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# *Interactive comment on* "A method to preserve trends in quantile mapping bias correction of climate modeled temperature" *by* Manolis G. Grillakis et al.

**Anonymous Referee #2**

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Review of esd-2017-53 by Grillakis et al.

General Summary:
The authors present a bias correction method that is intended to address the assumption of stationarity in statistical bias correction. The idea behind the presented method is to disaggregate "stationary" and "non-stationary" components of the model-derived time series by the means of a quantile mapping procedure. Then, the "stationary" (or normalised) component is bias corrected, while the un-corrected residuals are added back to the corrected stationary part of the time series. I have reviewed a previous version of the manuscript submitted to ESD (esd-2016-52; Reviewer 1). The authors



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show that this method preserves the trend signal of the original time series but largely reduces biases relative to an observational dataset. I believe that the paper has indeed substantially improved (and appreciate that the authors have taken the revisions seriously), but nonetheless I am still wondering about several potential conceptual and/or technical problems of the presented approach, which are specified below. Given that the idea of separating stationary and non-stationary parts of a time series for bias correction is certainly interesting, I believe it could be beneficial for the paper and the interpretation/understanding of the methodology if the authors would address these in the manuscript.

Major comments:

**1. Invasiveness of post-processing methods  methodological test in light of previous literature** While the idea of separating stationary and non-stationary parts of a time series for bias correction is interesting, I am still wondering about potential side-effects of the method. The NSM module fits a transfer function to each year of a grid cell based time series, hence the methodology implies a large number of parameters to be estimated (and thus statistical degrees of freedom); I am not sure which potential side-effects this could imply. Hence, I firstly would encourage the authors to discuss potential side effects, and potential disadvantages of a highly invasive method in comparison to simpler methods; for example, what is the advantage of the NSM+quantile mapping in comparison to a case in which one would simply remove the trend prior to quantile mapping (e.g. Cannon et al., *Journal of Climate* **28**, 6938-6959, 2015, among others)? How much are the results different to the case where one would subtract a smoothed time series on a moving window of few years (i.e. subtract inter-annual variability directly, instead of a complex procedure?); and only bias-correct the remainder part? In any case, I believe a comparison to other, simpler trend-preserving bias correction methods would be a crucial aspect of the paper. Potentially, an example based on random/artificial data could help to underscore the differences and advantages of

the NSM+bias correction methodology. Moreover, if (specifically) high-frequency variability is corrected towards a reference dataset derived from observations (these obs. datasets could be very noisy at high frequencies, because they are derived from individual sites); I wonder whether there is a certain specific sensitivity of the method to the spatial scale of the observational dataset and its high-frequency noise; i.e. whether the method potentially overcorrects sub-annual variability to noise in observations? (i.e. the so-called inflation problem, see e.g. Maraun, *Journal of Climate* **26**:2137-2143, 2013).

**2. Non-correction of inter-annual variability: Discussion about the concept of stationarity and which components actually could/should be corrected** Furthermore, I am still wondering about the authors' use of "non-stationary" vs. "stationary", where the former term is used for inter-annual and lower frequencies; whereas the latter term is used for sub-annual variations. To my mind, inter-annual variability (that is not corrected by purpose in the proposed method) could well be stationary, but biased. For example, model deficiencies on the inter-annual time scale are well-known and often related to land-atmosphere interactions (e.g. Fischer et al., *Geophysical Research Letters* **39**(19), 2012). The illustrative example presented in Fig. 5c shows that the raw model underestimates inter-annual variability compared to the observations that are (by intention) not corrected in the BC-NSM method. I believe it would be worthwhile to discuss in the manuscript, whether this aspect is indeed desired by a bias correction of "non-stationary" components? To this end, I believe that the authors could evaluate and discuss which kind of variability is being corrected by their method, using for example power spectra. Thereby, one could potentially learn or discuss how the spectrum of temperature variability (e.g. see Huybers and Curry, *Nature* **441**(7091), 2006) produced by a given model is altered by their bias correction methodology (or by other methods).

**3. Generalisation to other variables**

Finally, it would be interesting if the authors could discuss (at some point in the





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manuscript) whether their method is intended for bias correction of temperature only, or whether the method could be extended towards other climatic variables as well?

Minor comments:
p. 6, l. 183-185: If the CDF is split in segments and then the correction of each segment is performed according to Eq. 3, isn't there a possibility that there could be gap changes in the correction at the edges of the segments? p. 7, l. 212-236: I believe the explanation of the split-sample test could be separated from the introduction of the case study area and data in the paper? p. 8, l. 250-252: Is this statement "... some of the bias is attributed to the ability of the observation dataset to represent temperature..." based on evidence or speculation?

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