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Discussions

Interactive comment on "The representation of location by regional climate models in complex terrain" *by* D. Maraun and M. Widmann

Anonymous Referee #3

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General Comments:
In this manuscript, the authors present a new method to quantify the location representativeness of RCM simulation with respect to observations. The authors question the assumption of local representativeness of RCM simulation which is frequently assumed in statistical downscaling and bias correction. They show that the representativeness, which means comparability of temporal dynamics, can be shifted for seasonal precipitation sums in complex terrain such as the Alps during winter. This bias can be reduced to a large extent by representing the local observed grid cell with a non-local RCM grid cell, which could help in bias correcting and downscaling the RCM precipitation. The representativeness is quantified for the precipitation output of one regional climate model (KNMI RACMO) and the E-OBS dataset. It is based on quantifying the Pearson

correlation coefficient, which makes it also easy to apply. The presented results have broad implications for enhancing the skill of precipitation bias correction schemes and statistical downscaling methods. The manuscript is well written and the structure is clear. Overall, it is a welcome contribution to the field of precipitation modeling and downscaling. However, there are some issues regarding the method, conclusions, and quality of the Figures that need to be addressed before the manuscript can be considered for publication. I, therefore, recommend major revisions.

My main criticism is related to the measure of representativeness. The authors use in this study the Pearson correlation coefficient. This coefficient is a measure of linear dependence. It is very sensitive to deviations from a linear relationship (Wilks, 2011). The authors do not mention these limitations in the concept and data section and also do not discuss alternative measures. Alternative measures would be measures of rank correlation such as Spearman rank correlation or Kendall tau, which provide a more robust estimate of dependence. This point is of particular importance and needs to be carefully addressed because state-of-the-art bias correction methods such as quantile mapping do not rely on the assumption of linearity, but instead on the assumption that ranks are comparable.

The method is intended for bias correction of RCM data (p. 3020, l.1ff). The final goal of bias correction is to be applied during future periods. For this reason, stationarity of the relationships exploited for the bias correction needs to be assumed. The authors do not address this point. I suggest to discuss why the same non-local grid cell should be representative during future periods in a revision of the manuscript.

Specific Comments:
It is visible that some regions have a higher Pearson correlation coefficient in Fiugure 1 than in Figure 3 (first row). For example, central Spain for the winter season and parts of Russia located at the north-eastern border of the study domain during summer. This is surprising to me because the non-local representativeness should have increased the Pearson correlation coefficient and the maximized value should at least be that

of the local grid cell. Is the local grid cell (i, j) excluded from the maximization in Equation 3 and if so, why?

The panels in the center column of Figure 3 look very similar. As a matter of fact, I cannot see any difference between them. This is surprising because there are substantial differences in the other columns which is expected because of the significantly different impact of large scale circulation (i.e. boundary conditions) during summer and winter periods. Has there been an error in the plotting of these two panels or is there a reason why there are the same?

I did not understand the statistic shown in Figure 4. What is the unit? The authors should clarify, how the absolute trend bias is calculated (p. 3018, l. 28ff). This also relates to the discussion on p. 3020, l. 16ff.: The term "non-local bias correction" should be substituted by non-local representativeness because no bias correction has been processed in this study.

Technical comments:

The author should be circumspect with statements referring to RCM simulations (e.g., p. 3012, l. 4 or p. 3018, l. 21) because only one realization from one model has been investigated in this study.

I could not follow the arguments referring to the right panel of Figure 2 (p. 3017, l. 23ff) because the lines in this panel are very thin and the differences are hard to see. I would suggest to quantify the differences in one number to increase readability.

References:

Wilks, 2011, Statistical Methods in the Atmospheric Sciences, pp.627, Academic Press

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 3011, 2015.