

Interactive comment on “Characterization of precipitation product errors across the US using multiplicative Triple Collocation” by S. H. Alemohammad et al.

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The paper describes the application of a modified triple collocation approach to the problem of evaluating large-scale precipitation data sets. The proposed modification allows for the more appropriate treatment of precipitation errors as multiplicative in nature. Issues surrounding the potential impact of error cross-correlation are examined via the decomposition of triple collocation error results over heavily-gauged reference sites.

Overall, this is a high-quality paper on a topic of significant interest. The application

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of a log-transform to deal with multiplicative precipitation errors is a very nice methodological extension and clearly superior to the existing approach (of naively assuming that all errors are additive in nature in order to shoehorn them into a TC framework).

I also appreciated the effort to explicitly examine the role of error cross-correlation on TC-derived error estimates (in Section 5). However, the one important thing I felt was missing was a re-examination of results in Section 4 based on the (non-trivial) impacts of cross-correlated errors (isolated in Section 5). For example, a key result in Section 4 is the relative lack of accuracy for the GPI precipitation product. However, in Section 5 (and the supplementary materials) we also see that GPI is relatively more independent (i.e. contains less error cross-correlation) than the other precipitation data sets. Given that TC will penalize GPI for this lack of dependence. . . does this mean that the analysis in Section 4 is truly even-handed? Is GPI being unfairly penalized due to being truly independent from the other products – as opposed to it being FAIRLY penalized for its weak relationship with “true” precipitation? So basically, I’d like a little bit of guidance about how the conclusions presented in Section 4 should be re-examined given the cross-correlation issues presented in Section 5. Should the reader really trust that relative rankings presented in Section 4?

I understand that this is a generic problem with any TC analysis; however, I think there are a couple of things that the authors could do to better address this point. First, they could examine whether or not the relative rankings that they derive using TC (at the 6 reference pixel sites examined in Section 5) accurately reflect the rankings they achieve when comparing all the products against the high-quality rain gauge observations acquired at each sites. If TC can successfully replicate the gauge-based rank correlation analysis at these 6 sites – that would be good evidence that the spatially-distributed TC results in Section 4 are robust in a relative sense (despite the known bias issues associated with the neglect of error cross-correlation).

Another step that could be taken would be replace the TMPA 3B42 dataset with its “real-time” (RT) equivalent (TMPA 3B42RT) which is not gauge-corrected. This tran-

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sition would make the “TRMM” precipitation product relatively more independent from the NEXRAD and GPCP datasets (which also have a gauge-correction component). Therefore, this transition towards greater error independence should lead to an increase in TC-derived error for the NEXRAD and GPCP products and a decrease in error for GPI (when considered as part of triplet that includes GPI). The size of this increase (or decrease) could be used as an indication of how serious the cross-correlation problem is across the entire study domain. Does it – for example – significantly close the gap between GPI and NEXRAD TC results over the eastern part of the study area?

Therefore, prior to publication, I would strongly recommend that the authors address this issue in some manner. At the very least, add 2-3 sentences describing the consequences of the analysis in Section 5 on earlier results in Section 4.

Some other minor suggestions:

- 1) Page 2536, Line 9-11: Clarify what exactly is meant by “homogeneous”? You mean homogeneous in a statistical climate sense. . .correct?
- 2) Page 2535, Line 20-22: I don’t see how zeros would violate the assumption of error independence. . .however I can see how they would cause fatal problems in a log-transform analysis. The authors might want to re-write this sentence (or improve its clarity). Also, what about extremely low rainfall values (right at the edge of numerical precision). . . can they skew results conducted in log-transform space?
- 3) Page 2541, Line 1-4: I had to read this sentence several times to follow it. . .I’d recommend re-writing to clarify its meaning (e.g. be a bit more specific. . .representativeness error in what?....and cross-correlation in errors between what and what?).

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