**Hydrology and
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Discussions**

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

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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 up to 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.

Thank you! We appreciate reviewer's valuable comments.

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1) 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).

Response: We decided to use as a benchmark for this study a seasonal hydrologic forecast method (ESP) that is currently operational, and which places no reliance whatever on climate forecast skill. The idea was not to provide an exhaustive evaluation of various alternative climate forecast methods, but rather to perform a focused study that would assess how much additional skill could be extracted from an ESP-based method by exploiting weather forecast skill. Nonetheless, we have added some references to alternative statistical and dynamical hydrological forecast methods in lines 7-9 on page 1830.

2) In seasonal forecasting of hydrological variables, future climate forcings such as 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.

Response: Although we agree with the reviewer that model uncertainty would have played a role if we had used observations, we preferred not to do so and to structure our experiments so that they isolate the contribution of weather forecast skill. We chose to use a benchmark simulation as the basis for evaluating skill rather than observations for several reasons. One had to do with the lack of spatially distributed runoff (a variable produced by the model, in contrast to streamflow, which is an observed variable) and the absence of spatially distributed soil moisture observations. Having said this the VIC model has been widely compared with observed streamflow across the Contiguous U.S. (CONUS) and with available soil moisture data in many past studies (e.g., Maurer

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et al., J Clim 2002, which we cite). In general these studies have found that the VIC model was able to capture the climatology and seasonal variation of the streamflow, very well. Furthermore for this study we used the same model parameters as used in Maurer et al., 2002 that showed an overall good agreement between VIC derived hydrologic variables and fluxes with the observations across the CONUS.

We have now added lines 18-20 on page 1833, in the manuscript to emphasize this point.

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

Response: We actually performed this analysis for 6 months forecast period however we observed that the impact of 14 days weather forecast was non-existent beyond 3 months therefore in the manuscript we focus on 3 months forecast period only.

We have now revised that sentence to avoid the confusion.

4) 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.

Response: It is true that our experimental setup does not allow for probabilistic forecast over first 14 days of the forecast period (in case of OBS_MERGED_ESP and MRF_MERGED_ESP). We chose to use deterministic weather forecasts over the first 14 days to avoid the complications involved in merging 14 days of probabilistic forecasts with ESP forecasts that are also probabilistic in nature. (Please also see the response to the comment # 8)

5) 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.

Response: The 50 years of model spinup probably is more than is necessary, and certainly in humid or semi-humid regions a year or two will suffice. However, in arid

areas, the spinup time can be considerably longer, and we decided to be safe rather than sorry.

That sentence has been revised to avoid the confusion.

6) 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.

Response: The bias correction CDFs were derived for both the MRFs and the observation over the (1982-2003) period. The validation approach was chosen to be dependent rather than fully independent or leave-one-year-out approach in order to assess the highest potential of MRFs to increase seasonal hydrologic forecasts and reduce the uncertainties related to the bias correction spatial disaggregation approach of the MRFs. As a practical matter, a leave-one-out approach has little impact on the results, as the difference of one year in the percentile mapping used for bias correction has little effect. This has been clarified in line 21 of page 1834.

7) 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.

Response: We used "temporal disaggregation" because bias-correction of the MRF forecasts was done for 14 days accumulated precipitation and average temperature values and after bias correction we converted those 14 days values to daily values. This final step is in fact a temporal disaggregation (from 14 days to daily).

We have modified that sentence for clarity.

8) 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.

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Response: We replaced the first 14 days of each of the ESP ensembles with the same deterministic MRF forecasts (ensemble mean forecast) to make sure that the hydrologic forecast skill over first 14 days is coming from the MRF forecasts (and the IHCs) for each ensemble. Merging probabilistic MRF forecasts with ESP forecasts instead would have been complicated and increased the computational load, and we don't believe would have made a practical difference. Merging the MRFs with the ESP ensembles was done to evaluate the potential improvement in the mean values and not in the probabilistic forecasts. The ESP approach relies on the accuracy of simulations of the initial conditions, not in the climate. There is no skill in the ensemble meteorological forecasts (resampling) used for ESP, whereas in MRF_MERGED_ESP some skill in deterministic weather forecasts exists. A sentence has been added (page 1835 and line 5) to clarify this point.

9) 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.

Response: The 48 sub-regions considered in the paper were named after the 18 USGS regions in which they were located in. For example: PNW-1, PNW-2 and PNW-3 were located in USGS Pacific Northwest (PNW) regions. We think that this table will help readers with the acronyms used to name the 48 sub-regions. Table 1 is referenced on page 1835, L 17.

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

Response: We agree. We have added lines 21 on page 1835 to mention the advantage of Spearman rank correlated coefficient over Pearson correlation coefficient.

11) 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?

Response: We mean that given the degrees of freedom in our sample the correlation value was lower than it should have been in order to reject the hypothesis of the correlation being different from 0 at the significance level of 95%. We have added line 21-22 on page 1836 to clarify this.

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

Response: Good idea. We have done so in the revised paper.

13) 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.

Response: We agree. Those abbreviations have been removed. SST stands for Sea Surface Temperature. We now define all acronyms when they are first used.

14) 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.

Response: We agree with the reviewer's general assessment of the mechanism behind the hydrologic forecast evolution. Fig 6 does show the impact of MRF forecasts on runoff forecast skill relative to the potential improvement (i.e. maximum improvement due to use of "perfect weather forecasts"). For example even at lead-2 months there are some regions that show > 0.3 value of the ratio of actual improvement to potential improvement. This indicates that due to the use of MRF forecasts despite its limited skill, the improvement in runoff forecast skill was at least > 0.3 fraction of the potential improvement that can be attained by perfect weather forecasts.

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