Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-639-RC2, 2021

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Impact of bias nonstationarity on the performance of uni- and multivariate bias-adjusting methods" *by* Jorn Van de Velde et al.

Anonymous Referee #2

Received and published: 29 January 2021

This manuscript evaluated the impact of bias nonstationarity on the performance of uni- and multivariate bias correction methods. Specifically, five bias correction methods (including one univariate bias correction method QDM, one delta change method mQDM, and three multivariate methods MBCn, MRQNBC, and dOTC) were compared in terms of correcting the precipitation, potential evaporation, and temperature. The authors concluded that these newly proposed multivariate bias correction methods may perform worse than the commonly used univariate method in both climate and hydrology perspectives, due to the bias nonstationarity of climate model simulations. The topic is very interesting and is also important in climate and hydrology communities. However, I found the manuscript is hard to read, especially the experiment design and

the conclusion may be unreliable. My major comments are as follows

1. The authors did not explicitly present the study area of this paper. After looking for a while, I'm surprised to find that only one station and one grid cell were used (lines 115-143). In addition, there is only one GCM-RCM combination was used, and no information on its spatial resolution. As such, the results of this study are subjected to large uncertainty. 2. The manuscript is lengthy and hard to follow. I think some information does not need to be presented in detailed in the manuscript. For example, the introduction of the bias correction methods (section 3.1 to 3.3, almost 14 pages of the main body). All these information can be found in literatures. 3. Although the authors have stated in line 527 "As the effect on discharge is the overarching goal of this paper", I think the information on the hydrological model and hydrological simulation this quite poor in the manuscript. Firstly, I did not find how the hydrological model performed in the study area (e.g. Nash value or some other criteria). Although the goal of this study is to compare the difference between the univariate method and multivariate method in the hydrology perspective, the hydrological model should be well calibrated. Secondly, as is stated that the PDM model was not calibrated in Uccle but Grote Nete watershed, please give the evidence to show that it is feasible to drive the PDM using the climate data in Uccle. 4. The authors used scatter plots throughout the whole paper. It is difficult to see how many dots are located within the 0-1 square or to do the comparison (e.g. Figure 6). I suggest that maybe the authors can use some more quantitative metrics and figures to show the results. 5. Figure 3 to figure 9, the authors did not mention which period (calibration or validation) and which month the results are based, or is it based on the whole year? In this case, readers cannot understand and assess the results. For example, for the correlations between two variables (e.g. P and T), the correlation coefficients are quite different for each month (e.g. summer and winter), therefore, at least, they should be evaluated separately for each month. 6. Table 1, I am wondering whether the Spearman correlation coefficients and lag-0 cross-correlation between two variables reflect the same thing. 7. In general, I found that many expressions in the current manuscript are not very accurate. For

example, in line 568 the authors write “A surprising result for P is the high RBMB value for P99.5 for MRQNBC”, but I found in figure 3 that the corresponding value for MRQNBC is quite small. Therefore, I’m quite confused by many expressions in the current manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-639, 2020.