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Hydrology and Earth System Sciences Discussions

# *Interactive comment on* "Evaluation of numerical weather prediction model precipitation forecasts for use in short-term streamflow forecasting" *by* D. L. Shrestha et al.

**Anonymous Referee #1**

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Thanks to the authors for presenting the interesting results of rainfall forecasting from the UM-based NWPs. One fact I like best is that the manuscript describes the research utilising a series of models at different resolutions and its focus on the hydrological use. However, while the efforts are highly appreciated, I have a number of observations that I think need to be addressed in terms of the quality and the science of the paper.

General observations:

G1. The organisation of the paper. It seems to me that the paper is too long or the message has not yet effectively delivered. I understand that the the paper tries to



cover several models with a number of experiments. It is still hard to come up with a general conclusion after reading the paper. I would suggest to re-organise the paper to highlight the main points that need to be delivered.

G2. The hydrological aspect needs to be further strengthened, especially regarding the stream simulation. While the paper uses the catchment boundaries and the areal rainfall to evaluate the rainfall forecast (so that it can differentiate itself from other similar studies), it lacks the details when referring hydrological consequences, e.g., contribution to runoff generation.

G3. Some important technical details are missing, e.g., details of the ACCESS models, how the uncertainty sampling techniques are used (see specific observations below).

Specific observations:

S1. In section 2.1, I failed to get the reference to the ACCESS models but I would imagine they are linked with each other - e.g., by supplying LBC/IC from coarse model to higher-res models. It would be good to include such description so that readers would know whether these models are running independently or not.

S2. Dealing with seasonality (Line 2, page 12576). The period seems to include nearly one year. what is your consideration of the seasons and their impact. Further, during the winter period, how the snowfall is observed and how the NWP models predict the precipitation (overall or separate).

S3. Terminology. Line 14, page 12577. I would suggest to use a different name rather than RMSE to refer to your version of the standardised RMSE. Also, what is the value of the non-standardised RMSE which may make sense to see how large the error is.

S4. The use of ACCESS-G model only. Section 4.2, Line 16, page 12579. You stated that the reason is that the G model has the longest lead time. I suspect that this is due to the configuration and other models should also be able to run for the same period as long as you supply them with proper LBC data. The problem is, while you already

found that the G model is least useful (in terms for hydrological use), a long section is used to describe its skill.

S5. Line 16, page 12588, you stated "Any kind of NWP...". Could you explain why.

S6. Line 20, page 12588, "The NWP models ... at their native resolutions (i.e. hourly for individual cells)...". I think it is a misunderstanding of the "native resolution", at least for temporal one. In many models, the hourly resolution is a result of writing out the state variables every hour during the integration which means that you can actually change this to 0.5 hour, or 1.5 hours.

S7. Sampling uncertainty. A bootstrap procedure is mentioned (Line 7, page 12575) but I am not clear how it is implemented in this study and therefore cannot judge it is proper or not. Could you add a bit more details about the procedure.

S8. The last two paragraphs of page 12589. One fact missing here is that the sampling/interpolation method used in the study may have a more direct impact than the reasons mentioned in the last paragraph. Not only do the neighbouring stations in two grids cause problem, but two stations in the same grid. Also I would expect a quite different result if the IDW method is replaced with any other interpolation.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 12563, 2012.