

## ***Interactive comment on “Value of medium range weather forecasts in the improvement of seasonal hydrologic prediction skill” by S. Shukla et al.***

### **Anonymous Referee #1**

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This paper evaluates the skill of seasonal hydrologic prediction forcing with the medium range weather forecast. The medium range weather forecasts have been used to produce streamflow forecasting for lead times upto 2 weeks. This study explores the use of medium range weather forecasts to extend the hydrological prediction upto 3-months lead time. The paper is well organized and suitable for publication after minor revisions to address the following comments.

The seasonal hydrological prediction has been done with statistical and/or dynamical methods. This study uses dynamical approach by forcing blended 14-days weather forecast with resampled historical forcings. The references to recent statistical or dynamical method are missing (e.g. BJP approach).

In seasonal forecasting of hydrological variables, future climate forcings such as pre-

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precipitation is a major source of uncertainty. This study attempts to minimize forcing uncertainty with use of medium range weather forecasts to some extent. The uncertainty of hydrological model is ignored by comparing skill of forecast with simulation in a clever way. Since the observation for the runoff is available, I am wondering whether the results and conclusions would change if the observations had been used for the reference instead of simulation.

P1831, L17: Lead time is only 3 months, why do you need to run the model with 6 months forcing data?

P1832, L1-16: The good thing about the ESP method is that it allows to produce probabilistic forecasting. Replacing first 14 days ESP with single deterministic weather forecasts would be issue if the probabilistic forecasts are required.

P1833, L23-24: Why do you need more than 50 years of model spinup to forecast a period of less than 25 years? I believe a couple of years is sufficient.

P1834, L18-21: It is methodologically incorrect that bias correction of MRWFs for a period 1983-2003 should not be done with help of observation of the same period. To have an independent evaluation, the period outside 1983-2003 (e.g. spinup period) should be used to derive the bias correction factor.

P1834, L22: The term temporal disaggregation is misleading. This is simply the bias correction to daily time steps and it is computed from 14 days period.

P1835, L1-4: Why this is an issue? As I understand that deterministic MRF was used. If the ensembles MRFs are available then it is better to use ensembles to make consistency with ESP after 14 days lead time. As I mentioned before this will also allow for probabilistic forecasting.

Table 1 shows 18 USGS regions while the paper considers 48 sub-regions which are created by merging 221 USGS sub-regions. Since the content of Table 1 is not used in the paper, this table can be safely removed.

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P1835, L18-20: It would be good to mention the advantage of Spearman rank correlation coefficient over the Pearsons correlation coefficient.

P1836, L6-20: Please elaborate what you mean by skill is not significant. Do you mean that there is no correlation between the forecast and reference at 95% significant level?

Page 1837, L8-9: For those who are not familiar with US geography, describe the Great Plains regions.

P1842, L6: What is SST?

There are some abbreviations (e.g. CDC, SON, CFS, IRI etc) which are not used in the paper. I recommend removing such abbreviations.

Figure 6: I have an impression that there is virtually no skill of hydrological prediction forcing with MRWFs in most of areas for lead time 2 month. One of the reason is that the skill of MRF itself is not good enough. Another reason is that the signal of 14 days MRWFs is decayed fast or diluted with ESP, so skill is relied on ESP and initial conditions. It would be interesting to see the skill of MRF as well.

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