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Interactive comment on "The downscaling and adjustment method ADAMONT v1.0 for climate projections in mountainous regions applicable to energy balance land surface models" *by* D. Verfaillie et al.

Anonymous Referee #2

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This paper describes a new statistical adjustment method intended to correct the biases in regional climate simulations in order to force land surface models in mountainous regions, and its application over the French Alps. The method is applied to the results of a RCM simulation forced by an atmospheric reanalysis. Precipitation and temperature after correction, and snow cover after land surface modelling with corrected forcing variables are compared to observations.

The paper could be an interesting and useful addition to the field. The adjustment method is sound, and the evaluation work is serious. It may be publishable after major

revisions. However, the description of the method needs to be much improved and the authors need to totally rethink how they present the results of the evaluation, with much less figures, but that better synthesize the results (see my major comment bellow). Moreover, the authors also need to demonstrate that the novelties of the adjustment method (quantile-quantile mapping that depends on large scale circulation; method used for the temporal downscaling from daily to hourly outputs) are useful. I also think that the English is not very good, and need to be improved.

General comments

The paper is not particularly well written (despite visible efforts), with long and awkward sentences that sometimes make the paper difficult to understand.

Some important methodological aspects of the proposed adjustment method are not well described and sometimes not described at all. For example, the basic quantile-quantile mapping algorithm is not described precisely. In the description of the adjustment method, the authors simply describe the different steps very factually, but don't give the precise objective of the step (which is not always obvious) and very seldom justify the proposed solution (see my specific remarks).

The authors have produced a very large number of figures (28 figures with a very large number of sub-figures. In the end, we have hundred of illustrations and even more in supplementary materials). The sub-figures are often very small and therefore difficult (and sometimes impossible) to read. I think it is the job of the authors to do an effort to synthesize their results with a limited number of relevant illustrations, and to only show the important results (at least in the main paper): I disagree with the approach that consists in producing as much as possible illustrations and letting the reader finds what is important.

The core of the adjustment method, quantile-quantile mapping, is well known and has been widely used. An originality of the approach proposed in the paper (even if it is not really the first time it is used, as noted by the authors) is to apply the quantile-quantile mapping by regime of large-scale circulation. Unfortunately, the authors do not demonstrate the interest of this approach. Is it really useful to do that? A second originality is the method used to obtain hourly data from the adjusted daily RCM output, which is often a necessary step to be able to force a land surface model. The authors propose a quite sophisticated approach, but do not evaluate its interest compared to simpler approaches (e.g. daily cycle from an analogous day without adjustment, or climatological diurnal cycle), neither directly (using observations with hourly resolution, I'm sure that some are available on the study area) nor indirectly (for example by comparing the simulated snow cover obtained with the different approaches). The authors should demonstrate that the novelties they introduce are really useful. It would significantly reinforce the interest of the paper.

Specific remarks

L67-68. I'm not sure to understand. It depends on how one deals with the distribution tail, I think.

L98-102. Unclear and awkward sentence.

L102-104. Not clear

L127-128. OK, but in the end, the adjustment method is intended to correct the output of classical RCM projections such as the ones from Euro-Cordex. A smaller domain likely results in smaller biases compared to the biases found in typical RCM projections. Therefore the evaluation shown in this paper does not demonstrate that the adjustment method is able to deal correctly with the larger biases from classical RCM projections. I think it is a limitation of this work that should be pointed (including in the conclusion).

L130-144. The authors need to explain what exactly is SAFRAN, how the values at different elevations are obtained etc. It may help to better understand some of their choices for the adjustment method. They also talk about the "centroids" of SAFRAN massifs. How are the centroids defined, and what do they represent?

Part 2.3. It would be good to give the forcing variables, their time step etc. in this section.

L165. "Centroids": see a previous remark.

L173. Please provide a more precise description about the exact algorithm used for the quantile-quantile mapping. Only a very brief general idea is given for the moment. For example, how many quantiles are used? How does it work for the values between quantiles: is a linear interpolation is used? How does it work for the values greater than the higher quantile? How the fact that the probability of precipitation in the RCM is different than in SAFRAN is dealt with?

L174. Plotting? I hope that the authors do not really plot the simulated quantiles versus the observed quantiles.

L179. Most adjustment methods make the same hypothesis. . .

L187-194. For each massif, the authors use a single RCM grid point, the closest (either horizontally or also taking into account the vertical distance) of the massif centroid. Another solution, maybe better, would be to use all the RCM grid points within a massif, and use, based on their altitude, the most appropriate point for each elevation band within the massif. Another possibility, a priori more logical than the single point approach of the authors, would be to average all the RCM grid points within a massif. Obviously, the statistical properties of the spatial average are not the same than for a single point, but the values from SAFRAN on a massif are already spatial averages, right? I think it could make more sense. In any case, the authors need to justify their approach.

L201. Hourly to daily what?

L203-204. What do the authors mean by "each point". Each centroid? Or each elevation band within a massif? If they mean elevation band, using the word "point" is confusing.

L204. I'm not sure to understand why this precision is necessary at this point.

L210. I don't really understand how the "analogous dates" work. The authors need to give the general rationale of their approach, justify the choices they made, and better explain the step. Is there just one analogous date used? Is it just one random date among all the dates that match the different criteria? What is the justification for these criteria? In what sense the date is really "analogous"? The authors could search for a real analogous date, with similar temperature and precipitation over the massif for example. The authors need to explain the rationale behind the use of a day "consistent" in terms of precipitation. And why do they look at the average of precipitation over the Alps and not at the average over the massif of interest? Why the consistency is only defined in terms of occurrence of rain? The intensity does not matter?

L234. How does the optimal value of alpha is chosen precisely? Is it the same at each point?

L255 (point 8 actually). I don't really understand step 8. It seems that, first, total precipitation is adjusted. Then there is a phase separation given temperature and then rainfall and snowfall are re-adjusted separately (only in a variant it seems later in the paper)? Please improve the clarity of the description of this step (rationale and methodology).

L279. I don't see in section 2.4 where the different learning periods are introduced.

L286 "determined only for each massif". I'm not sure to understand.

L349. The "evidenced"? The entire sentence is awkward.

L352. "average altitudinal gradient"? I see the averages for each elevation band in this figure: the gradients are not plotted.

L415. "After 1 month of integration"? This formulation is not very good, I think.

L472. It is really useful to plot hundred of time series (in the main document)? I think

some integrated scores would be much better. Temporal averages in addition to the correlations shown later would be largely sufficient, I think.

L504-505. I don't think that the good scores are mainly due to the adjustment method. The small size of the RCM domain is likely the main responsible for the good correlations. With a small domain, the RCM results are very constrained by the boundary conditions as noted by the authors in a different context. The affirmation is therefore misleading (and references would be needed in any case).

L550-551. Why? The authors do not explain how they deal with extremes values in their algorithm (there are many possibilities. . .). It is therefore difficult for the reader to understand this affirmation.

L564. The temporal transferability is only very partially tested. To my opinion, it is not a major problem that the mean state changes with the learning period. What really matters is whether the trends or the differences between two periods change with the reference period. This is not assessed in the paper, and I think this point should be made.

L630. As noted previously, it does not really make sense to compare the results of different adjustment methods applied to different domains (and RCMs). The differences of performances are more likely to result from the differences of models and domains than from the adjustment methods...

L652-656. As the sentence is written, one may think that the authors want to apply the adjustment method over the entire Europe. Is it really the case? (which data-set would be used instead of SAFRAN in this case?). Or, they simply want to use RCM simulations with a larger domain, as I suspect?

L657. "RCM model" : The M of RCM stands for model.

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