Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-453-RC3, 2016

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Temporal and spatial evaluation of satellite-based rainfall estimates across the complex topographical and climatic gradients of Chile" *by* M. Zambrano-Bigiarini et al.

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Received and published: 18 October 2016

GENERAL COMMENTS

This study carries out a validation study of seven state-of-the-art SRE products (TMPA 3B42v7, CHIRPSv2, CMORPH, PERSIANN-CDR, PERSIAN-CCS-adj, MSWEPv1.1 and PGFv3) over the complex topography and diverse climatic gradients of Chile. Different temporal scales (daily, monthly, seasonal, annual) are used in a point to-pixel comparison between precipitation time series measured at 366 stations (from sea level to 4600 m a.s.l. in the Andean Plateau) and the corresponding grid cell of each SRE. Different continuous and categorical scores are used to assess the ability of each SRE to correctly estimate precipitation amounts and intensities. Despite literature is plenty

of studies about the assessment of the performance of SRE, additional efforts in this direction is very welcome since:

- new products and new releases are produced every year which are not still validated (I would like to thank the authors to use also new datasets like MSWEP).

- it is not clear what are the best performance scores to be used and which is the effect of the benchmark on the final results.

- Only by validation studies we can understand the future directions in the improvement of sensors and retrieval algorithms.

For the above reasons I think the paper is of interest of the HESS reader. On the whole, I found the paper well written and organized. It contains a lot of material which however has to be better synthetized in some key figures. For this, I suggest the authors to make an effort for presenting less figures or finding alternative plots or tables able to synthetize the results. I also have some major concerns (but nothing that impedes the paper publications) that the authors should address before the paper can be considered publishable on HESS.

1. The first is the discussion of the accuracy and the density of the benchmark dataset. After divided the area in different zones (latitude and elevation) the authors come up with a different number or rain gauges for each zone for each elevation. Since performance scores can be affected from the network density I suggest to report also the number of gauges used for each latitude and altitude zone and discuss more potential effects on the results.

2. Higher elevations in winter may be affected by the problem of snow which can be missed by rain gauges. Can the authors can add a comment on that?

3. It is not clear the rationale behind the choice of the SRE products. Some of them are gauge adjusted while some others not (e.g. CMORPH) despite a gauge corrected version of this product exists. I expect some comments on that.

4. In the outlook section (final section) it reads “Despite continuous improvements of most SRE products, a site-specific calibration is still recommended before any use in hydrological studies. This was evidenced by the PGFv3 dataset that showed a better performance compared to the other products due to the statistical merging of local precipitation observations. This highlights the need for adequate station observation networks to complement the SRE products, due to the dependency of SRE product quality on this ground-truthing.” Here, it is taken for granted that the present rain gauge network provides the best performance in hydrological modelling, and since PGFv3 compares well with it then it will provide the best hydrological simulation. However, there is no evidence (by a hydrological simulation) that this is true and even if can accept that this can be assumed reasonable where the rain gauge density is high it cannot be generalized everywhere, especially because PGF uses the same data of the rainfall network. I think this can make believe the reader that gauge data are the truth while they represent just a good (not in all cases) estimate of the truth rainfall. For instance, if the gauge density is too low the spatial representativeness of the rainfall might be seriously affected. I suggest to discuss more this statement or provide a proof able to demonstrate it.

Below I provide ADDITIONAL COMMENTS in order of appearance in the manuscript also indicating their relevance.

Pag. 2. Lines 24-29. MINOR. I think it is not relevant how other people called satellite precipitation estimates.

Pag 4. Lines 10. MODERATE. “The present study attempts to exhaustively evaluate -for the fist time- the suitability of seven state-of-the-art SRE products or hydrological application in this data-scarce and complex mountainous region”. There is no hydrological validation of the SRE in the paper. Please consider revising.

Pag. 13 MODERATE. Figure 14 is not cited neither discussed in the text.

Pag. 14 lines 28 MODERATE. Annual performance worse than daily ones. There could

be a dependence on the number of points (only 8, 2003-2010 for the yearly analysis) and thus a scarce representativeness on the performance of the products? Or it is an effect of the systematic bias a shorter time scale which then is amplified at annual scale. Can you add a comment on that?

Supplementary material is not used in the text and some performance scores are calculated but never cited in the text.

COMMENTS ON THE FIGURES

1. The number of figures in the paper and in the supplementary material is enormous. I suggest to merge figures 4, 5, 6 and -7 in one figure and better highlight the color of the dots since they are difficult to read (you can use a different colorbar).

2. Box plots are interesting but you can also consider to organize figures 10 to 13 in a Table. It will show the same concepts and will allow a better comparison between the different components of the KGE.

3. POD and fBIAS can be shown in a figure where in the x-axis you have the rainfall classes and on the y-axis you have POD and FBIAS. Then different curves will point to different products

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