

## ***Interactive comment on “Uncovering the shortcomings of a weather typing based statistical downscaling method” by Els Van Uytven et al.***

**Anonymous Referee #1**

Received and published: 21 May 2019

**Summary and Overall Quality:** This research investigates the fidelity of a weather-typing based statistical downscaling strategy used to generate hydrometeorological forcing with respect to several of the underlying assumption implicit to these methods. In particular, they evaluate assumptions relating to the robustness of predictor-predictand relationships - their predictive power, stationarity, and sensitivity to greenhouse gas forcing - and how well those relationships are captured by coupled models. The focus of this research is a case study for downscaling of precipitation and temperature for a catchment within Belgium and makes use of an established weather-typing based downscaling strategy that also includes use of Clausius-Clapeyron (CC) scaling adjustments. The authors find informative relationships between the chosen weather-type predictors and forcing variable. While the coupled models capture the

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general relationships, they exhibit significant biases in particular with respect to the frequency of the underlying weather types. The predictor-predictand relationships also exhibit non-stationarity. The authors find the use of CC-scaling adjustments result in the downscaling method being able to generate more extreme values and account for changes in variance. Overall, the manuscript is well organized, though the readability could be improved through more detailed formulation of the methods rather than the extensive narrative.

Specific Comments: 1) There is very little direct formulation of the SDM within the manuscript; it is mostly left to either supplementary material or to an extensive list of references. This left the manuscript feeling less than “self-contained,” and readability could be improved with more direct formulation of the methods. This should include moving the WT-formulation from supplementary material into the primary manuscript.

2) There are a number of different datasets that are being included. However, there is very little information/discussion on why these data were selected, and it is confusing how data are being used. Why were ERA-40 and NCEP/NCAR used when these are older-generation reanalyses? The resolution of the data are disparate; how was weather typing applied to each dataset? Were they all resampled to the coarsest-resolution data (5x5) to allow for consistent WT-metrics to be defined? If not, how might the fact that the finer resolution data are likely to capture more variability affect the frequency distributions of the different WT? Were all the CMIP models resampled to the same resolution? How is the in situ, station data, being used in the compositing? Are all of the precipitation composite information being drawn using only the station data? That is, are the reanalysis only being used for developing the WT-classification and the results are just different regroupings of the underlying precipitation; or are the reanalyses precipitation actually being composited as well?

3) It is not clear if the station precipitation data can be used together with the hydrologic model. Specifically, the hydrologic model appears to have been calibrated (i.e. tuned to) a different observational dataset with likely a different climatology compared to that

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of the climatology of a single station time series. This may limit the applicability of using downscaled forcing (to that of a single station) to a dataset with a different climatology than that used to calibrate the hydrologic model.

4) Results indicated super-CC scaling of precipitation changes. This indicates potentially significant components of non-thermodynamic generated forcing, either the frequency and/or intensity of weather types. The author's decomposition seems to only account for frequency changes of WT and/or precipitation changes, but is rolling-up covariant (deviation) terms into "other" forcing. A more detailed decomposition may be warranted to better understand the demonstrated super-CC scaling along with projected changes; specifically Figure 9 "other" should be more thoroughly decomposed.

5) Figure 10 is used to establish the lack of stationarity of the underlying relationships. However, the predictor-predictand relationship appears to only be evaluated with respect to temporal changes without any control for temperature changes. Given that the used SDM implements a temperature-dependent CC-scaling, it is possible that controlling for temperature changes (and CC-scaling) in addition to temporal changes may show that the utilized predictor-predictand relationship is actually stationary as long as temperature-dependency is also included. If accounting for temperature-dependent scaling related changes results in a stationary relationship, then this would provide a more robust justification for the use of CC-scaling as part of the SDM.

6) A potentially novel component of this work is related to the CC-scaling adjustments and implementation. However, it does not appear to be emphasized within the manuscript as much of the relevant material is placed in the supplementary manuscript. The scope of this work would be more novel with a stronger focus on these aspects and less on the general analysis of GCM biases in weather-type frequency and intensity; perhaps the former (CC) could be emphasized throughout the paper and the latter included in a more condensed fashion.

Technical/Grammatical Corrections:

Line 16: “160% to 240%” : This is confusing. Is the increase 60% to 140% of current day’s values or is the increase truly 160% to 240% more than today’s values (i.e. increases of 100% is a doubling of today’s values). Please clearly state.

2) Line 31: “downscaling and,” : There are several instances in the manuscript where the comma is placed after “and” in a compound sentence. In these cases, the “,” should be placed prior to the conjunction.

2) Line 31: “by (Hewitson et al., . . .)” : There are multiple instances in the manuscript where the full references are encapsulated within parentheses but should instead only have the publication year within parentheses. For example, 2)31, 3)10, and 3)23. Please carefully proofread.

6) Line 25: Redundant use of “independent”

9) Line 27: Figure 3 is noted but it should be Figure 2. Note that all figure numbers in the narrative should be double-checked.

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