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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Evaluation of ensemble precipitation forecasts generated through postprocessing in a Canadian catchment" *by* Sanjeev K. Jha et al.

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Thank you for the interesting work. You can find my commentary in the following.

Best regards, Fabio

GENERAL COMMENTS The paper shows the application of a correction technique for daily numerical weather predictions (NWP) for short-term hydrological applications and early warning. The daily rainfall amount on a regular meso-scale (about 50-km) grid shows a consistent bias that is corrected by the proposed technique using a statistical approach based on the mapping of the joint probability distribution between the pre-

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dicted and observed rainfall amount, in a multivariate normal framework (requiring the transformation of the variables).

The topic is very relevant to the scope of the journal, since the NWP approach is very common for meso-scale weather predictions, together with its problems of bias correction. The research work is not of extreme novelty, since the technique has been already proposed, but it is applied here for the first time on cold climates, bringing useful information for the practitioners. The results are clearly exposed and analyzed, showing a relevant improvement of the predictions brought the technique. I can recommend the paper for publication with some (minor) corrections.

The following main issues should be discussed:

1) apparently, the results are shown for only 2 catchments over the 15 in the study area; 2) the change of support between the observations (point measurement interpolation) and the predictions (25/50-km gridded data); 3) the authors mention the combined effects of snowmelt and precipitation as the cause of severe floods in the study area, but they don't investigate at all the hydrological response of the catchments, limiting the analysis to rainfall data. What is the effect of the correction on the hydrological response? This additional part would require some extra effort, but would be, in my opinion, a relevant improvement of the paper, constituting a truly novel step ahead in the research work. 4) you are using 3 years of data including an event with an ultra-centennial return time event (p.5 line 4). One can argue that it is a "lucky" training data set you are using, what is the sensibility of the technique to the lack of training data amount?

The following are mainly improvement suggestions and minor corrections.

SPECIFIC COMMENTS _ p.4 line 10: it is not clear how the Schaake shuffle technique is applied (p.4 line 10). Please give more details about that step. _ section 2.2 about the "statistical treatment of forecast" should be expanded a little to make it clearer: what is the aim? what is the verification score? you already have an ensemble of realizations,

why not just validating each realization to obtain an ensemble of scores? _ how the space/time auto and cross correlation is preserved over the stochastic realization? _ section 2.3 I would add a brief indication about the climate class (e.g. refer to the koppen giger classification) and regime type of the river in the study zone. This section could be considered as an independent one and put before the Methodology (section 2). _ p.5 line 9: the "sub-catchment averaged observed precipitation" is in reality a IDW interpolation of punctual measurements, compared to mean values over 25/50 km2 areas. Is it eligible to compare these two types of data? There is a change in support, shouldn't the IDW interpolations be upscaled to the grid resolution of the predictions? For example, keeping the two different resolutions, the variance should be different, independently from the accuracy of the prediction. Also, the density of the rain-gauge network may allow or not a reasonable upscaling to the prediction grid. This is an important point on which the evaluation is very dependent. _ as stated in the captions, the results shown in the figures concern catchments 10 and 11 only, what about the other catchments?

TECHNICAL CORRECTIONS The form is accurate and concise. Minor corrections in the attached pdf.

POSSIBLE IMPROVEMENTS/OPEN QUESTIONS: _ is the spread of the corrected projections ensemble realistic with respect to the observed data? A possible quantification, for example, could be based on the frequency of the observed data lying outside a certain confidence boundary of the ensemble. For example the 0.1-0.9 confidence boundary of the projection ensemble should contain 80% of the time the observed data in order to be reliable... in short-term risk assessment application this reliability measure can be relevant.

_ as you said, topography plays a major role in the spatial rainfall distribution, how spatial non-stationarity is taken into account in the (raw QPF,observed) probabilistic relation? Is there a relation between the error in the predictions and the topography? And between correction and topography? Empirical joint pdfs between observed/predicted

rainfall amount and elevation can be a good analysis tool to underline related strong points or pitfalls of the correction technique.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2017-331/hess-2017-331-RC1-supplement.pdf

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