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Hydrology and Earth System Sciences Discussions

Interactive comment on "Role of climate forecasts and initial land-surface conditions in developing operational streamflow and soil moisture forecasts in a rainfall-runoff regime: skill assessment" *by* T. Sinha and A. Sankarasubramanian

Anonymous Referee #2

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The manuscript aims at assessing the potential improvement of using seasonal climate forecasts in lieu of climatology resampling for seasonal hydrologic forecasting over a basin in the South East USA. Additional analysis is provided to assess the skill of the forecasts under ENSO conditions. The experiments include observed streamflow as the reference, VIC forced with climatological resampling, and VIC forced with downscaled GCM seasonal forecasts. The experiment is set up in a best case scenario,

where the seasonal temperature and wind forecasts are substituted by observed temperature and wind in order to isolate the skill from the seasonal precipitation forecasts.

The objective of the paper has been assessed nationally in different publications but this paper focuses on a particular region and allows more detailed analysis for certain hydro-climatological conditions. The experimental design is sound but the analysis is not explicitly defined. I do have some concerns with a couple of points. In particular:

-the ESP approach : in order to not run an ensemble, it seems that the authors used the ensemble mean for the precipitation forcing of VIC. It is not realistic and VICclim certainly has less skill than an ensemble mean flow forecasts.

-a bias correction is applied to the flow forecasts with no details on the approach. A bias correction will affect the analysis and needs to be presented along with an expectation on how this could affect the results.

- with the flow observation as reference and the current calibration (overestimation of Spring flow), it means that any negatively biased seasonal precipitation will show as improvement. It would be good to add a discussion with respect to the VIC simulation forced with the observed gridded meteorological dataset as reference instead, then compare with the observed flow in the discussion section. This would perhaps also allow supporting some of the conclusions regarding VIC simulations in low flow conditions.

-coordinate the period of calibration of the different parameters and the verification period. There are all sometimes independent and sometimes overlapping. This can drive to overfitting for some experiments and affect the inter-comparison of the different forecast approaches.

- "skill" is used throughout the paper for different metrics. Explaining what type of skill each metrics address would benefit the paper and clarify the conclusions. Which approaches is best for predictability, mean errors, etc.

- Needs clarification and reorganization in a couple of places. In particular the analysis approach is not well defined or presented. The result section could be focused on how to best answer the scientific questions. It would give more organization in the results section as well.

-Not sure why Flint is presented. There is no conclusion associated with this location

- Adding a diagram presenting the full experimental design would help clarifying the paper

Specific comments:

- The analysis is specific to a location – the title is then somewhat misleading. I would suggest adding the region in the title.

- Bias correction of the VIC flow: it is unclear how it is performed "based on calibration performance". It is all the more confusing that UW usually performs a quantile mapping –based bias correction . I would suggest the authors to clarify the bias correction approach they used.

- P5227L21: replace "soil moisture skills" by "IHCs, in particular soil moisture" or something equivalent.

- P5228L7: the term "updated precipitation forecasts" is confusing. This is a substitution of the precipitation forecasts from the ESP approach by precipitation forecast from GCMs? In the ESP approach there is an ensemble of precipitation. Is the GCMs seasonal precipitation forecast deterministic or is it an ensemble as well?

- P5229L4: seasonal forecasts issued once a month cannot really support a "real time forecasting system" but rather a planning system.

- P5229: please clarify the approach – what is the baseline seasonal forecast, ESP? And then you substitute the ensemble precipitation forecasts by a GCM deterministic forecast? Do you keep it as an ensemble?

- Figure 1: 12.7% bias is relatively significant. What type of calibration was performed on VIC? How many precipitation gauges used to derive the Maurer et al. dataset lie in the basin? What is the degree of regulation and consumptive use in the basin that could partially explain the difference. Is there any literature evaluating the latent heat simulated by VIC over this basin with another model for example? The point of the question is that the rest of assessment of skill is relative to observations. When there is a systematic bias like this, any low bias in precipitation forecast for the Spring will drive artificially to a decrease in the mean errors and flow improvement. It would be good to add a succinct analysis of precipitation forecasts so that we can better evaluate the sources of improvement.

- Section 2.3: why would you select ECHAM4.5 GCM grid cell that have the best rank correlation (for which lead time? 6-month accumulated or monthly precipitation?) and not take the overlying grid cells?

