Review of the manuscript submitted to HESS
Estimating monthly rainfall in rural river basins under climate change: an improved bias-correcting statistical downscaling approach
By D. L. Jayasekera1 and J. J. Kaluarachchi

The work presented in the manuscript describes an application of a mixture of methods to downscale and bias correct the outputs from climate models to station sites. A multi-states Markov process is first applied to simulate the temporal evolution of rainfall in a key station, than other multi-states Markov processes are applied to condition (at fixed time) rainfall depths in the other stations to the rainfall already determined in the key station (with the first Markov process). Then a bias correction based on pdf matching of best-fitted parametric distributions of historical and Climate Models series is applied on the outputs of the Markov processes. Finally the bias corrected series are perturbed on the basis of the trend observed in climate series.

In general, many parts of the paper are unclear. The most critical assumptions are not properly explained, motivated and supported.

The authors used a multi-state Markov processes. Rather than applying the classical two state approach (wet vs dry state), they define a fist status corresponding to zero rainfall, and the second, third, … status corresponding to rainfall occurrence within different intervals of rainfall depths. Note that, in general, neglecting the rounding due to the sampling discretization, rainfall values can be considered as continuous variables. It is not clear from the paper how a rainfall state (i.e. a rainfall occurrence within a certain interval associated to the current state) can be converted in a continuous value. I could not understand from the paper: either it is not clear, or it is not explained. Even if the status is not converted into a continuous variable, I am very skeptic about this approach which leads to an over-parameterization of the process: as a matter of fact, if N is the number of states, one has to determine NxN transition probabilities for the key station and NxN conditional probabilities (at fixed time) for each other station.
Honestly I believe that there are many methods based on parametric continuous distributions that are less parameterized and can be better calibrated. Moreover the database used is quite poor, 40 stations, but only a few can cover the considered timeline 1961-2000.

Although I am quite familiar with statistics, and even with fractal and multifractals (and the different kinds of dimensions), I was not able to understand equation (2), what is here the meaning of dimension, or why dimension d is set to 1000.

I have several other parts of my manuscript annotated with question marks because unclear or badly written. Thus, even ignoring my opinion on the approach, the paper is not suitable for publication.