

Interactive comment on “Hydrological modeling of the Peruvian-Ecuadorian Amazon basin using GPM-IMERG satellite-based precipitation dataset” by Ricardo Zubieta et al.

Anonymous Referee #1

Received and published: 12 January 2017

This paper examines the potential of the GPM-IMERG satellite-based precipitation dataset for obtaining reasonable model discharge estimates over the Peruvian-Ecuadorian Amazon basin. The MGB-IPH hydrological model, which has been used extensively to study the hydrological functioning of the Amazon basin, has been extended Westward and this study focuses on this region. In addition, several standard (commonly used) datasets, TMPA, are used and the results are compared with those using the GPM-IMERG data. These results are also compared to a purely rain-gauge based driven set of simulations. The results are fairly convincing in that the model is able to give quite reasonable discharge simulations over the period considered (for which the coverage of the different datasets overlap in time). The results are solid and

[Printer-friendly version](#)

[Discussion paper](#)



well presented: I also appreciate the concise nature of the paper for the most part: several details are glossed over and require a bit more discussion in the paper. I have highlighted some of these in my comments.

P.3 L.22: item b: Does this dataset (TMPA-V7) include information from the rain gauges shown in Fig.1 (all of them, half of them, just a few of them, none of them?). The authors note that TMPA-v7 and GPM-IMERG are the most similar...can the authors discuss a bit why this is so? I assume they use much of the same satellite and rain gauge data...?

P.3 L.5: Two questions related to details: could the observed rainfall have been interpolated to a 0.25x0.25 grid for better comparison with the TMPA data sets? 0.15x0.15 for the GPM-IMERG dataset? And in regions of mountainous terrain, oftentimes consideration of the altitude-rainfall gradient relationship is critical for spatially distributing rainfall onto a grid. Was this information included in the interpolation? Do the authors think this effect is important in this region?

P.4 L.9: MGB uses the rainfall aggregated to a daily time step. So, it seems that one of the main possible advantages of the GPM-IMERG (30 minute time step) dataset compared to the TMPA-V7 (3 hourly) for computing runoff generation by MGB is lost, especially since the convective nature of the rainfall is likely better resolved, in theory at least, using a 30 minute time step. Is there anything in MGB that can take advantage of the diurnal temporal distribution of the rainfall? If not, I think the authors should at least comment that hydrological models using sub-diurnal time steps might have larger differences between the TMPA and GPM-IMERG datasets owing to their different temporal resolutions...Can the authors comment on this?

P.5 L.1: How were these thresholds selected? Are they based on some sort of statistical analysis?

P.5 L.25: The text "overestimate observations"...should likely be modified to something like "produce overestimates compared to observations".

[Printer-friendly version](#)

[Discussion paper](#)



P.6 L.10-11: The authors have reported that the satellite-based datasets underestimate the dry and wet season rainfall much more for the Huallaga basin compared to the Ucayali basin: do the authors have any insights as to why this is?

P.6 L.18-20: While I am not surprised that detection of light events was difficult, why do the products have such difficulty predicting strong rainfall events? I might have (perhaps erroneously or naively I admit) assumed that such events might be better detected. Can the authors comment on this? For example, are the strongest events occurring in high altitude/mountainous regions which are more difficult to detect? Is the smoothing to daily averages related to this problem?

P.7 L.3: The calibration is glossed over a bit it seems: what set of parameters were calibrated? Were parameters calibrated separately for each precipitation dataset?. Also, no information is given on the quality or calibration of the MGB evaporation. Is it significant compared to the rainfall? Are there non-negligible compensating errors (evaporation bias might offset rainfall or discharge errors/biases)? A short discussion is needed.

P.9 L.8-9: The authors state that seasonal streamflows in the southern region are well modeled using the satellite datasets, and indeed the results support this conclusion. But in the northern part of the Western Amazon basin/region, the results seem to indicate that satellite products are not useful for obtaining streamflows from hydrological modeling: so this implies that further progress is still required.. It think this should also be stated in the conclusions.

P.9 L2.: A 20% detection rate seems low. Can the authors put this into some sort of context for the reader (e.g. is 20% indeed a reasonable value in this region, or would one hope to have 50%? or a higher value?). Also, what are the implications of these statistics and their impact on the MGB model simulations? Can a lower, say 10% detection rate, be assumed to be able to produce reasonable Nash scores? Or perhaps there is no clear relationship between these scores and the modeled discharge quality?

[Printer-friendly version](#)

[Discussion paper](#)



This is not clear. Can the authors comment on this?

P10 L.15-19: In Fig.4d, it is seen that the poorest Nash scores are in the northern part of the region shown: looking at Fig.4a (or Fig.1b), we see that there are relatively few observations in this region. But, this is where one would hope to benefit the most from a satellite product, but it seems this is not the case. In L.15-19 it is stated that such products hold promise for operational applications in data sparse regions, but it doesn't seem to be the case here? Can the authors comment a bit more or perhaps modify this slightly (seemingly) over-optimistic text?

General: the English is fairly good. My only comment is that occasionally words end in s which shouldn't or vice versa. Also, I saw quite a few instances where an article or "the" was missing. But aside from that, the text is in good shape. I would recommend tidying that up before publication: it should not be too much work for the authors.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-656, 2016.

Printer-friendly version

Discussion paper

