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

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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

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Discussion paper

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.

Response :

Thank you very much for reviewing the manuscript and providing comments. We agree that the result of this study are not as clearly positive as expected for the performance of ECMWF's System4, especially for lead-times longer than one month. As you mentioned, different reasons can limit the skill of seasonal forecasts, such as the spatial resolution of both observations and forecasts (grid), as well as the choice of a particular bias correction method. The clustering of the watersheds chosen for results presentation in the article will be detailed following your suggestion. Actually, during our study we did not find any clear relationship or pattern between geographical location or characteristics of the watersheds and forecasts' performance. However, the link between N-S performance and the forecasts performance have not been examined. We will examine this aspect in further details and add the information in the revised version of the manuscript. Concerning the time scale in the application of the bias correction method, we agree that it could also be further discussed. Reviewer #1 also pointed out this element.

We agree that others explanations to address the lack of performance for lead-times longer than 1-month will also be investigated and further discussed.

Finally, we would like to thank you for highlighting some technical issues. Answers and clarifications regarding your other comments and suggestions are detailed below each specific comments you outlined.

Specific comments:

1. **p. 1 lines 13 to 16 – I am afraid that the wording of abstract doesn't reflect properly results presented in the paper itself.**
   Response: Though, we agree that some nuances can be added, we do not fully agree with your recommendation. The 1st sentence of the highlighted lines is *For the 1-month lead-time, a gain exists for almost all watersheds during winter, summer and fall.*. This sentence is based on the results presented in the bot-

tom row of Figure 5, where the performance of corr-DSP is compared with that of ESP. Except for watershed number 5 during winter, all skill scores are indeed positive for winter, summer and fall, which indicate a gain in performance when using corr-DSP instead of ESP. We exclude spring because the results are too contrasted over the different watersheds. The second sentence is *However, volume forecasts performance for spring is close to the performance of ESP.* This sentence is still based on the bottom row of Figure 5. It reflects the fact that for watersheds 2, 4 and 10, the CRPSS is really close to 0. Then, for watersheds 1, 3, 6, 8 and 9, the performance is still close to 0 in favor of ESP or DSP. There is skill only for watersheds 5 and 7 during this season (spring). The third sentence *For longer lead-times, results are mixed and the CRPS skill score is close to 0 in most cases.* is still based on the same figure. Even if the CRPSS is close to 0, the color scale shows that if a preference is given, it is, in most cases, in favor of ESP. This precision could easily be added in the abstract to make it more precise, according to your comment. The last sentence *Bias-corrected ensemble meteorological forecasts appear to be an interesting source of information for hydrological forecasting.* could indeed benefit from a reformulation. Corr-DSP is interesting compared to the use of streamflow climatology. Moreover, compared to ESP, the added value of Corr-DSP is mostly visible for the 1st month. We propose to rewrite the last sentences of the abstract to include the above mentioned nuances as : *For the 1-month lead-time, a gain exists for almost all watersheds during winter, summer and fall. However, volume forecasts performance for spring varies from one watershed to another. For most of them, the performance is close to the performance of ESP. For longer lead-times, the CRPS skill score is mostly in favor of ESP, even if for many watersheds, ESP and corr-DSP have comparable skill. Bias-corrected ensemble meteorological forecasts appear to be an interesting source of information for hydrological forecasting for lead-times up to 1-month. They could also complement ESP for longer lead-times.*

2. **p. 11 line 4 "... of bias corrected forecasts. The raw ensemble ..."**
Response: We apologize for this, it has already been corrected in the revised version of the manuscript that is currently in preparation.

3. **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.**
Response : This is a mistake as we clearly see that the watershed 5 is not a good example in this sentence. The sentence *"in general, corr-DSP outperforms ESP for the 1-month lead-time for watershed 5 and 7."* will be replaced by *"In general, corr-DSP outperforms ESP for the 1-month lead-time, with some exceptions such as watershed number 5 in winter or watersheds number 3 and 9 during the spring."*

4. **p. 15 line 9 "...(a) ESP and (b) corr-DSP ..."**

   Response: This has been corrected in the revised version of the manuscript, thank you for pointing this out.

5. **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.**

   Response: In figures 8 to 10, the 1-month lead-time corresponds to the forecast issued on the 1st day of the spring freshet (namely on the 1st of April for the majority of basins), for the following month. This is obviously incoherent with

Figure 11. We thank you very much for pointing out this mistake. Lead-time is the delay between the date of emission of the forecast and the end of the validity period. We propose to add this definition to the paper. Moreover, we will make sure that all results and figure labels in the revised version of the manuscript are coherent with this definition of lead-time.

6. **p. 18, line 2-3 consider to use "monthly flow volume" instead of "monthly volume"**
   Response: We will, thank you.

7. **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 ...).**
   Response: The same issue has been addressed by referee #1. Throughout the manuscript, *Dispersion* refers directly to the spread of a single ensemble forecast, namely the variability of the members. This definition of the term 'dispersion' will be added in the revised manuscript.

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