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Hydrology and Earth System Sciences
Discussions

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.

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Dear Editor, first of all we would like to thank both reviewers for their comments and suggestions to our submission. The revised version of our manuscript (based on their comments) will definitely improve the quality and readability of our paper. The new manuscript will include most of the changes suggested by both referees. Detailed responses are attached below each reviewer′s suggestion or comment using red font color. Thanks once again for your feedback and prompt response. Anonymous Referee #1 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 appeal-

ing, 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 ïňĄgures and text. Therefore, I suggest to present a more elaborated discussion of the results, before full acceptance. We acknowledge this referee for his initial comments to our manuscript. We agree in the fact that our paper shows a logical structure and it is easy to read, but of course, there is still a chance of presenting a more elaborated discussion of the results, before full acceptance. Introduction Section 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-speciïňĄc 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). In the new version of our manuscript we are including a modified version of the introduction section. The key questions stated at the end of this section will be answered and further elaborated. In general, there is a need to perform site-speciïňĄc correction because every catchment is a unit with specific landscape and climatic features, and both of these can affect the performance of SPPs. The evaluation of season-targeted bias is also important and required given that the performance of SPPs is influenced by the way in which the sensors capture the information and how the algorithms use that information to estimate rainfall events i.e., SPPs cannot adequately differentiate between stratiform and convective rainfall events.

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 efïňĄcient empirical method for a speciïňĄc application (see p13-l5 to l9). This is a great point of view. We agree that this is the most confusing part of our paper because we are including all these analyses in the current version of our paper (1. test the magnitude of bias, 2. Test the temporal persistence of bias, 3. Test the spatial pattern of bias, 4. Test the most efiňĄcient empirical method for a speciïňĄc application). We will clarify what is the real motivation of our work in the new version of our manuscript. 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 ïňĆows occur (p3-l35). We will move the paragraph including the transboundary nature of the UZRB from the Study Area Section to the Introduction Section. We will also include some discussion about 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)

Section 2.2 Section 2.2 would require some supporting references for the SST-rainfall (observational) relationship. Also, I suggest to include and analysis terms of inter-annual to decadal variability. We will expand Section 2.2. to describe in more detail the climatology of the Upper Zambezi River Basin. For these purposes we will improve the discussion about the sea surface temperature (SST) patterns and their resulting atmospheric teleconnections. We will also describe how these oceanic changes are related to inter-annual or decadal variability of rainfall in the basin. 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 dataset. We agree that this is an important question that should be clarified in the manuscript. In fact, a quantitative analysis to compare the performance of CHIRPS and SPPs was carried out before submitting our paper (point to pixel correlations and hydrological simulations). This analysis revealed that CHIRPS climatology achieves better results than SPPs (in both correlations and simulations); therefore it can be truly used as reference dataset. In this regard we do not feel that this analysis should be included in the manuscript since our interest is not comparing climatology versus SPPs, but analyzing the performance of SPPs in the basin. In-

stead, we propose to include some paragraphs detailing the results of our quantitative assessment of CHIRPS and SPPs.

Bias Correction Methods Regarding the bias-correction methods, I would like to comment three issues. The ïňĄrst one is the potential inïňĆuence 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. This is a great comment. We carried out this analysis (before submitting our manuscript) by testing different thresholds between 0.1 and 2 mm. We found that these different thresholds do not have a significant influence in the correction of SPPs. We selected the threshold of 1 mm because it is the value more suggested by previous literature. We will mention this in the revised version of our manuscript. 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 ïňĄt between empirical and observed distributions. During our screening analysis we tested and compared different PDFs and finally determined that Gamma PDF (used for Quantile Mapping Method) is the best surrogate for rainfall statistics in the Upper Zambezi River Basin. This finding is strongly supported by previous literature. We will include more discussion about these results in the revised version of our manuscript. 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? Regarding this comment it is important to mention that both the Quantile Mapping and the Principal Components corrections are applied using all historical records which are updated every day with new data (climatology and SPPs); therefore, a comparison between validation and calibration periods is not required (used) for the real-time forecasting. Instead, will include an analysis and a figure presenting the results of the eigenvalues and eigenvectors to provide more details to the readers about our results.

Section 2.7 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, 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 streamïňĆow time series).

This is another great observation from this referee. We did not include a discussion (or comparison) about the magnitude of input and structural errors since the objective of our manuscript was to exclusively analyze the extent of input errors rather than structural erors. For instance, we compared the bias from different satellite products (input uncertainty) assuming that the input errors can appropriately be quantified if all other sources of uncertainty are kept constant. For example, we used the same model states and conditions for all simulations we performed. In this way, we were able to focus our analyses in the propagation of errors from the input sources rather than the influence of model′s structure. We will include more details about the experimental design carried out in our study plus some explanations about why we did not modify the structure of the model. Results 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 ïňĄner 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)? We analyzed daily, monthly and yearly scales, which is the main reason of including ″all scales″ in the wording of p10-l33. The section will be rephrased including these new suggestions. The spatial pattern of estimates and bias will be related to elevation features in the basin as way to reveal plausible explanations for the spatial patterns of bias in the basin. Conclusions I suggest to rewrite the Conclusion section. Currently, the au-

thors 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. This section will be rewritten according with modifications done in the revised version of our manuscript. Figures 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 ïňĄner time scales. Since Figure 8 presents the performance of SPPs along the seasonality of the basin, we feel that adding finer scales will not contribute in giving better insights about our results. It is also not clear how the composites suggested by this referee should be grouped and how many months should be included in the analysis.

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. Accordingly with these suggestions, we will include updated figures in the revised version of our manuscript. We tried different classification schemes for the maps and concluded that the one used currently in the paper is the best one for comparison purposes.

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 deïňĄned. 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" We will include all these suggestions in the revised version of our manuscript.

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