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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Performance of bias correction schemes for CMORPH rainfall estimates in the Zambezi River Basin" *by* Webster Gumindoga et al.

Anonymous Referee #1

Received and published: 26 October 2017

This manuscript, entitled "Performance of bias correction schemes for CMORPH rainfall estimates in the Zambezi River Basin", investigates the performance of bias-corrected CMORPH rainfall estimates over the Zambezi River Basin. Although the topic is relevant and worthy to explore scientifically, I believe the manuscript should undergo major changes prior to publication.

Questions posed to reviewers:

1. Does the paper address relevant scientific questions within the scope of HESS? The paper covers the relevant topic of bias correction of satellite-based rainfall estimates over the Zambezi River Basin and falls within the scope of HESS.

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2. Does the paper present novel concepts, ideas, tools, or data? The novelty of the paper is limited as it follows the structure of similar efforts carried out for other river basins. This should not, however, invalidate its publication in light of the relevance of the case-study.

3. Are substantial conclusions reached? The results are not prone to a clear-cut conclusion, but the authors do a good job of comparing the different methodologies.

4. Are the scientific methods and assumptions valid and clearly outlined? Generally yes, although clarifications on some of the methods and choices should be provided.

5. Are the results sufficient to support the interpretations and conclusions? I have mixed feelings about this. While the results are certainly sufficient to say something about the bias correction performance, I believe it should be further characterized by a more structured set of metrics that cover a broader range of features of the rainfall fields that are being corrected.

Also, and perhaps more importantly, the paper fails do describe the performance assessment methodology in detail. I believe that such an assessment should be made based on a hold-out sample, and this does not seem to be the case.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

8. Does the title clearly reflect the contents of the paper? Yes.

9. Does the abstract provide a concise and complete summary? Yes.

10. Is the overall presentation well structured and clear? Yes.

11. Is the language fluent and precise? Yes.

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12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Generally yes, perhaps with some exceptions. See the file attached to this review.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes. Please see comments and suggestions on the file attached to this review.

14. Are the number and quality of references appropriate? I believe they are.

15. Is the amount and quality of supplementary material appropriate? No supplementary material is provided.

General comments.

The paper focuses on relatively simple bias-correction methodologies and performance metrics. I believe it would be worth putting them into perspective by mentioning more elaborate techniques. In what concerns performance metrics the paper could also be improved. I recommend adding the root mean squared error, the mean absolute error, and qualtile-quantile plots.

In what sample was the bias correction tested? The same which was used to calibrate the correction methods? I would like to see a comparison made on a hold-out sample (in space). Because the rain gauge data are already known, the value of using bias-corrected CMORPH data is that they provide information on the regions between rain gauges. So being, it is important to know how the schemes perform in those regions. One way to do it is to calibrate and apply the correction over N-1 gauging stations, use an interpolation model to infer the bias corrected CMORPH values over the Nth gauging station, and compute the error there. This could then be done holding out other gauging stations.

How can the uneven distribution of the relatively few rainfall stations that were used affect results and the interpretation of the results?

Some of the rain gauge series are affected by missing data. I believe it is relevant to show exactly how much of the data is missing.

How the corrections are interpolated between gauge locations needs explaining.

I would like the authors to clarify if and how the tested methodologies can be used in predictive mode (in other words, can they be used to correct CMORPH rainfall estimates even if no rain gauge data are available?).

The disadvantages associated with each bias-correction method should also be clearly stated. If only (or mostly) advantages are highlighted the reader will be given incomplete information.

It is the first time I come across Taylor diagrams, so there is a high likelihood that I am wrong in my assertion (something I help the authors can help me with). The Pearson's correlation coefficient and the standard deviation are bias-insensitive (take a series, add a constant - a bias of the expected value - to it and it will display the same standard deviation; correlation between the original series and the biased one will be 1, regardless of the bias magnitude). As it is described (a function of R and STD), the root mean square difference appears to be also insensitive to what is perhaps the simplest form of bias. What is then the big advantage of the diagram, as employed in this paper, to assess the bias-correction methods?

Specific comments.

line 32. Although SRE are certainly prone to bias, this fact alone does not explain why they are so. The same cloud properties leading to different precipitation "behaviors" in different regions would...

line 71. What is the (relevant) difference between the rainfall depth and volume?

line 118. Please provide a reference for the estimated number of the people who depend on water from the Zambezi.

line 138. Please clarify why each of the cited publications is relevant.

line 162. The Zambezi contains, besides large lakes, very significant wetlands (e.g. the Barotse Plains and the Kafue Flats). Why were these not considered in the analysis.

Figure 1, 2. The Zambezi River Basin does not correspond to the one displayed in the figures in the region of the outlet, near the Indian Ocean. What is represented as a small strip is in fact a very broad delta. Also, ît would practical to add small map showing where the Zambezi is located in Africa.

line 225. What were the alternatives tested in the preliminary analysis?

line 264. The authors mention that knowledge of the study area had a role in grouping. What was this role?

line 322. I did not find any reference to "distribution transformation" in the work of Fang et al. (2015). There is an approach in that paper (variance scaling), whose expression resembles eq. 6 (although with differences). What is also puzzling is that the reference to correction of frequency-based indices appears in the abstract of that work, but applied to Quantile mapping and to the Power transformation methods. Can the authors clarify this?

line 367. Correlation does not imply interdependence.

line 384. Is it the ratio of variances being shown in the plots?

Figure 2. Somewhat hard to read. I believe it should be improved.

Figure 3. The quality of the plots differs. Please fix this.

Figure 5. Is this information not already contained in Figure 4?

Figure 7. The plots are difficult to interpret. Consider using a Log-scale on the y-axis.

Table 1. Please clarify what "estimated ratio" is.

line 660. How does adjusting the daily mean directly affect correlation coefficients and

root mean square differences (defined according to the paper)? Probably indirectly because daily means are time-variant. If so, the choice of window is very relevant and, unfortunately, only one window was explored.

Detailed comments.

Please refer to the file attached to the review.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2017-385/hess-2017-385-RC1-supplement.pdf

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