Manuscript: A method to preserve trends in quantile mapping bias correction of climate modeled temperature

Major remarks

The authors present a bias correction approach for simulated temperatures where this post processing is separated into three steps. Step 1 comprises the separation of the time series in a normalized and the residuals. In Step 2, a quantile mapping based bias correction is applied to the normalized time series, and then the residuals are added again to this bias corrected time series in the third step. In this way, trends and anomalies of the original time series are better preserved than in the case where the bias correction is applied to the full original time series.

The paper clearly presents the new method and its application, but I miss some more information on where does this method stand within general bias correction related research. Given the huge number of papers that have been published on the topic of bias correction within the recent years, the discussion section of the present study needs more comparison to other methods, especially to those that follow similar or related approaches, i.e. those studies that do not only perform quantile mapping based bias correction but also do something more. For example, how does the new method compares to the method of Haerter et al. (2011) who proposed a separation of time scales when applying bias correction, i.e. daily fluctuations are differently corrected than the monthly means. In a way, you are also separating time scales in your approach of separating into a normalized time series and the residuals. How would corrections according to Hempel et al (2013) look different than those obtained in the present study? The authors mentioned the approach of Hempel et al. (2013) in the introduction, but in the moment I don't see how the mentioned conceptual drawback may actually impact the bias corrected time series.

With regard to pros and cons, disadvantages and problems with BC, it is referred to previous literature. Even though I think that this generally ok to do so, I also believe that the conclusions section would profit from a paragraph about issues for which also the new method would not lead to improved results or would even lead to misleading results.

In some parts of the manuscript, the English is difficult to read or uses some unusual terms. I recommend proof reading by a native speaker.

The paper focuses purely on the new methodology and its implications on the corrected time series. Thus, the paper itself is a solid piece of scientific research and worth publishing. But I am wondering whether the authors chose the right journal for this, as I don't see aspects of Earth System Dynamics (ESD) in the paper. As the content might be interesting for climate impact modellers, especially hydrologists, another Copernicus journal such as HESS seems to be much more appropriate than ESD for the publication of the manuscript.

In summary I suggest placing the paper in another Copernicus journal such as HESS, and accepting the paper for publication after some revisions have been conducted.

I don't wish do stay anonymous, Stefan Hagemann

Minor remarks

In the following suggestions for editorial corrections are marked in *Italic*.

p.2 – line 38

… dependency *of* the temperature bias.

With regard to this sentence itself, please see remark to p.3 line 95-96 below.

p.2 – line 40

…the *modelled* reference …

p.2 – line 41-42

… and *preserve the* signal of the *latter*.

p.2 – line 45

… improvements *due to* this method.

p.3 – line 69

… output *provides* the …

p.3 – line 95-96

It is written:

"The procedure however overlooks the time dependency of the biases, i.e the unequal effect of the TF to the varying over time CDF."

This sentence is difficult to read and not clear to me.

The temperature distribution is varying with time, especially when a climate change signal is present. But that does not necessarily mean that the bias is also varying. Especially in climate change applications of bias correction, it is inherently assumed that the bias does not change with time. If the bias would actually be time dependent, an application of the bias correction to future data may be questionable. Hence, please rephrase and explain more thoroughly.

p.4 – line 122

Maraun (2016) *discusses* on …

p.4 – line 125/126

Do you mean: '… which *is not a* common practice." ?

p.4 – line 130

… problem *as individual model trend changes were cancelled out*.

p.5 – line 136

… approach *is* that …

p.6 – line 166

…in *order to* be added *later* again …

p.8 – line 235

…long-term *transient* climate …

p.8 – line 261-263

… in Figure *5, which shows the NM* separation of the raw *data into* residuals and normalized raw data in annual *aggregates*.

p.9 – line 365
… variability *are* contained …

p.10 – line 312
… standard *deviations* of the adjusted …

p.10 – line 321
This study *elaborates the* issue …

p.10 – line 329-331
… evolution *shows* that *it* is better … …residuals *from* the …

p.11 – line 332
… which *comprises* an …

p.11 – line 334-335
Sentence has wrong grammar. Please rewrite!

p.11 – line 336-338
This is a noteworthy feature, but I don't see this as an advantage (or disadvantage).

p.11 – line 339
… performed *on an* annual…

p.11 – line 344-345
This sentence is difficult to read, but its content is also rather trivial. It more or less means: 'The main reason for the quality of a climate model is its skilfulness.' Or in other words: 'A model is good because it is well performing.'

I suggest removing this sentence.

p.11 – line 345-346
On one hand, this is an important statement. On the other hand it should also be stated that in some cases, where the climate model has some problems, e.g. with circulation feature or sub-grid scale processes, the added usefulness may obscure that the bias correction can even deteriorate climate change signals (see also major remarks).

p.19 – Fig. 3
This figure comprises too many panels. As the panels showing the absolute MEAN and STD values of the five RCMs do not add much valuable information, I suggest removing them.

p.21 – Fig. 5
Panel b) is not cited in the caption. In the curve legend below panel b, there is a typo: BC-NSM. This should be corrected to BC-NM.

p.22 – Fig. 6
The panels c, d, g, h do not add valuable information. Please remove. Instead I suggest adding a panel showing the difference of RAW-EOBS.

<u>p.23 – Fig. 7 caption – line 566</u>
…percentiles *on an* annual …