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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Effects of univariate and multivariate bias correction on hydrological impact projections in alpine catchments" *by* Judith Meyer et al.

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The work by Meyer et al. presents an inter-comparison of a univariate and a multivariate bias correction (BC) method in terms of hydrological climate impact scenarios in two catchments of the Swiss Alps. For this purpose, daily temperature and precipitation amounts as simulated by ten EURO-CORDEX RCM experiments are bias-corrected toward observed catchment mean values and then fed into the HBV-light hydrological model. For BC the QDM and MBCn methods are employed, the latter taking explicitly into account variable interdependencies. The study finds important differences in the simulated streamflow for a historical period between QDM- and MBCn-based setups.

In general, shows MBCn shows a better performance. The main reason is an underestimation of snowfall amounts in the QDM-based setups (equivalent to a smaller snowfall fraction of total precipitation) which translates into smaller SWE amounts and an overestimation of winter streamflow while the spring meltwater peak is underestimated. The differences in the snowfall amounts between the two BC approaches furthermore translate into differences in future climate change signals of SWE, glacier coverage and, finally, streamflow. Qualitatively, the differences between the BC approaches are obtained for all ten climate model chains investigated, indicating a robust finding that seems to be valid for any GCM-RCM chain.

In general, the paper is of very high quality and nicely highlights an important potential deficiency of bias-corrected climate scenarios in the Alpine region. It comes at a perfect time, as several recently released national reference scenarios are based on univariate BC approaches similar to QDM (e.g., Austria: ÖKS15, Switzerland: CH2018). As such, the study is certainly relevant for the journal's readership. Its setup is sound and convincing, the results are presented in an appropriate manner and the conclusions are well-based on the results obtained. There are no language issues except for the mixed use of past and present tense in the presentation of the results, which should be revised. There are a few minor issues that should be corrected for as well as two major remarks (see below). However, I'd leave it up to the authors to consider these major comments or not. I believe a consideration would further improve the quality of the paper, but the study is sound and convincing even in its current state. My recommendation is therefore to return the paper to the authors for minor revisions.

Congratulations for this nice piece of work! Sven Kotlarski

MAJOR ISSUES

Cross validation: Similarly to the point raised by a previous reviewer, I believe that a proper cross validation framework would be helpful. MBCn is a more complex method than QDM, and there's an increased danger of overfitting. As MBCn explicitly cor-

rects for biased inter-variable dependencies, snowfall amounts (if derived by a fixed temperature threshold) are well represented by definition. Being aware of the criticism by Maraun & Widmann, cross validation still makes sense in a split sample framework, e.g. by separating the historical period into the 15 coldest/warmest/driest/wettest years and the 15 warmest/coldest/wettest/driest years and using these subsets for calibration and verification, respectively. In case this splitting cannot be handled by HBV-light because the transient character is lost, one could carry out a cross validation for at least one ERA-Interim EURO-CORDEX experiment (these experiments are available as well and are in basic temporal correspondence with the observations). In general a cross validation would make the point stronger that a multivariate BC is superior for the example presented.

Reason for underestimated snowfall amounts by QDM: I understand that the paper puts an emphasis on the hydrological consequences of the two different BC methods. These effects are very well and convincingly presented. However, the question WHY QDM shows these deficiencies is not ultimately answered. The reason is to be found in the T-P relationship of the QDM data, and probably already appears in the raw RCM data. To analyze this further, 2D histograms would be extremely helpful and also illustrative.

MINOR ISSUES

Introduction and conclusions: The literature review should account for the studies by Wilcke et al. (Climatic Change, 2013) and Ivanov&Kotlarski (Int. J. Climatol., 2017). Inter-variable dependencies in standard QM have already been analyzed in there. One of the results was that QM does not distort inter-variable dependencies as long as they are approximately represented by the raw RCM data. The results of the present work therefore indicate some distorted inter-dependencies already in the RCM raw output (which could be better described if my major comment #2 would be considered). These issues should also be discussed in the discussion/conclusions.

p2 l20: "... which correct for biases in the data's entire distribution..."

p2 l4: The CORDEX data are actually not available from the CH2018 archive. The respective website only explains the selection of EURO-CORDEX models for the CH2018 Swiss climate scenarios. In the present study, EURO-CORDEX data were probably obtained from the ESGF archive.

Table 2: Just a note: The two runs driven by CNRM-CM5 are critical as the driving GCM CNRM-CM5 has an inconsistency in the historical period. It is fine to use them for the present work, but in future works they might have to be removed. More information is available from the new EURO-CORDEX errata page available from www.euro-cordex.net.

Chapter 3.1: The description of the QM methods is incomplete in the sense that it is not clear if the correction has been carried out for the bulk series (all days independent of the time of year) or depending on the time of year (e.g., seasonal or DOY dependence). This information is critical, as a bulk correction could be responsible for the deficiencies of QDM in my opinion. I believe the authors employed a seasonally dependent BC, but this needs to be better explained (even if reference to Cannon et al. is provided).

p6 l20-21: "... until the multivariate distributions of bias-corrected and observed data match."

p11 l30-31. Do you have any explanation for the higher mean streamflow amounts for QDM? Are differences in ETP involved?

p14 l24: "...disappearance vary by over a decade ..."

p15 l31: "...empirical-statistical bias correction methods ..."

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