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Interactive Comment

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 #2

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The manuscript presents a weather generator (WG) for weekly precipitation at several stations in the Nam Ngum River Basin in Laos. The WG is based on a Markov chain and for estimating time series in the second half of the 21st century change factors (CFs) from SRES A2 simulations with CGCM3.1 and ECHAM5, both with a T63 resolution, are applied. In contrast to a previous paper by Kim et al. (2008), the CFs are not calculated by interpolating the raw GCM CFs to the station locations but by first applying a parametric quantile mapping (QM) to downscale the GCM precipitation to the station locations. For the fitting period 1961-2000 the WG results are also compared

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to raw output from the PRECIS RCM driven by ECHAM4.

In my view the manuscript is clearly not suited for publication because neither the content nor the writing style are of sufficient quality.

The writing style is very poor and wordy, and the paper lacks focus and structure as well as precision in specific formulations so that even after reading the manuscript several times I found it very difficult to find out what exactly had been done and what is new relative to the Kim et al. (2008) paper. As I am an expert for statistical downscaling and bias correction I think it is safe to assume that other readers would have similar problems. There are too many unclear points to be listed comprehensively, but some of the more important ones are included in the specific comments below.

The study does not have a clear scientific focus and merely presents in a rather unsystematic and superficial way the results of the approach outlined above. I cannot see anything in the content that is novel and of enough general interest to justify publication. The Markov chain, CFs and QM are all standard approaches and their use alone does not provide any new knowledge. The only slight novelty is the approach taken to calculating the CFs by first applying a parametric QM. However the question to what extent and why QM or any other form of bias correction does actually affect CFs is not discussed at all and the whole approach is ad hoc and not supported by a critical analysis. Moreover the explanation is partly incomplete and confusing. Even if the somewhat non-standard CF calculation was discussed in more depth, it would not provide enough scientific substance for a publication. A serious problem of the analysis is that the validation of the methods, which constitutes a large part of the results section, is naïve as it does not distinguish between properties that the estimated series have by construction and those that are non-trivial; for instance the fact that a bias-correction method (such as QM) effectively reduces biases in the fitting period can hardly be considered as true information.

The low quality of the manuscript might be partly due to the fact that the authors seem

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to be unaware of many recent papers on statistical downscaling, bias correction and change factors (e.g. Maraun et al. 2010, Rev. Geophys., 48; Kilsby et al. 2007, Env. Mod. Soft, 22; Christensen et al. 2008, Geophys. Res. Lett. 35; Piani et al. 2009, Theor. Appl. Climatol., 99; Dosio and Paruolo 2011, J. Geophys. Res. 116; Themessl et al. 2011, Int. J. Climatol. 31; Themessl et al. 2012, Clim. Change, 112; Maraun 2013, J. Clim., 26) and on key papers on GCM bias correction (Widmann et al. 2003, J. Clim. 16; Eden at al. 2012, J. Clim. 25).

Some specific comments (list is not comprehensive).

1.) The problem with GCM precipitation is not only that is does not provide sub-grid-scale detail, as stated in the introduction, but also that there can be large biases on the resolved scales (Widmann et al. 2002, Eden et al. 2012).

2.) The explanation of downscaling models in the introduction does not distinguish between Perfect Prog and Model Output Statistics/bias correction methods. For a recent review see Maraun et al. (2010)

3.) WGs with CFs are also used within UKCP09, which is fairly well known and should have been mentioned.

4.) The explanation of the Kim et al. (2008) CF calculation is imprecise; what does 'spatially downscale' mean? From what is said later it seems it is a spatial interpolation, but it could be anything.

5.) Page 6850, penultimate sentence 'To compare the rainfall distribution . . .' 'Compare' with what?

6.) Same sentence: What is the link between QM and the question of potentially different trends at the local and the grid-cell scale? To a good approximation QM can be described as a combination of an addition of a bias with a multiplicative scaling. The former changes CFs the latter doesn't. Whether a change in relative (CFs) or absolute differences between two periods (or of trends) is desirable is not a priori clear (see e.g.

Maraun 2013). As the QM prior to calculating the CF seems to be the key novelty in the manuscript a much deeper and critical discussion of these questions would have been needed.

7.) Section 3: There are repetitions of text, poor structure and unclear formulations with respect to data sources.

8.) Section 4.1: There are repetitions with respect to the correlation between the rainfall at Luang Prabang and streamflow.

9.) The nomenclature for explaining the Markov Process is partly confusing, t and n are both used for time. The idea is probably that one is continuous and the other discrete, but this should have been said more clearly and could have been handled better by using t_k. The definition of the conditional probabilities makes them dependent on the week w, but on the left hand side of the equations this is not made clear. The definition of N is not clear; if as it is said the range is divided by the standard deviation the result is usually a non-integer number, how can this be N, which is an integer? The states are sometimes denoted by 'i' and sometimes by S_i.

10.) It does not become clear how the transition probabilities are estimated. There is only the rather trivial information that the probabilities need to be between 0 and 1 and add up to one.

11.) Page 6854: I find the explanation of the generation of two (why two?) sets of random series very confusing, much of the notation is unclear.

12.) Section 4.5.: It does not said clearly that the bias correction is implemented as a QM and as said above the key question whether QM leads to more realistic local CFs is not systematically discussed.

13.) Page 6864, second paragraph: What is the inverse distance weighting applied to? I thought the estimates for the stations are obtained through QM.

14.) As mentioned above the whole results section is superficial and it is often rather unclear why certain aspects of the methods are being validated.

I have written most of this review before I saw the comments by reviewer 1. I agree with many of his or her comments and will therefore not add further detail to my comments.

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