Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2017-182-RC1, 2017

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Scenario approach for the seasonal forecast of Kharif flows from Upper Indus Basin" *by* Muhammad Fraz Ismail and Wolfgang Bogacki

Anonymous Referee #1

Received and published: 15 May 2017

This manuscript presents the application of a Snowmelt & Runoff model, which has been extended to include a Glacier melt component, for the seasonal prediction of the Kharif season inflows to the Tarbela reservoir in the upper Indus basin. As the authors clearly point out, the accurate prediction of these inflows is of key importance to the planning of water allocation to the extensive downstream irrigation areas, which are vital to Pakistan's economy and food security. The development of such a seasonal inflow product is therefore very relevant. As the authors point out this is very challenging, given the complex topography as well as the scarcity of data.

Although the model and results presented in the manuscript are therefore certainly of interest, I am of the opinion that the manuscript is not yet of a maturity that is sufficient

for publication in HESS. I would, however, recommend and encourage the authors to develop this research further and improve the manuscript for resubmission to HESS or another Journal.

I have a number of concerns on the manuscript in its current form.

The embedding of the manuscript in the current state of the science of seasonal forecasting is poor. The authors do not review different methods that have been proposed or are in operational use for developing seasonal forecasts. I would expect some discussion on statistical methods as opposed to methods that use numerical weather prediction models. At the minimum the Ensemble Streamflow Prediction (ESP) approach, which has been widely used for seasonal water resources predictions should be considered. In fact, the scenario method that has been applied, where historical years are selected, which the authors present in Section 2.6 seems to have large similarities to the ESP. If the authors review some of the literature on the application of ESP they will understand that ESP is particularly skilful where the initial conditions are persistent, as is often the case where snowmelt dominates the water resource availability. Strangely, this approach is discussed in the methods section, but I could not identify in the results and discussion section the resulting ensembles or evaluation of the results of the scenarios selected. In assessing the skill of the forecasts I think the authors could significantly extend the current discussion and results presented. Perhaps the most interesting is the question is if the forecasts presented have improved skill when compared to one or more reference forecasts. While the errors the author report are appreciably small, I would wonder how these compare to the errors compared to those of climatological forecasts, or another reference forecast. A good climatological forecast could be obtained using the average snow extent and water equivalent for the basin as an initial state, rather than the current variable snow extent based on satellite data. A more elaborate verification of forecasts would be insightful. A start could be to see how the frequency of the monthly volumes created by the (ensemble) forecasts compare to the observed frequencies, as presented in Figure 2. This could be developed further into a reliability diagram, or a rank-histogram, to name but a few commonly used statistical performance criteria commonly used to verify the skill of forecasting systems. I would recommend the authors consider developing a structured approach to these comparisons, where each model improvements proposed is rigorously evaluated against a benchmark to understand if that improvement is beneficial or detrimental to forecast skill.

The model the authors use is a very simple model, which in such a data scarce situation is a good choice in my opinion. However, despite that simplicity, there are some points that certainly deserve attention. In the way the model has been set up there appears to be a large set of parameters. It is not that clear in the manuscript how these have been derived during the calibration. Was this done as a pure calibration exercise, or was some physical basis used to estimate parameter values. This is perhaps the most apparent for the degree-day factors in Figure 6 and Table 2&3. First the unit should be added to the table, but assuming these are cm/day/C-1 then the values are in a reasonable range, if somewhat low at the start of the season. What is interesting is the linear relationship, with factors increasing during the season. I would like some more discussion on how realistic this is. Is there a physical reason that these melt-factors increase to such an extent, or is this the result of the calibration procedure? One reason for noting this is also that the hydrographs the authors present in figures 7&8 do not clearly show the contribution of the different sources of runoff that are considered in the model; direct runoff from precipitation; snowmelt; and glacier melt. As the authors note there are large uncertainties in the precipitation inputs. While RFE is chosen as an input, which given the scarcity of data is to my mind a reasonable choice, it is not so clear if there is an under-prediction of precipitation input. Comparison to the few ground stations may not help as these are, again as the authors note, likely underestimating the precipitation.

The simulated flood hydrographs are a combination of several inputs, including snowmelt and evolving monsoon. This could mean that higher degree day values re-

quired later on in the snowmelt season could be required in the calibration to compensate the underestimate of the precipitation input. This problem may be compounded due to the model presented not being mass conservative, and depends on the estimate (from satellite data) of the initial snow extent. The procedure that the authors present infers to my understanding the snow water equivalent through the selection of depletion curves. This is of course a novel approach given the lack of data, but it also contributes to model uncertainty. I think the manuscript can benefit from a more elaborate discussion/review of the methods proposed, as well as their plausibility in representing the different hydrological process that occur in the basin. This could include some thoughts on how the behaviour that is seen in some of the parameters, such as the increasing degree-day factors relate to the results of other research, or of observations. Although the model itself is not conservative, and does not include a base-flow component such as subsurface flow, the authors could evaluate the water balance across the fourteen year period they have selected. While this may include some assumption on glacier depletion over the period, it may be informative. Overall the presentation of the results and discussion is weak. Results of some of the methods described are not presented, such as the results of the ensemble scenario approach. Also, the hydrographs presented at the two gauging stations are for 2008, and seem not to represent real forecasts, but rather simulations. It is not clear. Forecasts have been developed for 2015 and 2016, but the results of these are not really presented, other than stating the estimated error. I would suggest the authors develop a much more structured approach to the results section; evaluating the model structure and its sensitivities, and then going on to explore the performance of the forecasts.

Finally, the manuscript lacks clear conclusions or take-home messages, as well as an outlook to scientific challenges that have been identified.

The Figures included are not clear and would need to be improved. Figure 3 is to a large extent redundant, and with the division of the basin easily displayed on Figure 1. The figures that include the hydrographs are also not easy to read.

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