

Response to Referee #1's comments:

I appreciate the great efforts the authors have made in response to my questions and concerns. The revision clarifies nearly all the points I raised. I would suggest authors to include the suggestions provided below in the paper before it is considered for publication.

Minor comments:

Lines 40-45: "Within these studies, gauge-based precipitation products are often treated as reference products, or benchmarks, when evaluating satellite-based and other non-traditional datasets." Here, it would be nice to provide some examples of non-traditional datasets, along with citations. I would suggest citing the relevant research articles provided below.

1. Shahi, N. K. (2022). Fidelity of the latest high-resolution CORDEX-CORE regional climate model simulations in the representation of the Indian summer monsoon precipitation characteristics. *Climate Dynamics*, 1-23. <https://doi.org/10.1007/s00382-022-06602-9>.
2. Shahi, N. K., Rai, S., Sahai, A. K., & Abhilash, S. (2018). Intra-seasonal variability of the South Asian monsoon and its relationship with the Indo–Pacific sea-surface temperature in the NCEP CFSv2. *International Journal of Climatology*, 38, e28-e47. <https://doi.org/10.1002/joc.5349>.

The first reference also supports your sentence provided in Lines 45-50 "However, gauge-based gridded datasets can suffer from inadequate representation of extreme event..."

The authors would like to thank the reviewer for the feedback on the revised manuscript. We agree with the reviewer that the first article mentioned above is relevant to the general discussion on precipitation datasets across India. The above article also analyzed some of the datasets used in this manuscript. This article will be added to the text (in Section 1) and will be included in the citations. We do not think the second article mentioned above is directly relevant to the manuscript and so we did not include it in the citations.

Response to Referee #2's comments:

The authors would like to thank the reviewer for the feedback on the revised manuscript.

In order to understand the effect of interbasin groundwater flow (IGF) on the water budget, the reviewer is suggesting case studies of watersheds (and their aquifers) which were identified to be affected by precipitation underestimation – the suggested work by the reviewer includes extensive literature review, data compilation and data analysis. The goal of this study is to identify potential underestimation of precipitation across the whole study domain, and not just a specific region. While regional and local studies are valuable, one would require extensive data as well as regional/local expertise to perform such studies. It is beyond the scope of this study to perform such case studies. We hope that our study encourages water agencies to share valuable data on precipitation, streamflow and other hydrological variables, and motivates the community to better quantify hydrologic budgets across India's watersheds.

Please see below the specific responses to the individual comments.

Dear Editor,

Thank you for considering me as a reviewer of the manuscript. The authors have attempted to address the comments and suggestions given in the first review, which has improved the manuscript. However, there still remain a few major concerns and areas of improvement:

1. The issue of IGF is acknowledged and addressed by a case study approach where adjoining watersheds in Southern India are studied for UoP. Authors rule out IGF by observing simultaneous occurrences of >1 runoff ratios in high-elevation watersheds. However, in most regions, aquifers occur at multiple levels, and I recommend Authors check the NAQUIM project and the data available with them for these and surrounding watersheds (<https://www.aims-cgwb.org/index.php>). The aquifer mapping in the region suggests the presence of two aquifers at different depths, which may potentially contribute to neighbouring watersheds despite elevational differences.

While I understand the limitations in accessing precise aquifer maps for India, the assumption of negligible IGF is not justified as a physical reality. Assuming otherwise due to a lack of data would distort the interpretations of the physical system and precipitation hydrology.

In the example discussed in Appendix F, the reviewer is asking us to investigate if groundwater from lower-elevation aquifers can contribute to groundwater at higher-elevation aquifers, using the data sources outlined above. While a preliminary review of the above mentioned data sources indicates the presence of some relevant information, we believe that it is not possible to adequately address the reviewer's concerns within the scope of this study. We do not have the resources nor the expertise to pursue such an investigation. We would like to emphasize that we do not assume interbasin groundwater flow (IGF) to be negligible or rule out its existence. We merely provided an example (Appendix F) where IGF is unlikely the cause of the observed watershed imbalance.

A case study approach is suitable in such cases to exemplify the proof of concept and I recommend authors use District Resource Maps (DRMs) by the Geological Society of India, the aquifer management plans by NAQUIM project, and secondary literature to develop an improved understanding of regional aquifers and the presence/absence of significant IGF in select watersheds with the observed UoP.

The suggested case studies to understand and quantify IGF require extensive field-scale data along with regional/local expertise. While the above mentioned data sources have some relevant information, we believe that it is not possible to adequately address the reviewer's concerns within the scope of this study.

I would also recommend incorporating the case study into the main manuscript body and discussing the assumptions, hypothesis, and results accordingly with appropriate citations.

As discussed in the manuscript, observed imbalance could be due to several factors, viz., underestimation of precipitation, IGF, water management, terrestrial water storage changes or other factors not considered in this study. Since the relative importance of IGF is unknown, we do not want to overemphasize it. Hence, we chose not to move the case study (Appendix F) to the main part of the manuscript.

2. In the absence of India-level data on most parameters, as acknowledged by the authors, perhaps it would be better to focus on developing representative case studies wherever most information is available.

It would be pertinent to find as much secondary literature as possible for at least a subset of the study area. I would recommend authors do an exhaustive watershed-level review of studies on water balance in at least a representation set of the "imbalanced" watersheds. It would help contextualise the findings from a triangulation approach, i.e., ET+streamflow records, aquifer properties (DRM/NAQUIM), and water management (secondary literature).

The reviewer is suggesting an exhaustive literature review on imbalanced watersheds followed by a "triangulation" approach. We are unsure if we would be able to address the reviewer's concerns after such a review. For the suggested "triangulation" approach, we neither have reliable ET data (as discussed in the manuscript) nor have adequate information on aquifer flow regimes and local-scale water management. It is beyond the scope of this study to perform the above suggested literature review and analyses.

3. The study uses IMD-APHRODITE blended product in IMD-missing regions. Both gauge-based products are known to diverge in certain landscapes and seasons as per recent studies (Prakash et al. (2015) is one of the earliest comparisons). I recommend authors perform a comparative assessment of the two products, a pre-requisite to grided product blending, and report the findings in the annexure. Another simple check would be to conduct a buffered pixel-wise comparison for overlapping regions (both IMD & APHRODITE) around the IMD-missing regions and report the bias, if any, and its impact on the results.

For those regions of the study domain where IMD is unavailable (outside of India's boundaries), we used the gauge-based APHRODITE dataset to supplement IMD. Instead of assuming zero precipitation for regions outside of India, we are using the data from APHRODITE. The reasons for choosing APHRODITE were discussed in Section 2.2 of the manuscript. Also, we included text on studies comparing IMD with APHRODITE. Unless adequate precipitation gauge data is available, it is not possible to assess the differences between these datasets and their relative merits and demerits. Identifying the differences between precipitation datasets for specific regions and seasons is beyond the scope of this study. Moreover, the goal of this study is to identify potential underestimation of precipitation and is not an inter-comparison of precipitation datasets.

I think the manuscript addresses a pertinent issue and recommend the manuscript for a major revision again to address the abovementioned conceptual issues.