Hydrology and
Earth System
Sciences
Discussions

Interactive comment on "An improved statistical bias correction method that also corrects dry climate models" *by* Fabian Lehner et al.

Anonymous Referee #1

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Firstly, I felt the title of the paper could be improved. The authors are not correcting dry climate models as that requires new rainfall parameterisations. It would be worth thinking carefully what the contributions are and then reflect them in the title. Secondly, I feel the approach presented has significant similarities to quantile delta mapping (QDM) of Cannon et al. (2015) and equidistant CDF matching method (EDCDFm) of Li et al. (2010) as well as to another variant of quantile mapping in the details below. Authors need to really focus on distinguishing their approach against the others, maybe using contrived synthetic examples where advantages can be highlighted, or using real data along the lines they have already done. Furthermore, I am still left with doubts on whether I have understood their approach, as their presentation is not mathematical enough for the reader to be confident. Some work on this aspect is needed.

My detailed comments are (in the order I read the paper): Line 35: I feel the authors need to also acknowledge the papers on correcting systematic biases that have been written with hydrological systems in mind. I am referring to those studies that attempt to correct biases in persistence, which is critical when a sequence (time-series) of inputs are coming from a GCM to drive a hydrological simulation. Some examples are 10.1016/j.jhydrol.2016.04.018, 10.1007/s00382-016-3510-z, 10.1029/2018WR023270, and these are by no means exhaustive so authors should look into other papers as well that have been written with a hydrological application in mind. Line 94: pretty sure both wet and dry day biases are corrected in MBC - 10.1016/j.envsoft.2018.02.010 by resampling additional wet days. Line 110: The statement of no extrapolation is needed needs to be clarified to be in line with the statement in line 180, for the extreme values. Line 159, 185: Is there any consideration for subtracting a linear trend to detrend the time series (Step (1) and Step (6)) rather than any non-linear trend which may contain in the time series? Line 168: What is/are the parameter(s) for the trial and error procedure to be said satisfactorily provide 100 correction values? And how is it relevant with the statement in line 329 which states that the remaining error of the proposed approach due to the defined-100 discrete values? Line 171 and 175: The correction value formulas which are presented are for the simulated-current time series. Will it be applicable to bias correct the simulated-future time series? Should the bias correction procedure of the simulated-future time series following the bias correction procedure of the simulated-current time series? I am just not sure what this does to the definition of a CDF, as I can see cases where the corrected value exceeds a limit of 1. Perhaps the authors have a good reason for adopting this approach, but it has not been motivated well enough I feel. Also, authors need to improve the way they present equations. CV in equation 1 is a function of a CDF of (I think) the observed series. Where this correction is applied is not clear to me if the CV is indexed with respect to the observed series. Is it applied to future simulations? Does that not create an inconsistency? I think this part is critical to the paper and it can be presented a lot better. Line 180: How can Step (5) be applica-

ble for the simulated-future time series in which most of the data points have a higher magnitude compare to the simulated-current time series? Line 193: Step (9) is more like additional information given with respect to the detrending procedure in Step (1) and Step (6). Therefore, the order of the step might be revised appropriately to provide a clearer stepwise procedure. Line 196: I feel the authors may be presenting the same approach as 10.1029/2009GL038401. A more mathematical presentation of the approach is needed, along with a discussion of potential issues that are created. I feel, for one, the authors may be introducing a bias in their representation of zero rainfall, but more details are needed. Line 290: It is stated that the climate change signal cannot be conserved for multiple time scales using the proposed procedure, with the discussion implying it can be done for both. This is analogous to the nesting approach which has been used a lot in the bias correction literature 10.1175/jcli-d-15-0356.1. Figure 9. Will the proposed approach applicable to bias correct simulated-future time series which contain different trend with simulated-current time series?

C3