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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 #3

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Review Evaluation of numerical weather prediction model precipitation forecasts for use in short-term streamflow forecasting.

Summary: this manuscript presents an evaluation of four NWP models over a 5500 km2 watershed in southeastern Australia, with an intended emphasis in streamflow forecasting. Forecasts are evaluated against individual station observations, without spatial interpolation, and against averaged precipitation over the catchment area. Continuous and categorical skill metrics are employed, and the influence of averaging window (3h up to 24h) as well as lead-time (3h up to 228h) is assessed. The major findings

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presented in this paper are common to other NWP model evaluation studies elsewhere: i) over (under) estimation of low (high) precipitation amounts, ii) an overall tendency to forecast non-zero precipitation with more frequency than that observed in the field, iii) decrease in forecast skill with increasing lead times. A rather interesting result is the influence of a marked daily precipitation cycle on forecast skill, due to the synchronicity (or lack thereof) between the forecast time, averaging window, lead-time and rainfall hourly stage. It is not easy to evaluate this paper, mainly because in my opinion there is a mismatch between the methods and the intended purpose of the research. While the title indicates that the NWP model evaluation is done in the context of streamflow forecasting, no amount of work is dedicated to linking precipitation forecasts with a hydrological model in a forecasting exercise. The only defensible link would be that of averaging rainfall forecasts at a catchment scale, but this is tricky, because relevant catchment scales may go from the tens to the thousands of square kilometers depending on the intended application, local climate, human presence, etc. Therefore, it is not possible to say that this work is relevant for operational streamflow forecasting based on just comparing averaged observations and forecasts over the area of a specific catchment. On the other hand, the methods presented here are appropriate and correctly applied for the evaluation of NWP forecasts per se, against observed precipitation. In this sense, as a meteorological evaluation exercise this research falls short in that the comparison area is too small, and one would hope to assess, for example, the skill of the models across different climates (regions), seasons, etc. In the latter case a subset of regions (catchments) throughout Australia could have been sampled to represent the above-mentioned conditions. I would strongly suggest complementing the study in such a way but maintaining the novel aspects indicated in the introduction of this paper. Examples of similar analyses (albeit for different precipitation products) can be found, for instance, in Skomorowski, P., F. Rubel, and B. Rudolf, 2001: Verification of GPCP-1DD global satellite precipitation products using MAP surface observations. Phys. Chem. Earth, 26, 403–409. McPhee, J., S. A. Margulis, 2005: Validation and Error Characterization of the GPCP-1DD Precipitation Product over the Contiguous

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United States. J. Hydrometeor, 6, 441–459. Xie, P. P., and P. A. Arkin, 1996: Analyses of global monthly precipitation using gauge observations, satellite estimates, and numerical model predictions. J. Climate, 9, 840–858. Joshi, M. K., Rai, A. and Pandey, A. C. (2012), Validation of TMPA and GPCP 1DD against the ground truth rain-gauge data for Indian region. Int. J. Climatol.. doi: 10.1002/joc.3612 Overall, the paper is well written but there are a few instances of cumbersome use of the English language. Please revise language and grammar thoroughly in an updated manuscript. I do not intend to point out editorial suggestions in the remainder of this review.

Specific comments Page Line Comment 12567 18 No hydrological perspective is provided in this work, please delete this statement or modify substantially the manuscript in line with my general comments provided above.

12571 1 The manuscript mentions a hydrologic model, and shows a figure with subcatchments indicating that their size is roughly similar to the 12-km NWP model resolution. Please provide more information on the hydrological application if a streamflow forecasting focus is to be adopted for this manuscript. Maybe I misunderstood, but later the authors aggregate to the catchment scale arguing that lumped models are used for forecasting. If this is so, what's the point in focusing on subcatchments? In hydrological (flood) applications, usually the relevant scales are neither the point (station) nor the catchment scale, but something in between hillslope and subcatchment scale.

12572 17 I think this statement is somewhat confusing/misleading. It is true that precipitation observations are used in construction, calibration and validation of hydrologic models, but it is also true that observations are routinely interpolated in some fashion in order to achieve the spatial coverage required by the hydrologic model. In turn, this is also a function of the spatial discretization used in the hydrologic model. A direct comparison between NWP forecasts and station data may be appropriate, but probably this has little to do with the fact that the NWP is intended for hydrologic applications.

12573 24 I'm curious about the concept of "event" used by the authors for evaluating

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the NWP skill. Is an event just an instance of non-zero precipitation at any given time window, or are the authors using the word in a more meteorological sense? In other words, an "event" could consist of a series of precipitation intervals, related meteorologically among each other.

12575 5 A more thorough description of the resampling technique used for estimating uncertainty bounds would be welcome from a reader's perspective.

12575 24 This section is confusing in that it is not clear what is being compared here. On one hand there are rainfall observations from a past period, and on the other we have modeled estimates. But the authors refer to "forecasts"... which forecasts are they talking about? The models can issue forecasts of hourly precip that here are being accumulated at 24h intervals, but the models issue forecasts for many lead times... so it there may be a few different forecasts by the same model for the same 24h interval.

12577 25 The authors state that RMSE does not show a spatial pattern by observing the plot of RMSE values against latitude then longitude. However, elevation may be a factor affecting model skill (an orographic effect does exist), and this variable may have no direct relation with lat-lon. This effect is observed later for the bias statistic. Please comment.

12580 3 It is not clear what this sentence means. Correlation coefficient seems to decrease with lead time, whereas RMSE seems to increase and bias shows a varying behavior based on the time of the day.

12580 15 Please revise the use of the word "unnecessarily".

12585 29 This sentence seems gratuitous, given that it mentions for the first time "synthetic data" without providing any details about the way it is obtained. Please consider deleting or enhancing this discussion.

12586 7 Do you mean this in general or for the intended application by BoM? Hydrologicval models with all kinds of spatial discretizations are used in forecasting application.

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12586 9 This sentence needs some support, either by evidence shown in the manuscript or by a proper literature review.

12589 17 I'm troubled by this paragraph. Is this result an absolute coincidence? Or is it to be expected? Not only this is only one catchment and only one year of data is analyzed, but no experiments regarding different hydrological model discretizations where performed, so no conclusion should be drawn at all from this result.

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