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Discussions

Interactive comment on "Benchmarking an operational hydrological model for providing seasonal forecasts in Sweden" *by* Marc Girons Lopez et al.

Anonymous Referee #2

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This manuscript presents a large scale study (39 493 catchments) that aims at gaining a better understanding of the main factors that drive the skill of ensemble streamflow forecasts in Sweden. Most similar studies in seasonal forecasting aim at distinguishing the contribution of initial conditions and that of meteorological forcings. In this manuscript, the authors rather want to distinguish the hydrological processes that drive the skill of seasonal forecasts across space and time. They also study the influence of aggregating the forecasts at different time scale (2 weeks, 1 months, 2 months, etc.) affects the skill, which I find very interesting. The authors show that forecasts are mostly skillful when initialized during the winter months, and for base flow dominated catchments. They also propose a classification of catchments into clusters with similar characteristics and behavior relative to seasonal forecasts. I think this is an interesting study, that can bring new information to better understand where we should concentrate our efforts to improve the skill of seasonal forecasts in hydrology. I only have very minor comments, that relate to methodological choices that I would like the authors to explain in greater details.

Detailed comments:

- Line 34: I am always bothered when people change the original name of a technique. The authors here define ESP as "<u>Ensemble</u> Streamflow Predictions", but this is not exactly what ESP originally stand for. In Day (1985), who originally proposed the technique, ESP refers to "<u>Extended</u> Streamflow Prediction". This may seem like a small detail, but 1) I think it is only fair to use the exact name that Day proposed for his own technique and 2) "Ensemble" prediction is very general and could very well be obtained using a dynamical meteorological model rather than past climatological scenarios. Therefore, designating ESP as "Ensemble" streamflow prediction can be confusing to some readers (I'm thinking especially about people who are unfamiliar with ensemble forecasting in general). ESP should refer to a very specific technique, but I have also heard people using it to refer to ensemble forecasts obtained using dynamical meteorological forecasts. Also, I think that Day (1985) should be cited, as it is the original reference for ESP.

- Lines 34-50 and lines 291-299: Speaking of dynamical forecasts: ESP is quite an old technique. And I agree that it is still what is used operationally for long-term hydrological forecasting by many operational agencies, and that it works well. However, long-term dynamical meteorological forecasts also exist and some studies focus on assessing their skill for hydrology, often using ESP as a reference for comparison (e.g. MeiBner et al. 2017; Baker et al. 2019, Slater et al. 2019, Bazile et al. 2017 and others). I don't have any problem with the authors using ESP instead of dynamical forecasts, but I think the use of dynamical me-

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teorological for seasonal hydrological forecasting should also be included in the literature review. There is a good discussion about NWP later in the paper (291-299), but I think it appears much too late. I strongly suggest including examples of NWP-based hydrological seasonal forecasting systems in the introduction, and possibly moving some elements from the discussion (a portion of lines 291-299) also in the introduction. I think it is important to explain why you chose to use ESP rather than NWP based forecasts, and to do so before the discussion!

- Page 4 lines 101-110: I'm not sure I understand why it is relevant to include regulated rivers in the study. They all end up in the same cluster (7), which unsurprisingly has a negative median skill. It would certainly be interesting to forecasts long-term inflows to reservoirs, as it could be useful for long term water management/hydropower production planning, but if I understand those lines correctly, this doesn't seem to be the case here (I understand that there are forecast points downstream from reservoirs, correct?). I think the rationale for including regulated catchment in the study needs to be better explained.

- Page 14 lines 240-253 and Figure 6: I would find it helpful if the abbreviations from Table 1 were used in this paragraph, which analyses Figure 6 (even though a sort of synthesis is presented in Table 2). I find it difficult to remember acronyms and abbreviations, so I had to go back and for the between the figure, the text and Table 1.

- Table 2: How are potential and actual evapotranspiration obtained? Is it really important to include both in the table?

- Page 16 line 268: Do you have any possible explanation why the cluster (5) with the highest general skill also have the largest spread? Is it possible that those two things (skill and spread) are related? What I mean is that if the skill is assessed by the CRPS and the CRPS is very sensitive to spread, then maybe the high skill

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is (at least in part) a consequence of this high spread? In any case, I think it would be interesting if the authors could provide a possible explanation.

- Page 18 lines 316-326: You mention the idea of using more sophisticated data assimilation techniques, such as the EnKF, but I think it would also be worth mentionning the possibility of assimilating other observations than streamflow, for instance soil moisture and/or snow water equivalent. This has been done in some studies (e.g. Huan et al. 2017), but there are still not that many in direct relation to seasonal forecasting.

- Page 19 lines 335-337: "This exercise shows that the regulation routines in . . ." There I finally found the justification for including regulated rivers in the study. I think this should be expressed earlier in the manuscript, around lines 105-120. At the moment, the explanations provided in lines 105-120 remain too general and it is hard to understand what it is that you want to test by including regulated rivers. At lines 335-337 it becomes clear, but it is too late.

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