Hydrology and
Earth System
Sciences
Discussions

Open Access

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

**Anonymous Referee #2**

Received and published: 12 April 2015

The manuscript by Maraun and Widmann presents an analysis and discussion of the local representativeness of precipitation as simulated by a regional climate model. Grid cell-based simulated seasonal precipitation sums in a reanalysis-driven RCM experiment are compared against seasonal precipitation estimates as represented by a gridded observational dataset. The temporal correlation of seasonal precipitation sums is considered as a measure for the representativeness of a specific RCM grid cell with respect to the precipitation climate as represented by the gridded observations. The analysis reveals high correlations in winter and lower correlations in summer which is explained by the lower signal-to-noise-ratio in summer due to a weaker synoptic forcing and a larger contribution of RCM-internal climate variability. For the same reason, correlations generally decrease from the western inflow boundary towards the East.

C969

Correlations (and thus local representativeness) are very low in the rain shadow of major topographic obstacles. The manuscript shows that this deficiency can to some extent be improved by selecting a more representative RCM grid cell. This also slightly improves the representation of seasonal precipitation trends.

The authors have identified a potential shortcoming of previous climate downscaling and bias correction efforts that often rely on the local representativeness of the simulated climate. They present an innovative approach to verify this issue. As such, the paper is of high relevance for the climate downscaling and climate impacts community and should be considered for publication. There are no language issues, and the presentation of methods and results is concise and appropriate for most parts. However, the manuscript still has a number of weaknesses for instance with respect to the overall scope of the proposed method (which seems to be somewhat exaggerated) or with respect to the missing discussion of alternative concepts of representativeness. Also, parts of the methodology (namely the cross validation setup) remain unclear. For this reason my suggestion is to return the manuscript to the authors and request a number of revisions, please see the listing below for details. The required revisions are major in their nature but could very well be covered by very few additional analysis and clarifications. I'd encourage the authors to work a little bit on these issues and to present a more suitable version of this important and relevant manuscript.

With kind regards.

MAJOR ISSUES

Effective climate model resolution and spatial averaging of climate model output: In the context of the presented manuscript, the issue of the effective resolution of climate models (e.g., Grasso BAMS 2000) is very relevant and should at least briefly be discussed. There's an ongoing debate in the climate model community whether climate model output for variables such as precipitation should really be analyzed at the grid cell level or if a spatial averaging is required prior to the analysis. Doing so could

C970

potentially improve the identified deficiencies in the local representativeness of climate model output. Furthermore, some approaches exist that indeed spatially average RCM output before applying it to impact models (e.g., Bosshard et al. HESS 2011). In the light of the presented results, would such approaches be favorable?

Measure for location representativeness: Only one method for quantifying the location representativeness (temporal correlation of seasonal precipitation sums) is presented. However, other concepts could be thought of but are not discussed. One measure might for instance be the representation of the seasonal or daily precipitation PDF after correcting for a mean model bias. Alternative methods do not necessarily need to be employed in the manuscript, but should at least be mentioned and briefly discussed since they could potentially lead to different results (and might have some advantages, see below).

Cross-validation setup and temporal stability of the most representative climate model grid cell (page 3015 lines 12-21): The cross-validation setup remains unclear to some extent, this section needs to be clarified. Apparently, the entire period is divided into four sub-periods, the most representative grid cell is determined for each combination of three periods and this time series is written into the remaining fourth validation sub-period. There's some danger that the resulting time series used for validation will consist of time series from different contributing grid cells, if the identified most representative grid cell changes from one 3x10/11 year block to another block. In my opinion this would be a shortcoming of the entire method as the spatial representativeness pattern is not stable in time. The authors should better clarify this point. Also, it would actually be nice to assess this temporal stability, i.e.: To what extent does the most representative RCM grid cell for a given observational grid cell depend on the analyzed time period?

