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Geoscientific Model Development Discussions

# *Interactive comment on* "A new bias-correction method for precipitation over complex terrain suitable for different climate states" *by* Patricio Velasquez et al.

**Anonymous Referee #1**

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The manuscript of Velasquez et al introduces and validates a new method to correct systematic precipitation biases of a regional climate model simulation in the complex terrain of the Alps. The correction is largely based on the quantile mapping method but explicitly takes into account orographic characteristics such as elevation and slope aspect. The 30-year long climate simulation is driven by a global climate model simulation and several intermediate regional nests. Simulated precipitation amounts are corrected towards two gridded precipitation datasets covering Switzerland and the Greater Alpine Region, respectively. The model evaluation, in general, shows a satisfying performance.



The topic of the work fits well into the journal's scope and is, in principle, relevant for its readership. However, in my opinion the manuscript suffers from a number of severe shortcomings and misunderstandings. These definitely need to be improved before a publication can be recommended. I'm listing the major as well as a few minor points below. The mentioned issues need to be worked on, and my recommendation is therefore to return the manuscript to the authors for major revision. I hope my comments are helpful to further improve the work.

Major points

- The underlying assumption of the presented exercise is that orographically defined classes are informative for the model's precipitation bias. In my opinion, this has not yet been convincingly shown. What would be required, for instance, is an analysis of the range of model biases WITHIN the individual orographic classes. Do classes separate from each other in such an analysis? Figure 7 provides an indication that this is not the case, as the spatial correlation does not systematically improve after application of the bias correction.

- As stated by the authors, the rationale behind the newly developed method is that bias correction would be possible for paleo climatic states subject to a different land surface topography (Alpine ice shield, for instance). There is a considerable danger that applying a correction method that is trained in today's climate does not hold for such a climatic state even if orography is considered as a co-variate in the bias correction. Large scale flow conditions, for instance, could be strongly different from today's conditions leading to a completely different bias structure even for the same orography class. Also, in a much colder climate the relation of snowfall to liquid precipitation would increase which might, in turn, lead to completely different model biases even for the same orographic class. To show that the assumption is valid, one would have to go much further with the modelling exercise. One could, for instance, carry out a second simulation with the very same GCM forcing but a modified Alpine topography in the RCM, and then apply the bias correction calibrated in the standard simulation with true orography. Would the bias-correction produce a realistic precipitation pattern in such a disturbed simulation?

- The introduction definitely needs to be worked on and be streamlined. It currently includes quite some repetition, and the line of argumentation is not always straight. Some basic references (for instance on the evaluation of CORDEX experiments in Europe and over the Alps) are missing.

- At several points in the paper the authors mention that the traditional QM approach would calibrate one correction function for the entire domain. This is certainly not true. In a pure bias correction setting (raw grid = target grid) a separate correction function is calibrated for each individual grid cell.

- The reason for the second bias correction step (first part of local intensity scaling) remains completely unclear to me. The third step (QM) would account for this already (by adjusting the percentiles).

- The general setup of the bias correction remains unclear. Is the correction carried out grid cell by grid cell, or in a bulk manner for each orographic class?

- Figure 3 is unclear. What do the boxplots represent and what is the true y-axis scale? Do the boxplots cover the spatial variability of monthly mean precipitation for the entire domain (a) or the elevation classes (b,c)? The text mentions that daily precipitation variability is shown, but how does this aggregate to monthly precipitation (y-axis label) then? If boxplots really show the distribution of daily precipitation values does it really make sense to use the IQR? Depending on the wet day frequency more than 25% of the days might be dry, for instance.

- Also the general validation setup remains unclear to some extent, the validation technique and the respective reference datasets used needs to be better described. It is sometimes unclear whether the Swiss 2 km serves as reference or the Alpine 5 km grid.



- Any kind of bias correction will only be as good and as appropriate as the observational reference. The validity of an analysis of elevation dependencies and slope dependencies at regional scales in the gridded observational precipitation datasets needs to be discussed. Does the reference grid really represent such dependencies?

- The application of the Ext-TFs mixes spatial scales (classes based on 5 km orography vs. classes based on 2 km orography). This is potentially dangerous and the effects of this mismatch should be shown. Why is the validation, in this case, not carried out on the 5 km scale as well?

Minor points

- page 1 line 19: “is” instead of “has been”

- page 2 line 20: What is meant by “weaker intensity” here? Unclear.

- page 2 lines 16-19: Line of argumentation unclear. RCMs were already referred to just above (line 12ff)

- page 4 lines 1-2: No true in general. Ban et al. for instance show that mean precipitation can also be much worse in convection resolving experiments. Certain aspects (such as the diurnal cycle) are improved, but not all.

- page 4 lines 4-7: I don’t really understand the reason behind this splitting in ten single 3-year simulations. 2 months spin up is certainly not enough for soil parameters and snow. Some more information on the setup and on the rationale behind it needs to be provided.

- page 4 lines 19-20: I guess this is hardly true. In areas where no observations are available gridded products can be subject to very high uncertainties as inter- and extrapolation are required here.

- page 5 lines 4-9: It remains unclear how these classes are computed. Based on the relation of a grid cell to its 8 direct neighbor grid cells? Please clarify.

- page 5 lines 15-17: Which threshold is then used in the present work?

- page 7 lines 30-32: This explanation seems to be not very likely given the turnaround time of atmospheric water vapor (a couple of days only). Water vapor should also frequently be resupplied by the boundary forcing of the RCM. Can you back this up by some reference?

- Figure 1: Why are Italy and Slovenia excluded from the Ext-TF analysis? They are part of the APGD dataset.

- Figures 4 and 5: Sorry, but it is unclear to me which bias is shown in these two figures. Bias of the IQR of daily precipitation amount sin Figure 5? Which intensity in Figure 4? Mean wet day intensity? Needs to be better explained.

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