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Hydrology and Earth System Sciences Discussions

Interactive comment on "Comparison of data-driven methods for downscaling ensemble weather forecasts" *by* X. Liu et al.

Anonymous Referee #2

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General comments

The manuscript entitled "Comparison of data-driven methods for downscaling ensemble weather forecasts" by X. Liu, P. Coulibaly, N. Evora address relevant scientific questions about downscaling ensemble weather forecast within the scope of HESS. While there is a lot of research work and publications on various aspects of downscaling global climate models GCMs) outputs, there are not many research works in downscaling ensemble weather forecast. Therefore the title of the work promises to fill the research gap by investigating different data-driven downscaling techniques to address this particular concern. However, the analysis methods used and the way it is explained in the manuscript do not meet the required standard for such a paper. The discussion of the results and the corresponding conclusion are very brief and do not answer all the

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questions one might have raised after reading the introduction. So, in general, while the subject mater raised in the paper is very relevant and the methodologies used are appropriate, however, it requires a clearer explanation of the (numerical) experimental setups and a better analysis and presentation of results in addition to a more in-depth discussion of the findings and appropriate conclusions.

Specific comments

-It would be good to give a brief explanation to differentiate weather forecast models from climate models

-There should be a consistent use of GP and EPR by first defining EPR in terms of GP and then using EPR only instead of using both interchangeably

-The authors should clarify if they are relating the 15 days ahead weather forecast data (along with its lag) with the 15 days ahead observation or some thing else. The downscaling setup should better be explained in terms of functional equations which clearly show the input and output variables

-The authors did not tell as which grid points of the weather forecast model are used to extract predictors. We don't know if it is the predictors are taken from the nearest grid point or averaged over some region

-For completeness of the methodology section, it seems appropriate to give a brief description of SDSM as the authors did for TLFN and EPR

-The analysis of the results should be on how good each of the three methods performed and try to identify the reason why they perform in such a way rather than giving to much emphasis on how TLFN and EPR performed better than SDSM

-Correlation coefficient (r) is not a good measure of goodness-of-fit. Two time series where one is just a time shift of the other (such as X(t+1) = X(t)) may have a very high r value. Instead, it would be more appropriate to use the Nash-Sutcliffe model efficiency coefficient (R2) to clearly see how much of the variance in the predictand is explained

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by the downscaling model

-The authors may show some results from partial auto-correlation and sensitivity analysis for a better understanding of the predicator selection process

Technical corrections

-Better to avoid use of the first person pronouns (we) or use it consistently throughout the manuscript

-Page 191, Line 18-19: “has showing” should be “has shown”

-Page 192, Line 11-12: “total precipitation” should be “daily total precipitation”; “mean maximum” should be “daily maximum” and “mean minimum” should be “daily minimum”

There seem to be many more technical errors that have to be cheeked during the review process.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 189, 2007.