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Interactive comment

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

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HESS-2019-40 Title: Uncovering shortcomings of a weather typing method statistical downscaling method Authors: Els Van Uytven, Jan De Niel and Patrick Willems

We would like to thank the reviewer for the constructive feedback and comments. Answers to the comments have been made (see below) and the manuscript has been changed accordingly (supplement to the comment, blue font). We would like to note that the manuscript has changed significantly to address the comments of all reviewers and to improve the readability.

Response to reviewer comments (Anonymous Referee #1) 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 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.

REPLY: The WT formulation in supplementary information has moved into the main manuscript. The readability has also been improved by removing some of the redundant information in the manuscript and the more direct formulation and explanation of the methods and the results.

(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? REPLY: Precipitation time series are available for the station in Uccle since 1901. We have, however, only access to the time series between 1901 and 2000. The range of this precipitation time series has been compared with the range of different re-analysis datasets. The comparison points out that the older generation re-analysis datasets cover the largest part of the available observed precipitation time series. More specifically, the re-analysis datasets and the observations have data in common for the period 1957-2000.

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?

REPLY: The WTs for the re-analysis datasets and for the climate model runs have been determined considering resolution of the WT classification system. This information was originally provided in supplementary information and has been moved into the main manuscript.

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?

REPLY: The WT classification system is applied to the re-analysis datasets. Next, the produced WTs are coupled to the observed precipitation amounts, providing the historical pool with WTs and their associated precipitation amounts. We have rewritten the methodology to better explain the coupling between the precipitation time series for the RMI station in Uccle and the associated WTs based on the output of the reanalysis datasets.

(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 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.

REPLY: In a study for the Flemish Environment agency, De Niel and Willems (2016) investigated the spatial and temporal variations in precipitation time series for 43 rain gauges in Flanders. Their results indicated significant differences between west (coastal area) and east (Antwerp, Flemish Brabant and Limburg). As Uccle is situated in central Belgium and is located approximately 100 km from the Grote Nete catchment, the application of the hydrometeorological time series for Uccle as input series for the hydrological model of the Grote Nete catchment involves small uncertainties. The calibration of the hydrological model is, however, performed using precipitation, potential evapotranspiration and discharge time series for stations in the Grote Nete catchment.

We remark that the hydrological climate change impact analysis is removed from the manuscript for sake of brevity.

(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.

REPLY: The decomposition of the precipitation changes into contributions arising from the dynamic and thermodynamic processes has been performed for the average daily precipitation amount, projected by the climate model output. Indeed, a more detailed decomposition of the precipitation changes could be performed, as for instance done by Kröner (2016) and Kroner et al. (2017).

We would like to point out that the decomposition is performed for direct climate model output. This means that the CC relation has been indirectly considered in the climate models and is not directly applied as done by the downscaling methodology. Moreover, in the case that the results for the downscaled time series would have been used, then it is questionable whether the CC relation influences the average daily precipitation amounts. More specifically, the CC relation influences the more extreme precipitation amounts, not the average precipitation amounts.

(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. REPLY: The stationarity assumption has been evaluated using the re-analysis based WTs and the observed precipitation amounts. Hence, focus is solely put on the relation between WTs and precipitation and temperature is indeed not considered. As pointed out by the reviewer, the stationarity assumption could become more accurate when also considering temperature as a predictor. The latter could be verified by defining surrogate climate model runs and apply the SDM to the surrogate climate model runs. We however note that the empirical precipitation distributions for the W WTs for the periods 1981-1990 and 1931-1941 differ over the entire range of return periods. The application of the CC relation would thus not resolve the differences.

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Discussion paper

One of the other reviewers (Mohammad Sohrabi) wondered whether other strategies exist to test the stationarity assumption. After carefully re-reading some references, we modified the verification of the stationarity assumption. In summary, the stationarity assumption implies that the relation between the predictors and the predictand remains time-invariant. In other words, the predictors-predictand relation, which has been established using historical observations, should remain applicable under climate changes. Assuming the stationarity assumption is valid, the individual contributions by the dynamical and thermodynamic processes to the precipitation processes would not change.

In this context, the decomposition of the precipitation changes is also applied to surrogate climate model runs. The latter runs are defined by splitting the observed time series in different smaller time series. The decomposition of the surrogate climate model based precipitation amount changes is thereafter compared with the decomposition of the longterm global climate model based precipitation amount changes.

The decomposition of the surrogated based changes indicates that the contribution by the dynamical processes is important and is thus not negligible. The influence of the large scale atmospheric circulation on precipitation in winter season is comprehensively described in literature (Boé and Habets, 2014; Sousa et al., 2017; Tabari and Willems, 2018; Willems, 2013). The results furthermore indicate that the thermodymical processes gain importance at the end of the 20st century. The latter in agreement with results of Ntegeka and Willems (2008), identifying an intensification of the precipitation amounts due to the increasing temperatures.

In the modified manuscript, the approach to verify the stationarity assumption has been replaced and above discussion has been added.

(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.

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Discussion paper

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.

REPLY: In the modified version of the manuscript, more focus is put on the application of the CC relation and less on the biases. âĂČ Specific Comments: (1) 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.

REPLY: The increase is estimated between 160% and 240%. Modified.

(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.

REPLY: Modified.

(3) 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.

REPLY: Modified.

(4) Line 25: Redundant use of “independent”

REPLY: Modified.

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

REPLY: Modified.

References Boé, J. and Habets, F. (2014). Multi-decadal river flow variations in France. Hydrol. Earth Syst. Sci., 18, 691-708.

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Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-40/hess-2019-40-AC1-supplement.pdf

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