

## ***Interactive comment on “A trend-preserving bias correction – the ISI-MIP approach” by S. Hempel et al.***

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Received and published: 21 February 2013

Authors present a new bias correction methodology for daily precipitation and temperature data, based on an already applied to the ISI-MIP methodology. The proposed methodology belongs to the parametric transformation methods. The manuscript is generally presented in a clear and understandable way. It is also nice to use a single timeseries to present the correction procedure clearly. However, there are still some major points of criticism.

1) On the initial submission, it was clear that the major caveat of the methodology was the lack of cross validation. Anonymous Referee #1 (AR#1) pointed out this problem, asking for a major revision.

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Authors claim that 40 years of observations are not enough to calibrate/validate their methodology. It is generally true that the larger the observational dataset, the higher the certainty level of the calibration. However, the fact that 40 years of data are not enough is not supported by the literature (e.g. the split sample calibration – validation scheme has been tested successfully in Piani et al, (2010), using 10 years' time slices – this is half the available data). Upon AR#1 request authors agreed to the need of a calibration-validation experiment (in fact they made an honest effort to add a calibration validation period). Having said that, one should expect that the entire evaluation of the methodology would be based on a calibration validation context. More specifically, the comparison to the WATCH methodology should be done by applying the WATCH methodology to the same periods of calibration-validation.

2) Authors should elaborate on their results further. The difference between the past and future trends of Fig .7 and the remaining bias of Fig .8 is not an adequate metric for the method evaluation. Assuming that authors will add the validation section, they should provide comparisons for every aspects of P and T they elaborate with, i.e. mean, standard deviation, number of wet days between corrected GCM P and T and the WFD P and T.

3) After the drizzle day correction, (page 63), the dry days' precipitation is evenly distributed in the wet days. This is a "fine tune" that serves for the need to keep the mean precipitation between WFD and corrected GCM consistent, but alters the climate signal in a fairly arbitrary way that is not supported adequately on arguments.

4) In Figure 9, 10, the lower and upper 10%-ile results are not presented. Especially the upper percentile of the daily precipitation is of great importance, since it carries a great proportion of the total precipitation. In many semi-arid areas, the upper 10 percentile may carry half the precipitation that the area receives. Finally the extreme precipitation, also included in the upper 10 percentile, is also an aspect that should not be ignored.

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5) An arbitrary upper bound of daily precipitation of 400mm/day is introduced to avoid single extremes. How this threshold was chosen? The WFD dataset has a maximum daily precipitation value of 724 mm/day and several daily values over 400mm/day. This suggests that the 400mm/day threshold not only limits the observational dataset itself, but also the probable “new extremes” of the future corrected precipitation, creating the false assumption that the maximum daily precipitation cannot exceed this threshold.

6) If authors will to compare their results to another bias correction technique’s results, it is of course very welcome. However it would be better to compare results for the entire year, not just a calendar month (April).

Minor points: Page 64 – lines 1 to 5: The correction procedure is subject to constrains. This means that there are grid cells that were not corrected at all. This should be defined in some way in Figure 7, 9, 10.

Page 58 - line 14: The threshold of 10 for the multiplicative factor is defined vaguely.

Page 58 - line 19-20: Authors state that a possible reason that multiplicative factor C can take unrealistic values is that the assumption that “model and observations are well described by the same type of distribution (e.g. gamma distribution) does not hold”. How did this conclusion occur?

Page 61- line 1 to 5: Authors state that gamma function well approximates the GCM and WFD precipitation, and that gamma function is not defined at zero. I could not understand why this is the reason that frequency and intensity should be separately corrected.

Page 64 - line 4: The cut-off value of 80 wet days is stated to be motivated by sensitivity studies performed in WaterMIP. A proper reference is needed though.

Figure 8, 9: the precipitation values should be presented in a more common unit i.e mm/day, mm/year.

References Piani, C., Haerter, J., & Coppola, E. 2010. Statistical bias correction for

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daily precipitation in regional climate models over Europe. *Theoretical and Applied Climatology*, 99, 187-192.

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Interactive comment on *Earth Syst. Dynam. Discuss.*, 4, 49, 2013.

**ESDD**

4, C61–C64, 2013

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