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Comment on hess-2021-498

Anonymous Referee #1

Referee comment on "Evaluating quantile-based bias adjustment methods for climate change scenarios" by Fabian Lehner et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-498-RC1>, 2022

This article seeks to present a new method for bias correction of climate model outputs. Also, it seeks to present an intercomparison of bias correction between three available methods, one of which is the new method. Bias correction is a very important aspect of future planning for climate change given the strong limitations of raw model outputs for practical hydrological decision making. Thus, articles suggesting method improvements, or with insightful intercomparisons, should be encouraged.

However, in this case, I fear a lot of work is required to raise the material up to publication standard within HESS. I have a number of concerns, none of which are specifically about the technical details of the method, but some of which build on technical concerns raised by the reviewers of the original pre-print (HESS-2020-515). The concerns are listed below.

1. Detrimental dual focus (aka what is the paper actually about?).

In response to the reviewer comments, the authors have elected to increase the prominence of the method intercomparison and decrease the prominence of the new method. For example, the title is now 100% about the evaluation and doesn't mention the new method, and the abstract doesn't go into details about what is new in the method, rather, it just says "we present a method". In contrast, the body text follows the original manuscript and is still focussed on the new method. So the paper has a dual focus. Unfortunately, it doesn't really work to have a paper about two things—each detracts from the other, leaving both unsatisfactory. So the authors need to decide: if the paper is about the new method, then the evaluation is subsidiary and should be specifically designed to justify why the new method is an improvement. Alternatively, if the paper is about the intercomparison/evaluation, the new method should **not** be mentioned at all.

2. Is the method an advance?

If the authors elect to focus on the method, the next question is whether the method is an advance. Prompted by reviewer 1, the authors admit that "Equation 2 in Li et al. (2010) is the correct mathematical description for our method when correcting temperature" and that "for precipitation, our method is almost equivalent to PresRAT". However, "the important difference to these methods is that our method is strictly empiric/nonparametric". Given this description and perusing the HESS manuscript types at https://www.hydrology-and-earth-system-sciences.net/about/manuscript_types.html, I suggest that this kind of change is more appropriate for a "Technical Note" and does not constitute a theoretical or practical advance worthy of a research article. If this path was taken, the material would need to be significantly reduced to fit with the requirement of "a few pages".

3. Are the methods being compared the right methods to compare?

The answer to this question depends on the choices made at point 1. If the author focus is to introduce a method that builds upon existing methods (as per point 2 above), then the most obvious point of comparison is the parent methods. Thus, the comparison should include EDCDFm, PresRAT, and any other methods the new method inherits from. This is the only way to determine whether the new method helps or hinders, and in which context this occurs. On the other hand, if the authors elect to focus on method evaluation/intercomparison, then I think some changes will be required to make the evaluation more novel. This will require reviewing what other literature tries to do this (with which I confess I am not fully familiar) and then ensuring there is something different (and substantial) about this intercomparison - perhaps it's the number of methods being compared, or that their types have never been compared, or perhaps it's the study area that's novel (Austria). In any case, it seems likely that more than two methods will need to be compared, which translates to more work for the authors I'm afraid.

4. What literature review is required and how should the study motivation be framed?

In the case where a slight tweak is made to existing methods, there is no need for a lengthy literature review espousing the benefits of the parent methods. We can assume that the case was argued back when the original methods were published, and thus all that is required is a short summary of the benefits (or, if debated, of either side of the debate). This is why a "Technical Note" can be (indeed, must be) so short. On the other hand, if the focus is on intercomparison, then the authors must provide a general review of all bias correction methods, a review of other intercomparison studies, and a justification for what this study is adding to those existing studies. Unfortunately, the current introduction does none of these things well. It is quite long and lacks narrative and structure.

I could go into greater detail on some further technical aspects of the paper but I feel there is not much point until these big, overarching questions are settled, since they will impact many aspects of the manuscript. I would be happy to review future versions and give more detailed comments then.

In general, this manuscript has the feel of funded applied research being turned into a paper as an afterthought. Funded applied research is very worthwhile in and of itself but

to add something to the academic literature there must be something substantive that is new, novel or insightful, and not every practical project has these aspects.

I feel that I should end on some positives. Notwithstanding my criticisms of structure, the article is quite well written, with a good standard of English, well presented (particularly the figures) and with good attention to detail. I wish the authors well in to alter/add/refine the material so that it does eventually appear in the published literature.