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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Stochastic bias correction of dynamically downscaled precipitation fields for Germany through copula-based integration of gridded observation data" *by* G. Mao et al.

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I read this paper with much interest, as its topic is important and the methods were not well known to me. The first reading left a few questions, and the second reading of the paper increased the number of problems with respect to the methodology and the results.

The goal of bias correction is to provide time series which preserve the signal of the meteorological models, but are not biased with respect to the observations.

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1. The methodology provides a set of realizations as time series. The evaluations are based on the mean of theses series. The mean is however not a good bias corrected series, as it has a different marginal (with reduced variance) than what was assumed. Thus the comparisons made with the mean of the realizations are misleading and do not reflect the quality of the corrected time series.

2. It is unclear for me why the authors did not use the normal copula for the description of the dependence. There is no reason to assume tail dependence, and there are no evaluations with respect to extremes.

3. It is not clear how the authors handle zero precipitation. Further how the problem of different zero precipitations for the model and the observations is treated? How is the distribution of the lower dry probability modified?

4. It is unclear to me why the authors did not use truncated copulas as suggested in Bárdossy and Pegram (2009) which is referenced in the paper. This approach could help to avoid several problems with the zeros.

5. The fit of a parametric distribution followed by a fit of the copula based on the empirical distribution is statistically not correct.

6. There are several problems with the spatial distributions of the bias corrected series:

   (a) Is a parametric fit of the local precipitation distribution really needed? In my opinion the empirical distributions or a non-parametric fit would do a better job. Further this would avoid some problems with the spatial discontinuities imposed by taking different distributions.

   (b) The use of different copulas for the different locations is also causing spatial ruptures.

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(c) The method provides individual time series for each pixel. The simulated series are independent, thus there is no spatial coherence. This is a serious restriction of the suggested methodology, which can thus only be used for local and small scale studies.

7. The evaluation of the series is only concentrating on the mean behavior of the simulated series. There are no attempts to look at other statistics, such as variance, dry probability etc.

8. The bias remaining after the correction is very high. It would be interesting to know how the observed and modelled precipitation itself was changing. Was the signal captured?

9. The possibly biggest problem with the method is its partial inability to reflect the signal of the meteorological model. The weaker the dependence between model and observations the less the model signal is reflected after bias correction.

   This is illustrated with a small example. I simulated 500 realizations of modelled precipitation with a mean of 2mm using an exponential distribution. The observed precipitation has a mean of 3 mm and follows an exponential distribution. Copulas with different degrees of dependence ranging from full dependence to independence were used.

   It is assumed that the model shows a precipitation increase of 50%. The corresponding bias corrected series were simulated, and the means were compared to the original mean (3mm for the observations). The increase in precipitation varied between 0 and 50 % depending on the degree of dependence. The weaker the dependence the smaller is the signal which is captured. This is unfortunately not in the sense of bias correction, where the signal should be reflected.

10. Remark: The quantile/quantile transformation can be regarded as a special case of the suggested methodology with a fully dependent copula (rank correlation

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equal to one).

In conclusion this is an interesting paper, which unfortunately left a lot of questions open. In my opinion the authors should improve the methodology before it can be published.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 7189, 2014.

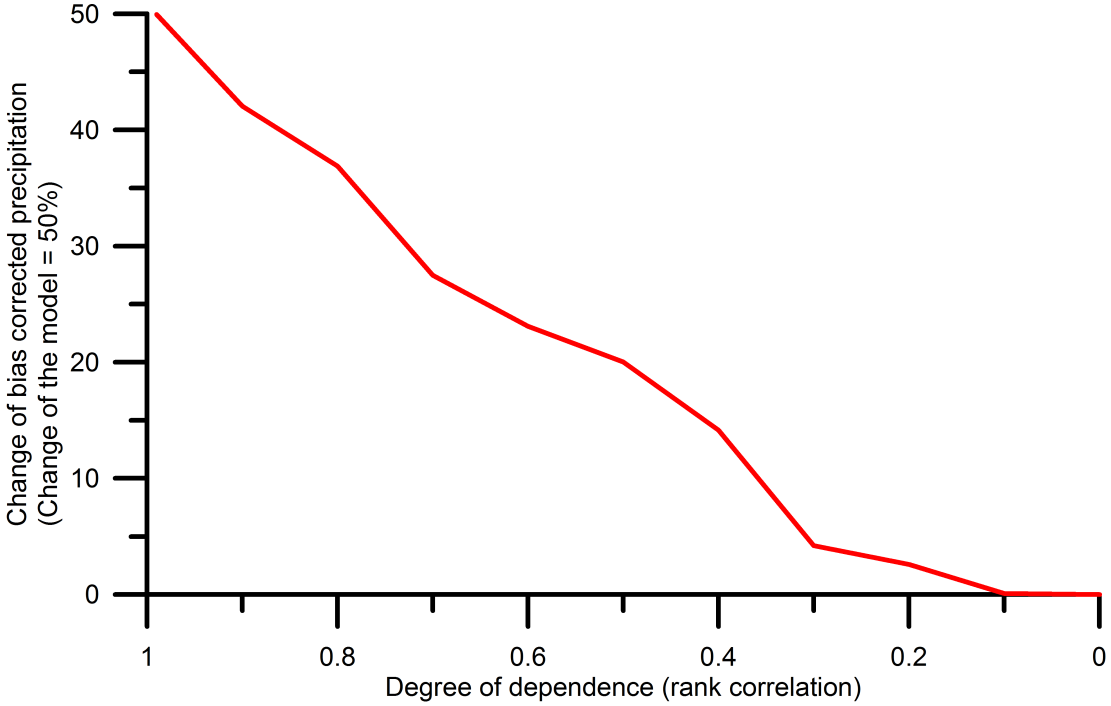

Fig. 1. Signal vs rank correlation

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