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Discussions

Interactive comment on "Estimating monthly rainfall in rural river basins under climate change: an improved bias-correcting statistical downscaling approach" *by* D. L. Jayasekera and J. J. Kaluarachchi

Anonymous Referee #1

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The authors present a method to downscale and bias correct precipitation from a GCM to the point scale. In its current state, the manuscript is in a state that it is difficult to asses it. Much of the information needed to understand it is missing, and several explanations are so muddled and unclear that the authors have basically failed to do their main job: clearly explain what their paper is about. I therefore have to suggest to reject the manuscript with an option of resubmission after several major gaps and weaknesses have been addressed.

Major points: First of all the paper is not standalone. The authors often refer to a paper by Kim (which they even haven't authored) for important aspects of the work. I struggled a lot to understand the algorithm and haven't succeeded yet. I initially thought that the author's propose a change factor based weather generator. In typical such approaches, a weather generator depending on a set of parameters is fitted to observed time series. The change of this parameters is then derived from a climate model to get a future estimate of the weather generator parameters. But the authors seem to have developed a different method, but the explanation of this method is not at all clear from the manuscript. Some questions: -how do you model occurrence/non-occurrence of precipitation with your Markov model (intro section 4) if you then later introduce N states, where N depends on the range of precipitation? This sounds self-contradictory! -what do the C(1,N) represent? Why do you produce continuous random numbers within these states? Shoudln't you just simulate in which state you are (i.e., 0 or 1)? And what does the notation (1,N) mean? And what does the dimension 1000 stand for? Why is it d_i, but then always the same? And does this just mean that you simulate 1000 time series of 100 years each? -why do you have separate bias correction and change factor algorithms? This sounds rather ad-hoc and is not motivated at all. And why don't you apply the change factors to the weather generator parameters but rather to the simulated precipitation directly? (I assume that this is done, it is not explained clearly). The whole section 4 has to be completely rewritten with a lot more detail and motivation in order to be able to discuss your results!

Second, the authors do not succeed to adjust the content and level of detail to the main scope of the study. In the abstract and conclusions, the authors state that the aim of the study is to propose a new bias correction method. But the focus is very blurred. Two GCMs and an RCM are considered and a lot of detail is discussed in the paper (e.g., model weighting) which is not necessary to explain the method but rather distracts. For a serious case study of the particular catchment, however, the study is by far not comprehensive enough as uncertainties due to model errors are not sampled sufficiently (2GCMs, 1RCM). Here the authors should make a clear choice and delete everything which is not needed! I would suggest to use only one GCM.

Third, the downscaling directly from a GCM in the current application is highly questionable. Climate change trends in precipitation, in particular in mountaineous terrain as in the study area, might strongly vary locally. The GCM, however, produces an area average trend for a large area that would be inherited by the local downscaled series. Also the interpolation as done by the authors would not improve this caveat. Also note that interpolation is not downscaling! There is a fundamental and important difference between the two.

Fourth, several concepts are muddled and important state-of-the-art studies are not considered. The approach is not a bias correction, if I understand it correctly, as it does not use simulated future time series directly and bias corrects it. So the method is not a delta change method. It rather uses future simulated time series to estimate change factors. (delta change and change factor are similar, but not the same). In the literature these methods are typically called change factor weather generators (e.g., Kilsby et al., Env. Mod. Soft. 2007). Also, if the delta change method (please do not call it perturbation method, perturbations are a clearly defined concept in physics) is to be introduced, please also discuss its massive drawbacks (Deque et al., 2007; Maraun et al, Rev. Geophys. 2010). A major issue which looks quite muddled is the discussion of scale issues. The authors point out differences between grid scale and point scale precipitation at several places, but the explanations are far from being precise (e.g., page 6850, last paragraph, section 4.5., first paragraph). For a recent discussion of scale issues in the context of downscaling, please have a look at Maraun, J. Climate, 2013. Also, consider that interannual variability is a function of scale and might in fact very well be represented by a GCM (e.g., Eden et al. , J. Climate, 2012). Other issues concern the already mentioned mixing up of downscaling and interpolation, but also prediction and projection or GCM node vs. grid box. To clarify some concepts I would recommend the review paper by Maraun et al., Rev. Geophys., 2010. It could well be cited in the introduction when discussing statistical downscaling methods (page 6849,

last paragraph, page 6850, first paragraph).

Fifth, the validation setup is not clear at all. In the given context, a cross validation seems to be impossible, as no real prediction is carried out over the observed time period (the model is stationary). Thus, any validation should be trivial, as the agreement with observations is obtained by calibration. I have not found any introduction to the validation setup in the paper, maybe I just missed it.

Other comments: Why do you simulate a year with 48 weeks? How do you validate this against observations?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 6847, 2013.