- P5332: specify that the calibration period is also the period of the analysis. In this context the VIC model structure and gridded dataset uncertainties are known and quantified for the remaining of the analysis. If the skill of the seasonal forecasts are evaluated with respect to observations, the assessment of skill and "improvement" should take into consideration the initial model errors. In this respect it is surprising that the reference is observed streamflow instead of the VIC simulation forced with the gridded observed meteorological dataset.

- P5232: specify that the spatial downscaling is performed using the observed gridded meteorological dataset as reference.

- P5232: many different periods are used so far : 1981-2010 is the period of the overall experiment, 1981-2010 is also the period of the VIC calibration, 1957-1980 for deriving precipitation monthly anomalies, 1957-2010 for the principal components, 1951-1980 for the temporal disaggregation. It would be good to use some consitency.

- Figure 2: please add in the diagram the different experiments: baseline, ESP, down-

scaled GCMS,

- P5235 L17: replace “Ensemble Streamflow Prediction” by “Extended Streamflow Prediction”. Specify here how you handled the ensemble of precipitation forecasts used for the ESP approach.

- P5235; please clarify which bias correction approach was applied? Was the objective to remove the uncertainties of the baseline simulation with respect to the observations?

- P5236 Line 2: are you evaluating the “skill of the VIC model” or “trying another way to derive streamflow forecast in order to evaluate the role of model uncertainties? This is not one of your scientific question or does not seem directly motivated to address the scientific question. Please clarify the added value of this additional experiment.

- P5236 section 3.2.1: introduce the metrics you are using for the analysis – what aspect of the forecast they represent; mean errors, variability, predictability, etc

- P5236L10: Specify the baseline for the PCR: observed streamflow?

- P5237: in the transition, perhaps introduce the analysis and which question it is supposed to address. Present the analysis before the results.

- P5238L13: The statement is not supported. If it was due to VIC poor performance it would be seen on both VICfcst and VIC clim experiments.

- I suggest having the results section more organized, either by season, by metrics. Not necessary by lead time.

- P5239L25: the VIC simulation of low flows for the baseline (Figure 1) seems better than the high flow season. Thereseem to be no real support for the “VIC model’s inability to simulate low flows”. It is possible, but just not supported here.

- P5240: the skill of ECHAM4.5 is assessed during ENSO conditions. Were different traces considered for VICclim in order to have a similar ENSO conditions? Did the spatial and temporal disaggregation training period got aligned with ENSO modes?

Did the PCR get calibrated over specific ENSO years for the same evaluation? The ECHAM4.5 might have de factor an ENSO signal. It makes then the comparison with VICclim and PCR relatively unfair.

- “skill “ is used for many metrics instead of “predictability” “mean errors” etc. Please be more specific so that we can summarize at the end what skill means.

- Section 4.4 line 1: the first sentence justifies the reminder of the paragraph by stating that VICclim and VICfcst have “good skill” at one month lead time. Please justify if this is mean errors, or reliability which will in turn allow assessing if the spatial variability is accurate in terms of predictability, mean errors, etc.

- Why is Flint presented? I t does not seem to bring any value.

- P5244L15-18: again the statement is not supported if both VICclim and VIC fcst does not show the same pattern.

- P5246L20: the ensemble mean precipitation forecast should not be used to drive the hydrology model.

- Need to assess even briefly the performance of the spatial and temporal disaggregation. It seemed that some of the figures were assessing the point but are not related to metrics used in the analysis.

Table 2 – why show Flint?

Conclusion 2: skill of soil moisture forecast cannot be supported – rather look at soil moisture patterns.

- P5238L12: if VIC had an issue simulating the low flow season in September then it should be on both VICfcst and VIC clim experiment. Could it be due to the GCM model forecast? I believe you actually mean the climate forecast model. Please specify in the text.p5239L25 confirms that you ment VIC. Although low flows are usually hard to simulate, it should be on both VIC experiments. The statement is not supported.

Figure 1 seemed to show good skill

- In the text, difficult to see to which basin it applies – what are the conclusions based on the difference between the 2 basins?

- Section 4.4: there is no evaluation with respect to observations, and the section does not bring skill assessment of the seasonal forecast. I would suggest the authors to elaborate on it and refer publication over expected effects of La Nina/El Nino . There are no real conclusions drawn from the section

- Adding a diagram presenting the full experimental design would help clarifying the paper

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