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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Bias correction of gauge-based gridded product to improve extreme precipitation analysis in the Yarlung Tsangpo-Brahmaputra River Basin" *by* Xian Luo et al.

Anonymous Referee #1

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I have carefully read the article: "Bias correction of gauge-based gridded product to improve extreme precipitation analysis in the Yarlung Tsangpo-Brahmaputra River Basin." By Luo et al. While I find that the results of the authors are interesting, I don't quite see how they amount to novel and publishable results as they stand. I should stress that my field of research, in the strictest sense, is bias correction of hydrological data from regional climate models for use in impact model forecasts. So, although I am well informed in matters concerning bias correction of observations, there may be something in the significance of this article that I am not quite understanding.

The authors are using two sources of non-gridded observational data (NMIC and GHCN) to bias correct APHRODITE which is a gridded observational dataset. They use 4 well established Bias Correction (BC) methods. The first two are essentially multiplicative correction factors. They differ in that the second uses a wet-day correction. The third is a variational method, fitted to correct mean and variance, and the last is a parametric Quantile Mapping BC. All these methods are well established, their pros and cons are well studied.

As far as I can see, the authors use all the available non-gridded data to correct the APHRODITE data-set, and then examine the effects of the different BC methods against the very same non-gridded data-set that was used for BCing. This implies that all the comparative results (section: "Evaluation of APHRODITE estimates") are only demonstrative of the mathematical construction of the BC methods and not of any increase in the skill of the corrected APHRODITE data. In simple words, if you bias correct a model to an observation, then, trivially, it looks like that observation. In climate forecasting, one uses past observations and hindcasts to calibrate the BC method and, subsequently, applies the results to bias correct future climate simulations. To validate the BC method one divides the observations into two periods and uses one for correction and one for validation. The studies I have reviewed where observations are bias-corrected, usually divide their observations into two groups as well, one for correction and one for validation, alternatively they sometimes use a leave-one-out cross-validation method. Again, unless I missed something, the comparisons of extreme events indexes between corrected and raw APHRODITE, while insightful, doesn't tell us anything about which one is better since we do not have observations of extreme event statistics from the non-gridded data.

In conclusion, I suggest that the authors extend their work to validate the bias-corrected APHRODITE against observations that were not used in the calibration process and then resubmit their work. Below are line-by-line comments the authors may find useful. Comments:

Line 30 to 32: I do not think the authors have proven this statement: “Bias correction [. . .] greatly improves the performance of extreme precipitation analysis”

Line 36 to 38: I do not see how the results, since they are not cross-validated, help select a bias correction method. Moreover, there are many more bias correction methods available in the literature than those mentioned in this article. See Teutschbein and Seibert 2012 or Cannon et al. 2015

Cannon, A, et al. Bias Correction of GCM Precipitation by Quantile Mapping: How Well Do Methods Preserve Changes in Quantiles and Extremes?, Alex J. Cannon*, Stephen R. Sobie, and Trevor Q. Murdock, Pacific Climate Impacts Consortium, University of Victoria, Victoria, British Columbia, Canada. https://doi.org/10.1175/JCLI-D-14-00754.1

Line 90: As explained above, I do not think the authors “evaluated” as much as “described” their performance.

Line 125: The sentence: “The ratios of rainfall observations. . . “ and the sentence after are unclear.

Line 146: I find the indexing not to be exhaustively clear. Is Pobs a station data value? Has the corresponding PAPH been interpolated or vice versa?

Line 153: I know what a wet day correction is but I doubt anyone who does not would understand this sentence.

Line 172: Why show the Gamma density if you are fitting the CDF? Indeed, why write a generic functional form at all? What do the authors mean by “matched”? Is it “fitted”?

Section 2.3.2: I do not see the need for 5 different error measurements.

Line 203 to 204: IDW has serious effects on extreme value distributions. The authors should compare what the distributions look like before and after interpolation.

Section Results: As explained above, results in section 3.1 are unsurprising, while

section 3.2 are not clearly useful to APHRODITE data users.

Line 250 to 251: I do not see how the authors can say this.

Section Conclusions: The authors draw three conclusions and, in the strictest sense, I agree with all of them. This is because the first conclusion is unsurprising while the last two are couched as possibilities instead of results. I refer to language such as: “. . . is expected to perform better in extreme precipitation analysis” and “extreme precipitation may be greatly improved”. While I absolutely agree with these two statements, they are not novel.

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