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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Attribution of hydrologic forecast uncertainty within scalable forecast windows" *by* L. Yang et al.

Anonymous Referee #2

Received and published: 9 December 2013

Title: Attribution of hydrologic forecast uncertainty within scalable forecast windows

Authors: Yang et al.

Summary: In this manuscript the authors report a study performed to examine the influence of initial condition (IC), future atmospheric forcing (FC) and hydrologic model uncertainty (MU) in the streamflow forecast skill in upper Hanjiang River basin. This topic is very relevant to the scope of the journal HESS as well as water management and planning at weekly to seasonal scale.

Recommendation: It was a pleasure reading this manuscript. I found it to be well written, organized and overall easy to understand. Graphics were clear and innovative. I certainly recommend the publication of this script however after some changes. Please

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see my comments below.

Major Comments:

(1) I think if the authors' goal was to pick two different forecast initialization dates to highlight the performance of ESP during "dry to wet" and "wet to dry" transition periods, then I am not sure if July 01 was the best choice as a "wet initial state". By looking at the Fig. 1 it is not clear to me, if July is the month when the hydrologic state is the wettest in the year. I think September may have been a better choice to depict "wet to dry transition", since the rainfall appears to be declining from September on. It is no surprise at all why the ESP method wont perform well beyond 30 days or so, when the forecast was initialized in July since the following forecast period receives appreciable amount of precipitation in first few months (which likely has high inter-annual variability as well). Speaking of which, I would also caution the authors to not make generalize conclusion (such as conclusion #1) about the performance of the ESP method for "wet initial state" case (based on their findings for July 01 forecast date) because decay in persistence of IC in that case is due to more rain in the upcoming forecast period. If the "wet initial state" is followed by a dry spell then the IC signal wont decay as rapidly. Finally, I would also suggest reviewers to include soil moisture in this plot so the readers can easily see which month is in fact the wettest/driest month of the year, in terms of the hydrologic state.

(2) I am not convinced if β parameter as suggested by the authors is the best way to explain the skill of ESP (or influence of IC vs FC). Performance of the ESP method (assuming accurate knowledge of the ICs) depends on the variability of rainfall in the forecast period, not so much on the actual amount of the precipitation. For example, if in a region over a given forecast period rainfall amount is high but it doesn't vary much at all interannually, then even the climatological forecasts (i.e. resampled historical meteorological ensembles) as used in the ESP method, will be skillful, which in turn will mean that the ESP method will perform well in that case. The authors also acknowledge this fact in the manuscript. Why not simply use "kappa" parameter as

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suggested by Mahanama et al., 2011? Why not use standard variation of rainfall accumulated over different forecast windows instead of the actual amount to explain the performance of the ESP method? Please explain.

Minor Comments:

(3) I really like Fig. 6 and 7. I thought it was a nice way to compile a lot of information on the variability of the skill of ESP method into two figures. However I think the authors should provide a bit of more details about those figures in the text. For example, at first I wasn't sure if the numbers 5 to 30 on both x and y-axis denote ranks of the events of actual value of IC and Precipitation.

(4) Line 5 page 11797: "processed-based hydrologic". Do you mean "process-based hydrologic"?

(5) Line 20 page 11798: The main point of Shi et al. (2008) was to demonstrate the influence of post-processing of ensemble forecasts not the calibration of hydrologic model. Please revise this sentence.

(6) At some places in the manuscript the authors use "post wet" season to describe the end of the wet season, however in the legend of the Figure 1 that period is referred as "post-flood". Please be consistent with that. Personally I like "post wet" phrase better than "post-flood".

(7) In figure 9 (a) Shouldn't ESP_dry line (solid dots) go between January to May only as in (b). Please check that figure.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 11795, 2013.