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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Assessment of methods for seasonal streamflow forecasting in the Upper Indus Basin of Pakistan" *by* Stephen P. Charles et al.

Anonymous Referee #1

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In this study performance of streamflow forecasts for Kharif Season (April-September) in the Upper Indus Basin of Pakistan is assessed. Streamflow forecasts are generated using the Bayesian joint probability (BJP) approach. Several predictors such as antecedent flow, climate indicators, and ESP based streamflow forecasts are used to test the performance of the streamflow forecasts. The study finds that in general BJP streamflow forecasts based on predictors antecedent flow and climate indicators perform the best. Variation in the skill is found for the focus basins, and for the early and late part of the season. In general, the manuscript is well organized and methods are technically sound. I do have a few comments/suggestions, some of which are moderate to major, which need to be addressed before publication.

Major comments:

(1) It would be helpful, mostly for the readers who are not well aware of the seasonal cycle of climate in the region, to add a figure for both basins that show the seasonal cycle of precipitation, temperature, runoff/streamflow. Similar to Fig. 2 of this manuscript http://journals.ametsoc.org/doi/full/10.1175/JHM-D-14-0213.1. Such a figure would provide a needed background to the readers about the region and also help interpret the results of streamflow forecasts evaluation. (2) The authors mention lack of climate forecasts skill in this region. I would encourage them to show a map(s) of the long-term skill of at least rainfall (winter) and temperature (winter and summer) in the region. I think a case for using statistical forecasts such as ones presented in this study can be made better if statistical forecast skill is demonstrated relative to the skill of dynamical forecasts, not just climatological forecasts. As of now, there are several global dynamical forecasts systems that provide operational seasonal forecasts. One of them being the North American Multimodel Ensemble (NMME, http://www.cpc.ncep.noaa.gov/products/NMME/). (3) The authors use March streamflow is the only predictors reflecting antecedent conditions, it is not clear why other variables such as snow water equivalent, soil moisture, total water storage were not used. Nowadays observations (through remote sensing) or simulations (e.g. through GLDAS https://ldas.gsfc.nasa.gov/index.php) of those variables are readily available. Especially in a region where snowmelt runoff is dominant, I would think snow and soil moisture would provide some streamflow forecast skill. (4) I would also encourage the authors to provide some more details regarding the PIT plots in the method section. To my knowledge PIT is not a typical metric used for forecast evaluation so it would help the readers to get a bit more details on them and also briefly describe what each type of the figures (a through e) highlights regarding the forecast skill.

Minor comments:

(5) P2, L24: Not only P and T but other atmospheric forcings as well. (6) P2, L29: This statement regarding the skill of dynamical forecast skill should be made more specific,

e.g. mention the regions and seasons etc. (7) P3, L23: Summer streamflow would depend upon winter T too, as winter T would influence snow accumulation. Please revise. (8) P4, L5-10: These sentences are confusing and hard to understand. (9) P5, L21: Please see comment #2. (10) Results in Table 1 and 2: It is not clear if those results are after cross-validation or before? Or are the results presented in Figure 3 onward are cross-validated? I would suggest comparing the cross-validated skill vs the skill calculated using the entire period. (11) Section 3.3: Suggest dividing this section into three sub-sections to discuss each of the verification scores separately. (12) P8, L2: It is surprising to see that MEI (May-June) from the previous year is a skillful predictor. Could you comment on why that may be? During May-June, ENSO events are in initial development stage and sometimes may change signs in the later part of the year so it is surprising that in this case, you are finding MEI May-June to be a skillful predictor for the streamflow of the following year. (13) P8, L14-15. I thought that in some cases March flow was the highest skill predictor and adding any predictor didn't increase the skill so why are you using both March flow and climate predictors here? (14) Figure 5 and 6. These figures are used to compare the skill of BJP vs SRM based streamflow forecasts. I think it would be better to combine them both into one figure. Maybe just show SRM forecasts with a different color. (15) Conclusion: The last two bullet points are not really findings. I suggest to separately discuss them after listing the findings. Also please mention here the current state-of-the-practice for generating streamflow forecasts in the region and the value the methods explored in this study will add.

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