

# ***Interactive comment on “Performance of GPM-IMERG precipitation products under diverse topographical features and multiple-intensity rainfall in an arid region” by Safa A. Mohammed et al.***

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The authors would like to thank Reviewer#2 for the time invested in reviewing our manuscript. Some of the reviewers' comments offer valuable insights for revising and improving this manuscript. We have followed the comments carefully and responded to each comment or suggestion.

This paper presents quite an exhaustive summary of a data comparison of gauge- and satellite-based measurements during 4 years for the semi-arid region of Saudi

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Arabia. The main focus lies on the performance of the satellite product (GPM-IMERG) with respect to topography, rainfall intensity, and season. Main results include that the first half of the year shows better performance than the second, smaller rainfall better than larger, and coastal areas and mountains worse than inland. With all its exhaustiveness in reporting the numbers, the study reads like a technical report for a research project rather than a scientific article. With respect to the science, not many of the most obvious questions were answered or raised at all, as detailed below. Because I do not see an easy way to transform this report into a scientific study, I must reject it for further publication in HESS.

Reply: The authors disagree with the reviewer with regards to what counts as a contribution to science. Novelty and contribution to science can take several forms. This is definitely not the first, nor is it going to be the last, article published on the evaluation GPM IMERG products under different topographical, climatic, and hydrological conditions. As such the authors did their best to apply state-of-the art methods in evaluating GPM satellite products over one of the least represented regions in literature, the Arabian Peninsula. Also, the authors disagree that this article reads as a technical report since they have had extensive experience writing scientific articles and can easily make such a distinction. We would have appreciated a more constructive review pointing out the “obvious questions” that we may have missed, and we would have taken on the necessary effort to include those in the revised article, but the reviewer chose to deny us this chance.

Like the other reviewer noted, there is a strong overlap (topic- and author-wise) with two recent studies (<https://doi.org/10.1016/j.atmosres.2018.12.029> and <https://doi.org/10.1016/j.jhydrol.2018.02.015>), who conduct the same statistical comparison of the same satellite data in the same or a neighboring region. In response, the authors argue that more years have been studied now, or hydrological zones have been considered instead of political ones. It is true that more years are covered now, but each year is still treated separately, so there is no real gain in significance. More

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generally, it is not clear what scientific differences there are between here and there, so the overlap remains.

Reply: The authors do not see any problem with their attempt to conduct state-of-the-art evaluation methods to a study area that does not appear often in literature. The reviewer's claim that the overlap of authors is grounds for rejection is not founded, many research groups publish several articles on the same study area. If anything, this shows that our research group is keen on improving and expanding the scientific methods applied to our study region. We were forthcoming in noting the differences between this article and the previously published ones. We would like to note that the methods applied have been subject to improvement and we will continue to improve our methods and obtain better data to enhance our evaluation, so you can expect to see more articles in the future that covers the same study area. In addition, neither the techniques used, nor the scientific outcomes match the articles mentioned by the reviewer. In this article we have focused on two new outcomes (1) evaluating the effect of the topographical features of the study area on the satellite detection accuracy, and (2) evaluating the capability of the satellite products in detecting light rain. In the last point raised the reviewer argues that the use of data from multiple years does not represent an improvement in the evaluation of GPM IMERG data. Perhaps the reviewer is not familiar with the scarcity of ground observation data in the region (Arabian Peninsula), where obtaining multi-year country-wide data is in itself an achievement. The authors did not only treat the data obtained from each year separately, but we also provided an overall discussion of the performance in the 4 years of data (Lines 275-304). If we are given a chance to improve our manuscript presentation we would add rows to Table 3 to present a collective performance evaluation of GPM IMERG products throughout the 4 years.

As for the science, my impression is that if the authors had started, and they should have, from a map of seasonal rainfall climatology for the area, it would explain much of the presented results. Related to that, many claims are not really surprising, such

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as that satellite sensors fail to detect smaller showers or that rainfall errors scale with rainfall magnitude. If the authors decide to try to put this into a scientific article, I recommend strong reduction (why are there so many statistical measures?) and focus on the essential results, include climatology information and argue from that. It is likewise important that each result is augmented by a solid significance analysis, given that only 4 years of data have been used and external factors play an important role. This is especially important since not much aggregation has been undertaken with the data, leading to so many single questionable results instead of a few with greater significance.

Reply: The authors would like to thank the reviewer for his valuable suggestions. The seasonal rainfall variation was requested by the first reviewer and was included in our authors' comment AC3 (Mohammed et al. 2020). Nevertheless, the requested map can be produced and included in the final revision of the manuscript. Moreover, the authors believe that the implemented statistical measures complement each other and are very important for the full evaluation of GPM IMERG products. Mainly these statistical measures are used to assess three main issues: detection accuracy, errors, and the consistency (Lines 215-229). These measures were adopted by almost all previous research in this field.

More details: L 15: Why three?

Reply: because they are the main products produced by the GPM satellite and are subject to investigation by many research articles.

L 18: Isn't that the purpose of doing the final run? – If that doesn't improve results it would not be done.

Reply: This is not always the case, had the reviewer checked other recent articles (including our latest publication Mahmoud et al. 2019) it would have caught his/her attention that the Final product does not necessarily have the best performance.

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Abstract: The text should focus more on the surprises of the analysis. As it is written, the results are exactly as one would expect.

