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Interactive comment

Interactive comment on "Can local climate variability be explained by weather patterns? A multi-station evaluation for the Rhine basin" *by* Aline Murawski et al.

Anonymous Referee #2

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Main impression: The paper presents an evaluation of downscaling climate information over the Rhine region by making use of weather patterns. A number of different options are explored, and the conclusion is that best results are obtained with mixed predictors: sea-level pressure in addition to temperature and humidity fields. The main new aspects of this study include the specific emphasis on the Rhine region, large number of stations representing the local climate, and the long time series for searching for weather patterns.

One question I have with this analysis is whether the evaluation of the method is best done when making use of cross-validation of split-sample for calibration/evaluation.

Perhaps the main message has a tendency to get lost in all the details? This could

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be fixed with some revision and an emphasis/reminder of how the details support the main message. I think the abstract may be rewritten - a bit bolder - to make the paper look more interesting.

Details:

L15p2: “statistical approaches are comparatively cheap, computationally efficient and relatively easy to apply. . .”. No, it is not always easy to apply statistical downscaling in a good fashion that correctly captures the dependency to large-scale conditions. However, it’s easy to apply both dynamical and statistical downscaling to get some output - be it reliable results or non-representative numbers.

L28p2: “The underlying assumption of the downscaling based on weather patterns is that the regional or local behaviour of climate variables is a response to the larger-scale, synoptic forcing.“ More precisely, downscaling also works if only a fraction f(X) of the variability (which one would expect) is dependent on the large-scale conditions X (local processes n are also usually involved): $y = f(X) + n$. However, both large-scale dependent and local variability must be accounted for. One case in point is precipitation, as discussed in the paper.

L31p2: “Statistical downscaling tends to underestimate the variance of regional or local climate and may poorly represent extremes”. Some past studies have not accounted for the contribution local processes n, hence the variance in the results will be less than observed. Variance inflation is flawed and a priori gives incorrect results (von Storch, 1999).

L12p3: The assumption of stationarity is more severe for GCMs and RCMs, which rely on parameterisation schemes, involving statistically trained equation to represent the bulk description of unresolved quantities (e.g. cloud schemes). In GCMs/RCMs the results of such schemes feed back to the calculation of the large-scales, whereas for statistical downscaling/weather generators, they can produce a trend in biases. Also relevant for L15p19. When mentioning this only in relation with statistical downscaling

(SD), the reader gets a distorted picture and thinks it only affects SD - this has resulted in a myth within the downscaling community.

"Data:" Gridded observation (EOBS) and station data were mixed? This can introduce artifacts (spatial and temporal inhomogeneities). Furthermore, gridded daily precipitation is no good for analysing extreme precipitation, as the grid points are weighted sums of surrounding observations and hence are expected to exhibit different statistical characteristics (tail of distribution - see attached fig).

Also see http://www.icrc-cordex2016.org/images/pdf/Programme/presentations/parallel_D/D3_Chandler_CORDEX2016.pdf Furthermore, isn't EOBS limited to after 1950? Perhaps it's better to skip France altogether even if the picture is less complete?

"3.1 Weather pattern classification" - the paper discloses that the cost733 class software was applied to both reanalysis and GCM data (?) - but does that mean that the weather patterns are the same for the models and reanalyses?

"3.2 Finding optimal classification parameters" Keep in mind that with many tests, the likelihood of finding an accidental match increases. The "problem of multiplicity" - See Wilks (2006).

Eq. 3 - 5: Daily rainfall amount is far from normally distributed, whereas the root-mean-square metric is more appropriate for temperature, which tends to behave more like the normal distribution. TSS, WSS and BSS will be strongly affected by a few heavy precipitation events (acknowledged in 4.1.2), and explains low scores for the metric EV. For precipitation, it may be wise to look at aggregated statistics, eg seasonal wet-day mean (precipitation intensity), wet-day frequency, and probabilities (e.g. Benestad & Mezghani, 2015): The precipitation frequency exhibits a close connection with the circulation pattern (e.g. SLP), whereas the intensity is more complicated and is expected to be strongly affected by local small-scale processes (eg convection, which may be consistent with Fig 7 and mentioned in the discussion), but be somewhat moderated by large-scale conditions. Furthermore, the observations represent a poor sample - a rain

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gauge represents a few cm2 capture of a spatially heterogeneous phenomenon with a scale of km2. Aggregation in time or space may give a clearer picture that is less affected by sampling fluctuations. The alternative to downscaling single station data and then estimate the area average is to estimate the area average from observations and then downscale this index. I suggest adding some text about this possibility and these issues in the discussion, at least.

