

Interactive comment on “Potential application of hydrological ensemble prediction in forecasting flood and its components over the Yarlung Zangbo River Basin, China” by Li Liu et al.

R. Romanowicz (Referee)

romanowicz@igf.edu.pl

Received and published: 9 May 2018

The paper presents an application of ensemble meteorological forecasts from the ECMWF to forecasting flood volumes and other streamflow components in the YZR Basin.

The application of flow separation in forecasting and analysis of forecast skills of different flow components, including flood volumes, base flow, first flood in a year and annual maximum flood are the main novelty of the study. The second aim of the paper is to study “the impact of an ensemble of Pareto optimal solutions on model simulations”. I feel that the authors have failed in combining those aims together and it is the

C1

main drawback of the paper.

The authors apply the Variable Infiltration Capacity model (VIC) to ensemble flood forecasting and to separate snow-driven runoff from the total runoff. The interpolation of daily data to 6-h time step was performed using a Inverse Distance Weighted method coupled with an elevation-based lapse rate.

The available observations were divided into calibration, verification and evaluation periods. The authors refer to their previous paper published in Journal of Hydrology (Liu et al., 2017) when listing the additional datasets used in the present paper. I advise them to repeat that information in the present paper to help the reader. Also the authors refer to that paper while describing the calibration stage and as a result the description has become not very transparent. It is not clear if the snowmelt component was previously used and the number and names of parameters optimised in the present paper are missing. The authors should state clearly which parameters they optimise and how the separation into snow-induced runoff component is calibrated. The authors mention some validation but the description is not clear. In summary, the Data section should be extended and the Methodology section re-organised.

The authors compare an ensemble of multiple objective Pareto simulations and single best in their ability to forecast different flood components. The authors apply Preference Ordering Routine (POR) to choose the N-Pareto-optimal sets. There is no explanation of why that particular method was used nor what is the physical meaning of the applied numerical procedure. The authors set the number of ensemble members to ten, but it is not explained why that number was chosen in statistical terms.

In the hydrograph separation subsection 3.2, the authors do not explain which data were used for the calibration/validation of the separation parameters.

The post-processing of ECMWF forecasts is performed in an arbitrary way, without checking if it is necessary and provides better forecasts. Bias correction does not always give positive results regarding forecasting (Kiczko et al., 2015, Benninga et al.,

C2

2018).

The Results section includes hydrological model performance and an assessment of flood volume and flood component forecasts. This section I find very confusing. It does not help that the authors use abbreviations that the reader needs to be acquainted with.

The authors conclude that 7-day accumulated flood volumes are easier to forecast than the peak flows. The snow-induced flood component is not well captured whilst the rainfall-induced floods are forecast well. Taking into account the fact that the snow and glacier melt forecasts were not available that conclusion is not surprising.

The authors find that the base flow component forecast is insensitive to the forecast lead. As the base flow dynamics is slower, the forecast lead may not cover the base flow variability. However, on page 12, the authors state that for NX, the base flow forecasts show a deterioration with a lead time. A synthesis of the overall results is missing.

The language requires correcting by a native English speaker. The text is rather difficult to follow.

Overall I find the paper interesting and worth publishing after moderate corrections, despite the need to improve the presentation of the text and the language problems.

Some Editorial comments:

Page 1: The Abstract conclusions are not transparent. It is not mentioned that the forecasting performance varies within the catchments studied.

Page 2, lines 5-9 Style should be corrected.

Page 2., lines 25-27: Is this snow tracking model used in the present paper?

Page 3, lines 111-113: style should be corrected.

C3

Page 5, lines 17 and 20: instead of the word “theorem” I would use “attribute”

Pages 7- to the end: there are language problems in nearly all pages and language editing by a native English speaker is needed.

Figure 5 – there should be some quantitative assessment of the differences between simulated and observed snow cover. The comparison does not look well!

Figures 10-12 - it would help if the columns were named (snow-melt -induced and rainfall-induced components).

Benninga et al. (2018) is not referred to in the text.

References

Benninga, H-J., Booij, M.J., Romanowicz, R.J., Rientjes T.H.M, (2017) Performance of ensemble streamflow forecasts under varied hydrometeorological conditions, *Hydrol. Earth Syst. Sci.*, 21, 5273–5291, <https://doi.org/10.5194/hess-21-5273-2017>

Kiczko, A., Romanowicz, R.J., Osuch, M., Pappenberger, F., 2015, Adaptation of the Integrated Catchment System to On-line Assimilation of ECMWF Forecasts, in: R.J. Romanowicz and M. Osuch (eds.), *Stochastic Flood Forecasting System*, GeoPlanet: Earth and Planetary Sciences, Springer International Publishing, DOI 10.1007/978-3-319-18854-6_1, 173-186

Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, <https://doi.org/10.5194/hess-2018-179>, 2018.

C4