Reply: The authors disagree with this statement entirely. Scientific investigations report on both expected and unexpected results.

L 52: "sub-par"?

Reply: If the reviewer is proposing that we use "subpar" without the hyphen, then we can definitely do that in the revised manuscript. Otherwise a synonym can be used.

Intro: Most of the Introduction is about (already known) satellite technology and should be removed.

Reply: We appreciate the reviewers' comment, it would have been more helpful if the reviewer had specified the sections that are deemed unnecessary.

L 126: "for hydrological..."?

Reply: The sentence will be modified to read "TRMM cannot be the only source of data for hydrological applications as it provides limited input information"

L 140: This chapter could be significantly shortened. Giving the key characteristics is enough.

Reply: Thank you for your comment. This section will be improved in light of the reviewer's comments in the revised manuscript.

L 169: "...nor distribution"?

Reply: The distribution of the stations used to produce the GPCP data is not representative for the study area because it relies on few stations.

L 197: again too technical and not of interest.

Reply: Thank you for your comment. However, we have received interest from previous reviewers, on other related publications, to include such technical information to

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improve the replicability of the results.

L 205: "grids points"

Reply: The phrase will be corrected to "grid points".

Figure 2: If the Figure describes what is in the preceding text it should be removed.

Reply: Thank you for your comment. The authors will reduce the text and keep the figure for better illustration and understanding of the module that we used. This is thought to improve the replicability of results.

L 205: You should discuss whether and why there are no false positives (IMERG>0, MEWA=0) in the IMERG data.

Reply: The authors have discussed this particular point in Lines 221 to 223. Since, the analysis focused on matching only datapoints where rain was observed by gauge stations with that of satellite data, it did not include false positives. However, we will try to add a few lines discussing the false positives in the dataset.

L 221: That is a false characterization of the CSI. Please correct, and please explain why you choose more than one index. I 222: If they give the same values then only one should be used.

Reply: The authors agree with the reviewer and intend to remove the statement and calculation of CSI as it does provide additional information about the satellite performance in this study.

L 223: This should be moved to I 205.

Reply: Noted.

L 225: This is again a false characterization of MAE and RMSE. Both are closely related, and it should be justified clearly if both are reported.

Reply: The authors will correct this statement in the revised manuscript to read "The

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MAE and the RMSE are indices that, when used together, can assess the variation in the errors of a dataset. The RMSE is either larger than or equal to the MAE. This difference between RMSE and MAE correlates with the variance in the individual errors in an analyzed dataset.”

Table 1: HESS readers should know about this, so it can be moved to an appendix or cited from the literature. Or one can simply cite the Mahmoud et al. (2019) study.

Reply: The authors believe that including the equations used is important as a reference for the reader. Should the editor suggest to move Table 1 to the appendix, we would not object to that.

L 242: No reference is necessary for the definition of seasons.

Reply: Thank you for your comment. We will remove the citation.

L 243: This should be removed or merged with the introduction.

Reply: Thank you for your comment. We will remove this part.

Figure 3, legend: please label altitude

Reply: The authors already made the corrections in their response to the first reviewer AC3 (Mohammed et al. 2020)

L 276: The result should be presented for each season, not for each season and year. And if you decide otherwise, the reporting should at least mention the typical variation between years and try to understand that.

Reply: The authors appreciate the reviewer’s valuable comment. The authors tried to give such overall discussion in the text. However, we will try to include give more focus on the overall seasonal accuracy rather than including the differences of between the years.

L 281: irregularly because you have such small samples, see previous comment.

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Reply: Noted, the authors will revise this discussion to avoid any generalization.

L 289: First you show a large table, and then you report almost every number in it.

Reply: The authors will try to reduce repetition without compromising the discussion.

L 325: The fact that rainfall errors increase with rainfall magnitude is a normal scaling behavior. I would be surprised if it were otherwise.

Reply: Nothing is a “fact” unless proven by reliable investigations. As far as the authors know this is one of the few attempts to evaluate GPM IMERG performance in detecting light rain.

L 345: Remove or move to Introduction.

Reply: Noted. We will move this part in the introduction.

Fig. 5: This looks like single figure with 6 different scalings. Comparing it to Fig. 3 we obviously see topography here. Moreover, it gives only little information because the main features are probably below significance anyway. Why is the CC pattern opposite between IMERGE-L and IMERGE-F?

Reply: Figure 5 is not a single figure with 6 different scalings, but rather 6 small figures of the results of the CC and POD for the three satellite products IMERG-E, L, and F.

Fig. 7: Is the difference to Fig. 5 only that for Fig. 7 all stations are aggregated for each region?

Reply: The aggregation for each figure depends on the feature we intend to analyze. Figure 5 is needed to evaluate the impact of topography and Figure 7 illustrates the variation in the performance of satellite data over the different hydrological regions.

References Mahmoud, M. T., Hamouda, M. A. and Mohamed, M. M.: Spatiotemporal evaluation of the GPM satellite precipitation products over the United Arab Emirates, Atmos. Res., 219(January), 200–212, doi:10.1016/j.atmosres.2018.12.029, 2019.

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Mohammed, S.A., Hamouda, M.A., Mahmoud, M.T., Mohamed, M.M., 2020. Interactive comment on “Performance of GPM-IMERG precipitation products under diverse topographical features and multiple-intensity rainfall in an arid region” by Safa A. Mohammed et al. Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-547-AC3>, 2020.

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