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Interactive comment on "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* Dong-Ik Kim et al.

Anonymous Referee #2

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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

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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 reanalyis 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 point-scale 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 small-scale 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.

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).

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

Further comments:

p2 l11: this sentence makes no sense and does not logically link to the previous sentence.

p2 l16: the data are not just coarsely represented in model calibration, they are simply coarse.

p2 l23: 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.

p3 l2: "spans from" English!

p3 l11-13: this does not make sense. If pressure and wind are not assimilated, how can the synoptic situation then be represented?

p3 l14: what does "on the other hand" refer to?

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

p4 l9: 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.

p4 l13: bias correction cannot reduce errors in numerical models! It can, at best, postprocess numerical models.

p4 l14: "Jacob Themessl et al" should be "Themessl et al.". The name is Matthias J. Themessl.

p4 l15 "referred to as other names" grammar!

p4 l18 "usually based on a gamma". No - this is not true. There are many other implementations, and often non-parametric approaches are used.

p5 l1: "underestimation" Not necessarily. In particular moderate extremes might be overestimated (in the range where the scale parameter dominates).

p5 l13 and following: as discussed above, this approach is not sensible, at least not for a deterministic method which is interpreted at multiple sites.

p12 l12-16: 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.

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.

p14, l2: "mainly" well, what other reason should there be?

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

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