Deficiencies of the gridded observations: It is well known that the EOBS dataset has deficiencies due to a rather coarse underlying station network. This is also true for the region of the European Alps. The spatial representativeness pattern shown in Figure

2 does therefore not necessarily result from a climate model problem in representing spatial precipitation variability, but could also result from deficiencies in EOBS. The authors need at least to verify if any precipitation station close or within the considered RCM grid cell (Domodossola) contributed to the EOBS analysis for that grid cell. If not and if, for instance, only stations to the North-West and across the Alpine ridge have been used, the resulting pattern shown in Figure 2 could simply be explained by a wrong local representativeness of the EOBS analysis itself. Another possibility would be to apply the gridded Alpine precipitation dataset of Isotta et al. (INT J CLIM 2014) that is based on a much larger number of stations and that could be aggregated to the RCM resolution to avoid spatial scale artefacts. According to their Fig. 2 there's at least one measurement station close to Domodossola (might be Domodossola itself).

Improvement of trends (Page 3018 line 28 to page 3019 line 7): Looking at Figure 2 (right panel) I'd argue that temporal trends in seasonal precipitation sums are very low and to a large extent masked by interannual and decadal climate variability. This is supported by the fact that the pattern shown in Figure is rather noisy and doesn't actually present systematic results. In my opinion this questions the entire analysis of trends in the present manuscript. In case the authors have a good reason to keep it in, it would at least be interesting to know if the most representativeness climate model grid cell for a given observational grid cell (determined according to the correlation measure) would also correspond to the grid cell with the best agreement of the seasonal precipitation trend (which I'd doubt very much).

Applicability of the approach and conclusions concerning GCM bias correction: The manuscript somehow provides the impression that the proposed method is an option for improving current downscaling and bias correction approaches by not choosing the climate grid cell located above the region of interest but the most representative grid cell. In my opinion this is no possible as in a climate change context one switches from a reanalysis-driven setup to a GCM-driven setup. Previous studies (e.g. Kerkhoff et al. J CLIM 2014) have shown that RCM biases do not linearly add up to the biases of

the driving data and strongly depend, for instance, on the driving GCM. This implies a strong influence of the potentially biased GCM large-scale circulation on the RCM results and, hence, on the spatial representativeness pattern. In other words: If a most representative RCM grid cell is identified in a reanalysis-driven setup, this might probably not be the most representative grid cell if the RCM is driven by some GCM. Furthermore, the identification of representativeness using the concept of temporal correlation is not possible in a GCM-driven setup. What would be the solution to this? The authors should at least discuss this point. It also concerns their conclusion that bias-correction of GCM results is questionable. Give the background explained above, the same conclusion (though somewhat weaker as topography is better represented by an RCM than by a GCM) would apply to the bias-correction of GCM-driven RCM output. One way to get out here might be the application of a different concept of defining spatial representativeness that could also be applied to GCM-driven setups (e.g. based on the marginal distribution).

Comparison of Figure 1 and Figure 3: Figure 3 shows the non-local representativeness as opposed to the local representativeness in Figure 1. While correlations are generally higher in Figure 3, some regions can be identified where non-local representativeness seems to be LOWER than local representativeness (e.g. some parts of Finland have a larger DJF correlation in Figure 1 than in Figure 3). This means, that the application of the non-local concept actually deteriorates the representativeness over these areas and does not maximize it. Is that right? And if so, why? Is it because the 11x11 grid cell search radius excludes the central grid cell (which coincides with the observational grid cell)? If so, I'd suggest to slightly modify the methodology and to also include that central grid cell to come up with the best correlation in the entire 11x11 grid cell radius (even if it is the local grid cell). In case the authors decide to stick to their methodology, they should at least mention this detail more prominently.

MINOR ISSUES

- Title: the term "by regional climate models" is too broad in my opinion as actually only

one individual RCM is investigated. Results could be different for other models. My suggestion would be to change to "by a regional climate model".

- Page 3012 line 6: "mountains".

- Page 3012 line 25 to page 3013 line 4: Downscaling approaches exist where

- Page 3015 line 12: Before describing the cross-validation setup (see also above) the total length of the investigated period should be introduced (1961-2000). Currently this information is only available further below.

———————————————————

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