such as floods and droughts. Of these impacts, water-related hazards are closely linked to changes in rainfall intensity, which are of primary concern to water resource managers.

p2|16: the data are not just coarsely represented in model calibration, they are simply coarse.

(**Response**) This sentence has been changed as follows:

"However, it has been widely acknowledged that the observed data are coarse in space, and long-term climate data are not readily available in many countries around the world."

p2|23: what does "finer" refer to? Or should it be just "fine"? In any case I would not agree that reanalysis are provided at a fine resolution. What is more important is that they provide a complete field.

(**Response**) The term 'finer' was used to emphasize the spatial aspects of reanalysis data compared with observation, because the reanalysis can provide spatial information for a variety of climate variables. To be clearer, this sentence has been changed as follows:

"A primary strength of the reanalysis data is that compared with observation, they provide spatial information with a longer period, a few of which can cover the whole 20th century."

p3|2: "spans from" English!

(**Response**) "spans from" has been changed into "spanning from".

p3|11-13: this does not make sense. If pressure and wind are not assimilated, how can the synoptic situation then be represented?

(**Response**) For ERA-20cm, no atmospheric observations were assimilated (Hersbach et al., 2015). For this reason, ERA-20cm is not able to reproduce actual synoptic situations as described in the manuscript, but the ensemble can provide a statistical estimate of the climate. The detailed on ERA-20cm is found in Hersbach et al. (2015).

p3|14: what does "on the other hand" refer to?

(**Response**) This was used to distinguish descriptions for NOAA-20cR, which followed after this phrase, from the products from the ECMWF (ERA-20c and ERA-20cm) described before this phrase.

p3: here the limitations of the reanalysis data should be discussed.

(**Response**) As aforementioned above, this study aims to statistically extend the sample size, especially for extreme values, in a certain area with spatio-temporally sparse observation network. In this vein, we have not fully described the limitations of the reanalysis. We agree that those discussions are valuable for this study, so the limitations has been included in the revised manuscript as follows:

"(Page 3, Line 19) However, although substantial improvements have been made in the modeling process, previous studies have shown that reanalysis datasets still have their own systematic errors which vary in space and time (Bao and Zhang, 2013; Bosilovich et al., 2008; Gao et al., 2016; <u>Kim and Han, 2018</u>; Ma et al., 2009) <u>It is also clear that century-long reanalysis data may misrepresent long-term climatic trends or synoptic scale variability, especially for the first half of twentieth century, and there exists the difference in temporal variability between century-long reanalyses (Befort et al., 2016; Brands et al., 2012; Donat et al., 2016; Krueger et al., 2013; Poli et al., 2013)".</u>

p4|9: there is a more recent review by Maraun (2016) and the recent book by Maraun and Widmann (2018). Also the selection of methods is rather arbitrary.

(**Response**) Thank you for the references. The literature review on the previous studies for bias correction has been included as follows:

"The underlying concepts for the bias correction approach vary from <u>a simple</u> <u>delta change or mean bias correction to a quantile mapping (QM) or a multivariate approach based on copula-</u>

based technique (Laux et al., 2011; Mao et al., 2015; Maraun, 2016; Maraun and Widmann, 2018; Teutschbein and Seibert, 2012)."

p4|13: bias correction cannot reduce errors in numerical models! It can, at best, postprocess numerical models.

(**Response**) We have changed the sentences as follows:
"Although each method has its own merits and limitations, previous studies have shown that bias correction methods were generally capable of reducing systematic errors of numerical model outputs and, among them, QM showed better performance than other approaches, especially for precipitation."

p4|14: "Jacob Themessl et al" should be "Themessl et al.". The name is Matthias J. Themessl.

(**Response**) We have changed the name of the citation.

p4|15 "referred to as other names" grammar!

(**Response**) We have changed "referred to as other names such as 'distribution mapping' and 'probability mapping' " to "referred to as 'distribution mapping' or 'probability mapping' ".

p4|18 "usually based on a gamma". No - this is not true. There are many other implementations, and often non-parametric approaches are used.

(**Response**) We agree that there are various transfer functions from empirical CDF to a composite distribution in quantile mapping for precipitation. As the proposed methodology was contrived to interpolate transfer function parameters, we did not explore the non-parametric approach in this study. Among parametric QMs, the gamma distribution is routinely adopted for monotonous fitting, while current studies have employed a composite or mixture distribution for improving extreme values more effectively as described in the manuscript. In this context, we have changed the sentence as follows:
"The QM method, referred to as 'distribution mapping' or 'probability mapping', was used to rectify the cumulative distribution of the modelled data against that of the observed data by employing a transfer function. To allow for interpolation in space and extrapolation in time,

the parametric QM-approach is considered in this study. For daily precipitation, a gamma distribution is commonly adopted in parametric fitting."

p5|1: "underestimation" Not necessarily. In particular moderate extremes might be overestimated (in the range where the scale parameter dominates).

(**Response**) We have changed the sentence as follows:
"In other words, the gQM approach may result in misrepresentation of the upper tail of the distribution, which, in turn, can lead to underestimation of the design rainfalls."

p5|13 and following: as discussed above, this approach is not sensible, at least not for a deterministic method which is interpreted at multiple sites.

(**Response**) As aforementioned in response to 1., a primary objective of this study is to statistically extend the sample size, especially for extreme values, in a certain area with spatio-temporally sparse observation network. Although there still exist some biases in the IM-PCM method, the uncertainty range of design rainfall might be significantly reduced with increasing data. In these perspectives, the suggested approach is still meaningful.

p12|12-16: this listing is a bit naive. The GEV is designed to model block maxima. It may fit a distribution tail rather well because it is flexible (3 parameters), but conceptually this doesn't make sense. Here some discussion should be added.

(**Response**) To identify the best marginal distribution, we have considered GPD, GEV, Gumbel, Weibull and Lognormal which have been commonly adopted in rainfall frequency analysis. As you indicated, we agree that the use of GEV distribution cannot be justified for the composite distribution. Thus, we removed GEV distribution. Thanks for the comments.

p13, eq. (3): this model is a bit crude. There are many implementations that ensure at least continuity at the transition point between gamma and GPD, some even smooth-ness. The method here essentially has a jump.

(**Response**) Thanks for the comments. We pragmatically adopted a composite of two distributions because the non-continuity over the threshold does not affect the overall bias results.

p14|2: "mainly" well, what other reason should there be?

(**Response**) The term "mainly" has been omitted.

Section 3.3: as discussed, this is extremely dangerous and should not be done.

(**Response**)

As aforementioned in the response to 1., a primary objective of this study is to statistically extend the sample size, especially for extreme values, in a certain area with spatio-temporally sparse observation network. Although there still exist some biases in the IM-PCM method, the uncertainty range of design rainfall could be significantly reduced with increasing data. In these perspectives, the suggested approach is still meaningful.

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