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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 manuscript "Effects of univariate and multivariate bias correction on hydrological impact projections in alpine catchments" by Meyer et al. investigates the effect of univariate versus (BC) multivariate bias correction (MBC) on the representation of snow, ice and rain representation in a climate change impact assessment approach. MBC has the advantage to control for variable interdependency (T and P namely) that in turn influences rain-snowfall fractioning. They used BC and MBC to bias correct and downscale 10 GCM-RCM combination using quantile mapping and drove the hydrological model HBV-light in a transient setting (1976-2099). They analyzed the effect of

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the different methods for snow-water-equivalent, icemelt, streamflow amounts and its composition (ice, snow, rain) over time. The paper adds to the ongoing discussion on the possible effects of intervariable dependency, especially by adding the information about the effect of streamflow composition.

The paper is scientifically interesting, original, overall very well writing and certainly within the scope of the journal and of interest to the readers. Texts and figures are widely clear and lead the way for reasonable conclusions.

Beside some minor comments that I will list below, I have two more substantial concerns that refer to the design of the study.

1. The hydrological model was calibrated for the entire reference time period, e.g. 1976 – 2006, (page 7, line 16 ff) against streamflow, snow (SWE and snow-covered area), and glacier volume.

Three things puzzles me here:

1.1 There is no validation period!

1.2 Model performances against observation are not presented at all, neither as statistical measure nor in the graphs. I am aware that only differences between input data sets are analyzed in this study, still the basic performance measures are needed to frame the results. E.g. if the representation snow melt is not well captured (what I do not assume here) but the streamflow is (hence snow melt insensitive) than the snow sensitivity to changes in the input data might also be underrated.

1.3 this concern about validation and performance measures also extends to both QM approaches (was a cross-validation framework used? provision of verification statistics is needed)

–> more emphasis should be laid on the presentation of validation and the introduction of a validation period/validation framework is required

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2. If I understand correctly, the combination of climate model data and the hydrological model is as follows:

- The quantile mapping is performed between climate model output and the average catchment value for T and P

- This mean value is interpolated within the catchment by a lapse rate that is fixed for each day of the year (extracted from the reference period)

This approach might be needed in HBV-light, but is based on the assumption that the lapse rate is not changing over time and is independent of certain events. This is a very strong assumption that is disproven by numerous study showing e.g. elevation depended warming, for instance. I assume, and to my own experience, that the slope of the lapse rate is quite sensitive to SWE simulations. Hence, this strong assumption likely influences the robustness of your results. Furthermore, you cannot control for this in the calibration of the model, as you limited the evaluation of model performances of SWE and snow-covered area to 2000 – 2500 m asl (page 7, line 21), which is exactly the catchment mean elevation for which the lapse rate is of minor effect. To me the fixed lapse rate is a very critical points in the study and need to be solved.

A possible workaround of this issue would be to perform a quantile mapping that is not based on catchment mean values, but for each grid cell of the HYRAS data set. This procedure is currently done for the new CH2018 climate scenarios by MeteoSwiss. Doing so, you can extract the lapse rate for each day separately and use this dynamic lapse rate. With this procedure you would not have to make this strong assumption of a static lapse rate and have much more reliable results.

I think that these proposed changes are accomplishable in a reasonable time. Therefore, if those and the following comments are addressed, I am happy to comment on the manuscript again and likely recommend a publication. I am looking forward to the revised version.

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Specific comments:

Page 1, line 23ff. I suggest to state this result (“for the historical..:”) prior to the effect on the future as this ensures an improved bias correction for MBC.

Page 2, line 24: This publication might also be of interest (I am not an author): Wilcke, Renate Anna Irma; Mendlik, Thomas; Gobiet, Andreas (2013): Multi-variable error correction of regional climate models. In: Climatic Change 120 (4), S. 871–887. DOI: 10.1007/s10584-013-0845-x.

Page 4 line 2: Only the “Unterer Grindelwald”-glacier is big (∼6.biggest in Switzerland). It is a glaciated catchment, but covered by smaller glaciers. Please, rephrase. Therefor, also the following sentence needs to be rephrased.

Page 5, line 7ff: Please highlight that you apply catchment averages and that these averages are the “Target” in the quantile mapping approach (If I understood you correctly)

Page 5, line 10: This is a very unusual time period as it crosses to climate normal periods. Do you have any reason of this time window. It hampers comparability to other climate change impact assessment studies.

Page 5, line 13. Which gauging station was used. I am only aware of the FOEN station in Lütschine-Gsteig, and the Weisse Lütschine, Zweilütschinen. Did you use differences of these stations?

Page 6, line 6: This is phrased wrongly. Univariate QDM cannot be both widely accepted (and used since several years) and developed by Cannon et al. 2015.

Page 6, line 9: detrending of a time series is problematic, as assumptions about the kind of trend are necessary. Can you please add information about the way the trend is treated and comment on possible effects.

Page 6, chapter 3.1: additional information about the validation procedure should be

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given as well as information about the “target value” (catchment averages)

Page 6, line 22: “Climate model data” instead of data

Page 6, line 28: Please, quantify the difference by adding grid cell size and range of catchment area.

Page 7, line 16: so no separation of calibration and validation time period ! see general comments

Page 7, line 21-22: is it correct that you not only evaluate but calibrate your model against this elevation limitation? Please rephrase.

Page 7, line 21-22: I disagree with the statement that only the area 2000-2500 is crucial as in my experience it is also very important for streamflow how much of the entire catchment is covered by snow – and hence contribute to snow melt. Please, comment.

Page 8, Figure 2: What is also striking is that noBC is performing better than QDM for rainfall. Can you add on this?

Page 10, Figure 4: Maybe an error occurred, as the boxplots in the lower panel of the Schwarze Lütschine graph is missing.

Page 14, line 22: Please rephrase: It depends not on the bias correction but more specifically on the consideration of intervariable dependencies.

Page 14, line 24: Are the found glacier retreat comparable to other findings?

Page 15, line 8: Much more critical to me is the assumption of a fixed lapse rate, even more under climate change conditions

Page 15, line 34: is there a type? “re bias ”

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