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Discussions

Interactive comment on "Bias Correction of Satellite-Based Rainfall Estimates for Modeling Flash Floods in Semi-Arid regions: Application to Karpuz River, Turkey" *by* Mohamed Saber and Koray K. Yilmaz

Anonymous Referee #2

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The work presents a study on the application of satellite-rainfall data for flash flood simulations for a basin in the Mediterranean region of Turkey. The first part of the work focuses on the analysis and bias correction of satellite rainfall estimates with reference to available rain gauges. The second part presents the hydrologic simulations based on corrected satellite-rainfall estimates. The topic of satellite-based flood simulation is of interest, particularly for data scarce regions as the one considered in this study. However, the work presented in this manuscript is limited in many aspects, which I highlight in more detail below, thus making it unsuitable for publication.

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Major comments

1. Clarity and originality of objectives. It is not clear which is the major added value of this study. Is this the first work regarding satellite-rainfall and flash floods? I do not think so. Just a simple google search using the keywords (satellite, rainfall, flash floods) return several other past and recent works, which in fact were completely ignored from the authors. References need to be updated and authors need to contrast their objectives, methods and findings to what is already shown from previous studies. This will help the reader (and reviewer) to understand what is the added value of this work and subsequently how significant this is.

2. Structure and coherence of manuscript. The coherence and structure of text in current version of the manuscript is very poor. At a large part I had the feeling that I was reading a document that was put together by simply pasting sections from a master thesis or similar. Many statements are unnecessarily repeated. The flow in developing the context in the introduction, but also other parts, is incomplete and sometimes certain statements are out of context. I provide some examples in the list of specific comments below.

3. Limitations in methodology and analysis. There is a general lack of clarity in many steps of the methodology that limits the understanding (at least of this reviewer). For example, was the bias adjustment factor derived and applied at monthly scale? Why not at daily scale? Also, was the spatial resolution of the hydrologic model at 1km? Do you think this is appropriate for modeling flash floods? What is the rational for selecting a rainfall threshold for the analysis? This is not explained well in the manuscript. Is the bias correction applied also for the values below the selected threshold (i.e. you may be increasing the "noise" as well if you do that). I provide more specific comments below.

4. Limitations in findings and conclusions. According to the conclusions of the authors one of the important findings relate to the selection of an appropriate threshold for

the correction of satellite. What is the rational for selecting this threshold? What are the potential dependencies of this threshold? This is important to generalize findings. But most importantly, several satellite-correction techniques have been proposed over the last decade that involve more advanced approaches (e.g. stochastic error models, distribution matching procedures etc) than the one proposed. Is the simple method applied in this study superior? If the authors wanted to demonstrate simply the effect of a simple correction on the hydrologic simulations, then they should have at least present the hydrologic analysis for both corrected and not corrected results.

The authors state also that the "bias factors calculated in this study could be used for hydrological applications at any region with the same climatic conditions." Given the large differences of satellite-rainfall error properties reported from numerous evaluation studies, I am considering naïve to state that an adjustment factor could be unconditionally applied to another region of same climate...

Overall, I believe that more work is needed to bring the manuscript to a level adequate of publication and the work should focus on advancing the analysis as well as the writing/presentation of results.

Specific comments

1. L13-14 in the abstract is not clear. Please revise. In general the abstract is too descriptive and does not provide the overall objective of this work clearly. 2. P2, L4: "hydrologic response is expected....". It is not expected, it is proven by several studies. 3. P3, L1-4: Why you mention all these products when you are just using 1 of them? And by the way, why you considered only 1 product and did not include at least the high-res CMORPH? 4. P3,L8: "...model implementation and calibration..." please revise it is not clear. 5. P3,L15-16: "..each flood acquires some particular and inherent characteristics of the occurrence locality". The meaning is totally unclear here. 6. P4, L9-10: You mention the issue of model parameter uncertainty but how that relates to the rest of the context discussed in this work? 7. P8, L11: "underestimated

bias" is not appropriate wording. Rainfall is underestimated not bias. 8. P8, L18-19: Interpretation provided is not meaningful. A rational for the selection of 1mm threshold is needed. 9. Why you present results on monthly scale (e.g. Fig2)? This temporal scale is completely our of flash flood context. 10. What is the difference between fig10 and fig 11? 11. P16,L16: "...tendency to underestimate rainfall..." is a result of NO DETECTION which is different from underestimation. 12. P17,L4-8: Is this the first time that these findings are reported? 13. Section 3.2: Why repeating the procedure? Is bias adjustment factor from monthly comparison applied at hourly scale? 14. Fig17 is not necessary as it is not related to main topic of this work. It is enough to provide reference to Ogzuler, 2003. 15. P21, L7: I am not sure that "physically-based" is an appropriate description for this model. Do you consider the curve number and linear storage models as "physically-based" approaches? 16. Fig19. "disaggregation (1km)". There was no mention on disaggregation of satellite rainfall estimates. If you mean that GSMaP rainfall was simply mapped on 1-km model pixels then this is not disaggregation. 17. P25,L1: satellite rainfall data offer better "coverage" not "spatial resolution" as mentioned here 18. P28, L1-2: Explain how you identify areas prone to flash flood events? 19. Fig.21 is simply a representation of the river network and in fact a coarse representation of the river network. It is not clear to me how this info can be used for "water management and disaster rist reduction" as stated by the authors. 20. P29, L1-2: "The main objective of the study is to enhance the capability of flash flood simulation using the corrected satellite-based rainfall data sets". You need to provide a comparison of hydrologic simulations between uncorrected and corrected. Also you hydrologic analysis should be focused on the flash flood events by providing error properties on their corresponding characteristics (e.g. flood peak estimation) instead of an overall efficiency metric that includes the whole time series.