

***Interactive comment on* “Evaluation of high-resolution satellite precipitation products using rain gauge observations over the Tibetan Plateau” by Y. C. Gao and M. F. Liu**

Anonymous Referee #1

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General comments:

This is an interesting manuscript of evaluating three satellite precipitation products with rain gauge observation over the Tibetan Plateau area at the daily level. The three satellite precipitation products include TMPA, CMOPRH and PERSIANN. The evaluation was done by taking into account of the averaged spatial patterns, and different behaviors in various climate zones, rainfall categories and elevations. The topic of the manuscript falls well in the scope of the HESS journal, and the flow of the manuscript is very straightforward to follow with enough clarity. The biggest concern of mine is the

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added scientific value of this work, which I will explain in detail next.

The regional evaluation of the hydro-climate products has its importance and necessity; however, these types of work can be continued infinitely by applying to place A, B, C etc. Thus I recommend that the selection of these types of study in the HESS journal should satisfy the following two criteria at the same time: 1) a special interest of the study area; 2) valuable insights derived from the evaluation analysis. The current manuscript has done well in the first criterion, i.e the importance of Tibetan Plateau area is well illustrated in the introduction and conclusion. However, the second criterion is relatively weak. The well known conclusion that TMPA/CMOPRH outperform to PERSIANN gets repeated in this work; and the only possible insights of elevation influences is quite superficial.

Specifically, for example, the analysis on the elevation impacts has several weaknesses. (1) The choice of 3500mm is lack of scientific support; and “systematic” error does not show really “systematic” since contradictory pattern shows up in Figure 8. (2) Three products show some contradictory trends for the bias with elevation in the same regions and among the different regions, and the author did not provide convincing arguments to explain these differences. I am also further curious that given the poor trends in various regions, how to explain the residual of the linear trends? (3) The same problem exists in that the author did not explain why the HIIC and IID behave differently from other climate zones in CMORPH and TMPA behaviors.

In able to publish this work in HESS, I strongly recommend the authors to develop a more comprehensive analysis, esp. the elevation part. In sum, I recommend major revisions for this paper.

Specific comments:

Page 9505, line 6-7: rewrite the sentence to incorporate the 1998 flood impacts with the sentence before.

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Page 9506, line 3-4: Combine these two sentence together, i.e. “Though high-resolution products have bias, the accuracies need to be evaluated.”

Page 9508, line 13: desert and forests are land cover not microclimates.

Page 9512, line 14: “correlations and biases” of what? Please specify this.

Page 9512, line 20: has difficulties in addressing

Page 9514, sect 3.4: Please refer to the “General comments” for the specific comments.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 9503, 2012.

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