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Interactive comment

Interactive comment on "Bias correction of daily satellite-based rainfall estimates for hydrologic forecasting in the Upper Zambezi, Africa" *by* Rodrigo Valdés-Pineda et al.

Anonymous Referee #1

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The manuscript by Valdés-Pineda et al presents an interesting application of satellite-based rainfall estimations feeding an hydrological model. The paper shows a logical structure and it is easy to read. The pipeline for such an application is appealing, but in my opinion, authors should improve the discussion of the results in order to provide better advise to readers interested on the topics, and to give a better context and motivation for the work done. Moreover, there are assumptions that are not clearly stated or supported by figures and text. Therefore, I suggest to present a more elaborated discussion of the results, before full acceptance.

The Introduction section would requiere a slight change on the wording. There is a key question in p2-l10 (page 2 - line 10) that is not further elaborated: Why there is a need to perform site-specific and season-targeted bias correction?. Also, this section should present in more detail main factors affecting the estimation's bias (see p9-l1 to l4). Moreover, it is not clear if authors seek to test the magnitude of bias, the temporal persistence of bias, the spatial pattern of bias or simply the most efficient empirical method for a specific application (see p13-l5 to l9). The analysis of previous work on UZRB should include some discussion regarding the spatial and temporal resolution of the satellite-derived products against the observational scale (gauges) and processes scale (e.g. predominant rainfall and runoff processes). In my opinion, the transboundary nature of the UZRB is a relevant issue or driver to approach the use on satellite-derived information on water resources management. I suggest to include this analysis into the Introduction instead of the Study Area section. Please, indicate when maximum flows occur (p3-l35). Section 2.2 would requiere some supporting references for the SST-rainfall (observational) relationship. Also, I suggest to include and analysis terms of inter-annual to decadal variability. Most of the results and conclusions rely on assuming that the CHIRPS data set properly represent the patio-temporal patterns in the UZRB. However, the manuscript discusses this issue mainly in qualitative terms. I suggest to better present a quantitative assessment of the representativeness of the data set. Regarding the bias-correction methods, I would like to comment three issues. The first one is the potential influence of offsetting the drizzle effect to 1 mm. Is there any relationship between local rainfall intensity features and the 1 mm threshold? I would like to suggest a brief sensitivity analysis for this issue. The second issue is the assumption of the Gamma-PDF as the best surrogate for rainfall statistics. Authors should provide a quantitative assessment in a pixel-basis for the goodness of fit between empirical and observed distributions. The third issue is about the novel approach presented. It would be useful for readers to also include some analysis in terms of results of the eigenvectors and eigenvalues. For instance, are there significant changes on loads depending on the validation and calibration periods? . The use of an hydrological model to assess the performance of gridded or satellite-derived data is appealing. However, there are a few issues that should be discussed. First,

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Discussion paper

how authors are able to separate different uncertainty sources (input and structural)? There must be a discussion regarding the (potential) magnitude of model's uncertainty against input's uncertainty. Also, there is a lack of discussion regarding the ability of the model to properly represent the hydrological process within the basin (not only the streamflow time series). The Results section is mainly descriptive. I recommend to include more discussion. For example, p10-l33 states that a given results is anticipated for "all scales". However, the manuscript only shows daily and monthly values. Authors should rephrase these section or perform analyses at finer temporal scales (14-days, 14-days windowing). Also, please provide (plausible) explanations for the spatial patterns of estimates and bias. Are high/low values only related to elevation? How cover could affect estimates? Is bias relate to synoptic types (p12:l21)? I suggest to rewrite the Conclusion section. Currently, the authors include several conditional sentences instead of proven facts or result-supported comments. Authors should be more concise and precise on answering two or three research questions. I would be informative if along with Fig 8, authors present and compare estimates for dry and wet 3-days (or 1-week) composites. Thus, readers could compare estimation at finer time scales. Figures 4, 13 and 10 should be redrawn to improve its readability. Figure 8 could be presented in terms of differences, too. Figures 3 and 11 should follow the same format. I suggest to use on maps quartile (or other division) for the color scale in order to better identify spatial patterns.

p3-l25: use lower case for km. p4-l12: I suggest to delete lines 12 to 16, as the authors state well-known knowledge. p8-l19: The acronym EOF is not defined. Through the manuscript, authors use the terms "forecasted" and "simulated" as interchangeable terms. Please, be consistent. I would prefer the use of simulated. p11-l31: Lines 31-35 should be places as comments at the end of the manuscript as they not provide facts or conclusion supported by results. p13-l16: delete "always"

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