L3p10: The humidity estimate from reanalyses is difficult to validate - it may have substantial errors?

"4.2.2 Seasonality" - it's not clear what "the earliest and last months of occurrence in the course of the year" are and how they are specified.

"4.2.3 Persistence" - the duration of phenomenon/event/pattern may follow the geometric distribution, and differences in the models and reanalysis can be gauged based on its statistics. It can provide an estimate of what differences one would expect from randomness and what is likely a systematic bias.

Minor:

I would move the first sentence in the abstract to the beginning of the introduction. You don't need to explain why or provide justification in the abstract.

Second sentence in the abstract is a bit difficult, and can be rephrased or moved out of the abstract. It distracts the story away from the main findings. I'd start the abstract with "An objective classification scheme is presented . . ."

L28p4: "For the workflow proposed here three different sets of climate data are needed:" Comma between "here" and "three"?

L1p11: "The selected classification was compared to the Hess-Brezowsky-Grosswetterlagen" - Use "compared with" rather than "compared to" when there was an actual comparison?

References:

Storch, H. von. “On the Use of ‘Inflation’ in Statistical Downscaling.” Journal of Climate 12 (1999): 3505–6. Wilks, D. S. “On ‘Field Significance’ and the False Discovery Rate.” Journal of Applied Meteorology and Climatology 45, no. DOI: 10.1175/JAM2404.1 (2006): 1181–89. Benestad, Rasmus E., and Abdelkader Mezghani. “On Downscaling Probabilities for Heavy 24-Hour Precipitation Events at Seasonal-to-Decadal Scales.” Tellus A 67, no. 0 (March 30, 2015). doi:10.3402/tellusa.v67.25954.

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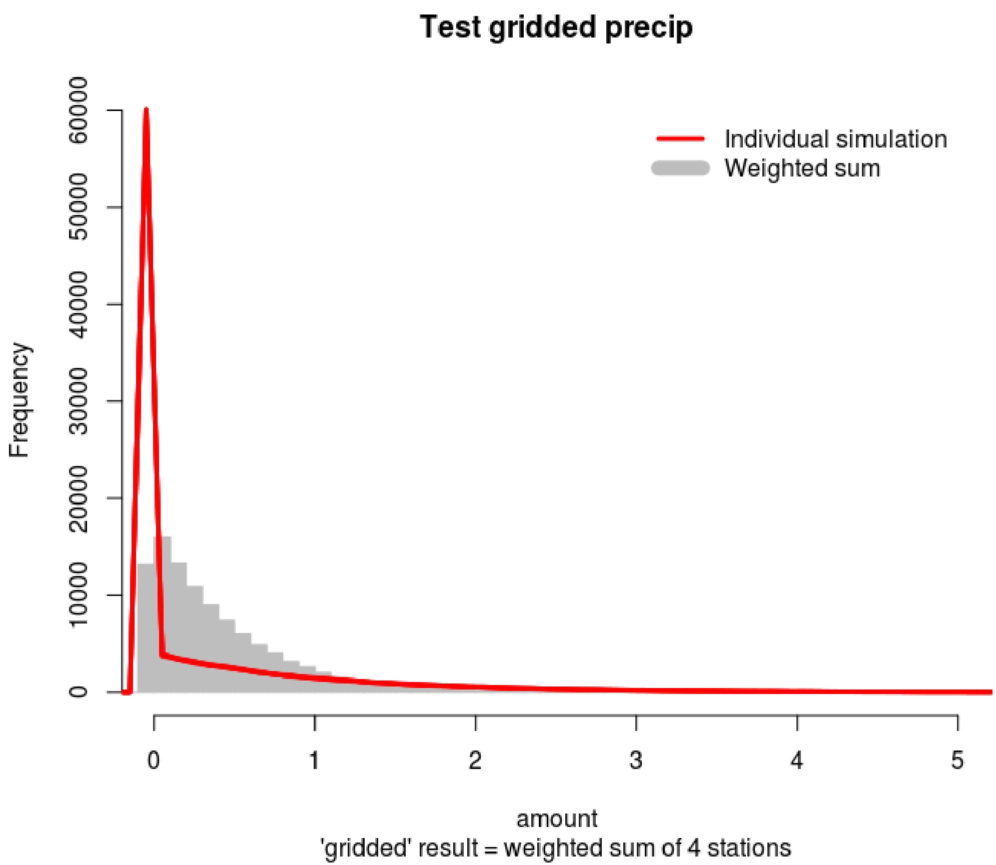

Fig. 1.

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