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Hydrology and Earth System Sciences
Discussions

# *Interactive comment on* "Verification of ECMWF System4 for seasonal hydrological forecasting in a northern climate" *by* Rachel Bazile et al.

**Anonymous Referee #2**

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General comment: The paper presents study of potential skill of different meteorological forcing for seasonal forecasting over 10 basins in Quebec that are operationally (short-term) forecasted and economically used for hydro-power production. For these basins in particular, a seasonal forecasting system delivering streamflow volume forecast might be of great potential economic benefits resulting from more effective operation planning. The aim of the study is to compare three methods of seasonal forecasting, namely: a) hydroclimatology (based on simulated streamflow); b) ESP (streamflow simulation based on known initial conditions of the basin and ensemble of historical precipitation and temperature observations); and 3) dynamic hydrological modelling using ECMWF seasonal forecasts of precipitation and temperature. Topic of the paper is fully appropriate for the HESS. Authors present solid introduction and literature



review. They use correct methodology that is generally well explained. Results are presented in a clear and understandable manner. The results are probably less optimistic than one might expect when a complex dynamic modelling approach is implemented, especially for a lead times longer than 1 month, however even negative (or not clearly positive) results are worth of publication (I suspect the limited resolution of aggregated observed meteorological data to be one of the factors that contributed to bit fuzzy results.). I recommend accepting the paper after some minor revisions to the paper as proposed bellow.

Authors presents results in more detail for 3 of 10 researched, as they are referred as representing different behaviour of evaluation statistics. For readers, I believe, some more explanation (e.g. on how basins are clustered in this aspect to groups represented by selected basins) would be beneficial. This should also be reflected in the discussion of results (could some physical geographical characteristics be the underlying reason? Do the verification results correlate or not with N-S performance of the hydrological model for these basins?). Authors use simple linear bias correction of ECMWF System4 Forecasts based on differences between forecast mean and observation on a monthly time scale. This method doesn't reflect the ensemble spread of the forecast or the temporal variability of precipitation and temperature within individual months. It would be valuable if authors shortly discuss this issue, in particular, if the bias corrected precipitation and temperature forecasts exhibit ensemble spread over-prediction or under-prediction behaviour (it might have a consequence for interpretation of stream flow and volume forecast results). In general, I would suggest that reasons of a failure of corr-DSP to outperform the ESP beyond 1 month lead time are further investigated and discussed.

Specific comments: p. 1 lines 13 to 16 – I am afraid that the wording of abstract doesn't reflect properly results presented in the paper itself. p. 11 line 4 "... of bias corrected forecasts. The raw ensemble..." p. 13 line 16 Authors state that "in general, corr-DSP outperforms ESP for the 1-month lead-time for watershed 5 and 7." Just bye eye

control of figure 5, I haven't that intention especially as for basin 5 the ESP performs much better for winter period. p. 15 line 9 "...(a) ESP and (b) corr-DSP..." p. 16-19 figures 8 to 10 present 1, 2 and 3 months lead times of spring freshet forecasts. This is defined as (for majority of basins) period from April 1st to June 30th. Does it mean that the 1-month lead-time is forecast issued on March 1st (etc.). Please note that in fig. 11 this is obviously the case as the 0 months lead time is also included. More description of graphical symbols in fig. 8 to 11 should be provided too. p. 18, line 2-3 consider to use "monthly flow volume" instead of "monthly volume" p. 18, line 6 Authors use term "dispersion" throughout the paper, e.g. "this possibly originate from bias propagation or dispersion issues." However, I am afraid that the meaning of "dispersion" is not clear and needs some correction (e.g. ensemble spread of meteorological inputs, variability of...).

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