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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Evaluation of precipitation estimates over CONUS derived from satellite, radar, and rain gauge datasets (2002–2012)" *by* O. P. Prat and B. R. Nelson

Anonymous Referee #1

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The authors evaluate relatively long (2002-2012) records of precipitation estimates from different sensors and devices (satellite, radar, rain gages). The focus is on the continental United States and analyses are performed at different temporal scales. The work and results seem solid and fit nicely within the line of research that has been pursued by the two authors. With that said, I have some major and minor issues with this study, as detailed below.

Major comments:

1. A big problem I have with this study has to do with its motivations. I found asking

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myself "why this study" quite a few times. If the focus is on a climatological analysis, then the satellite is not your best choice given that you have rain gages for a much longer period of time. The authors wrote "this paper proposes to evaluate satellite precipitation estimates in the perspective of climate applications." Why? I would use 30+ years of rain gage measurements rather than 13 years of satellite estimates to accomplish that.

2. I don't think that the United States is not the right place to perform a study of this kind because of the large rain gage network. I would also argue that these results cannot be generalized to other regions of the world for two main reasons: 1) different climatology, synoptic conditions and types of event; 2) lack of a dense rain gage network (compared to the United States) to bias-correct 3B42. A study of this kind would have made more sense to me if the focus had been global and/or if analyses had been performed at the sub-daily scale (even though I would still argue that in the United States there are rain gages providing data at the hourly/sub-hourly scale for a period of time longer than the satellite).

3. There are a number of additional rain gage gridded rainfall products with high space time resolution (e.g., see data by Ed Maurer at Santa Clara University).

4. I would assume that the vertical bars in Figures 4 and 6 are standard deviations. If that's the case, I doubt that the differences in mean are statistically significant. Please test this formally.

5. Another element that is not discussed and that could affect the evaluation of the products is related to the fact part of the study region is outside of the orbit of TRMM ($\sim$35N/S).

6. Section 5 requires some additional work. I don't think it makes sense to compare "extreme" rainfall at fine resolution (Stage IV or even worse rain gages) with respect to a $\sim$625 km$\hat{}$2 pixel. Analyses of this kind should have been performed by either regridding Stage IV and interpolating the rain gages, or by using thresholds associ-

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ated with the rainfall distribution for each pixel/location and products (e.g., 95th or 99th percentile).

Minor comments:

1. While the manuscript is generally well written, there are few typos here and there [e.g., pg. 11492, line 14 (remove parenthesis); pg. 11497, line 6 (stage IV); pg. 11499, line 15 (3B43RT)].

2. Pg. 11505, line 9: rain gages have troubles measuring solid precipitation as well.

3. Pg. 11509, line 12: why “radar-only” if rain gages are used as well?

4. Please include confidence intervals in the qq-plots on Figures 3, 7, and 8.

5. Figure 10: are the proportions different at the 5% level? Similar question for Figure 11.

6. Are the 3B42RT data rerun every time there was a change in the number of satellites providing data? If not, it is hard to make comparisons.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 11489, 2014.