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Hydrology and Earth System Sciences
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Discussions

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.

Anonymous Referee #2

Received and published: 26 March 2020

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 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,

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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.

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

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 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.

More details:

l 15: Why three?

l 18: Isn't that the purpose of doing the final run? – If that doesn't improve results it

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would not be done.

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

l 52: "sub-par"?

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

l 126: "for hydrological..."?

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

l 69: "...nor distribution"?

l 97ff: again too technical and not of interest.

l 205: "grids points"

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

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

l 221: That is a false characterization of the CSI. Please correct, and please explain why you choose more than one index.

l 222: If they give the same values then only one should be used.

l 223: This should be moved to l 205.

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.

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 Mahnoud et al. (2019) study.

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

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

Figure 3, legend: please label altitude

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.

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

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

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

l 345: Remove or move to 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?

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

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