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Discussions

Interactive comment on "Analysis of the effects of biases in ESP forecasts on electricity production in hydropower reservoir management" *by* Richard Arsenault and Pascal Côté

Anonymous Referee #2

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This manuscript presents a study on the effects of bias in seasonal forecasts developed using the well-known Ensemble Streamflow Prediction (ESP) approach on release decisions from a series of reservoirs for the generation of hydroelectric power stations. The energy generated is destined primarily for use in the Aluminium smelting processes. This study presents a nice example of the use of seasonal hydrological forecasts in decision making, relating probabilistic forecasts and their typically inherent biases to the decisions that are made with these forecasts. This study is of interest to the readership of HESS, and although I think that the different methods used for the optimising the releases informed by the seasonal forecasts are relevant, I think that the main interest (as also suggested by the title) are in how uncertainties and biases influence the optimal decisions made. I would, however, suggest some improvements and clarifications to the manuscript to increase the noted appeal to the readership of HESS. One of the main results that the authors seem to conclude is that a forecast without bias is not necessarily as beneficial as when there the forecast has positive bias. To most hydrologists working in seasonal hydrological forecasting, this seems to go against what is often considered as the ultimate goal of bias correction methods: developing an unbiased forecasts. Although the authors elude to it to some extent, one of the main reasons for this being that this tends to avoid spill, which is penalised in the optimisation as the volume of water that is spilled is then not used in generating power. This is particularly so when the minimum base load constraint is not included, as then the optimal solutions then tend to run the reservoirs at low heads (though this will generate less power for the same release discharge, and may incur higher penalties due to the recreation constraint). When the minimum base load constraint this changes, as this is now imposed as a constraint rather than being included as a penalty. Including this constraint reduces the “room” for the optimisation algorithm. I think that this discussion is interesting, but do think it should be generalised in the discussion. The conclusions found are not general to the use of probabilistic forecasts, but are conditional on the shape of the decision making problem (as formulated in the optimisation function). This sheds an interesting light on the value of forecasts, and how value is related to the relative penalties imposed by the different parts of the objective functions (for example recreation versus hydropower generation). I think it would be good if the discussion is extended to reflect how the conclusions found would change if the shape of the objective function changes. What would happen if the hydropower objectives changed (the current requirement would seem to favour a steady load, rather than for example hydro-peaking), how would this change if there were additional constraints or penalties on downstream releases (I would suspect a flood damage penalty would result in the same conclusions as this would also favour spillage being avoided, but an environmental constraint may favour spilling). I think the essence of my comment is that I agree with the authors that it would seem that a biased forecasts is to be preferred but this needs to be considered from the point of view of the decision process that the forecasts are used to inform. From the point of view of the hydrological forecast in its own right it make sense that the forecast is as unbiased as possible. That there is more value in the biased forecast in this case is in essence the result of a transfer of risk through an objective function that is not symmetrical. This risk transfer may work very differently in a different setting. Another good example is in water allocation from reservoirs for downstream irrigation. In this case it would be of value to tend towards a low bias in the ESP ensemble as this avoids the risk of insufficient resource being available to farmers who may have planted on the expectation of a higher availability of water. It may also be interesting to understand if the biases in the ESP forecasts have different effects through the season. I can imagine that the value of the ESP forecast, as well as the effect of biases differs depending on the time of year.

In the paper the approach the authors take is to use the simulated historical discharges as the reference, effectively removing issues with model biases and errors. Though I agree that this is a sound approach, application of the approach in a real situation would mean that these biases and errors should be considered. It would add to the paper if this is discussed. What are the additional biases that would then need to be taken into consideration?

Specific comments: Page 2; Lines 5-10: This section discusses the usefulness of ESP forecasts, commenting that these are particularly of value in the longer term. I think this discussion should be qualified to some extent. The skill of ESP when using climatology as a reference is derived from the persistence of the initial states. For hydrological systems that have a strong nival regime, such as in the case study presented here, there may be such a persistence. This also implies that the skill varies seasonally, being highest at the start of the snowmelt season, and lowest at the start of the snow accumulation season. This seems to be eluded to later (page 8, line 15) where the near zero correlation between the model states and future inflows is mentioned. This would mean that at this point the ESP forecast is solely based on climatology, which means that there is no skill.

Page 7; Line 30: The authors suggest in the discussion on considering the simulated discharge as forecast target that this by-passes issues related to the initial state of the model. I think it would be more appropriate to refer to this as bypassing issues with model error and how representative the initial state in the model is of the true hydrological conditions at the start of the forecast.

Page 8; line 17-20: The construction of the ESP forecast is discussed where the available data is divided into two periods, with the test-bed run for the second period using the ESP ensemble in the second period. Later in the paper the authors elude to possible issues as a results of the inhomogeneity between these two periods. I think it is relevant to explore this further. Is there a bias introduced due to the selection of the periods, and if so, what is the sign of this bias and how does this compare to the multiplier applied to the ESP to produce biased forecasts. Page 10; Line 21: I found the notation in the equations somewhat confusing. It is not so clear what K. T, J, pertain to. It would be useful if these could be explained. Page 11; Line 4: Unit outages are mentioned, but little detail is provided of what the implications of these outages are. To what extent do these restrict the available options for meeting the objectives? How are these outages determined? Is this according to a set schedule or according to historical data? Page 11; Line 26: “incorporates on branching” – this is a strange formulation; perhaps rephrase.

Page 24; Figure 3: The scaling of the b) figure does not readily help its interpretation. Clearly the LSJ is the larger catchment and hence much larger inflows. Perhaps use relative volumes, with an indication of what the average volumes is in each Is the flow in the left column the mean across the 3-month season? If so, then is this not the same as the volume (the only difference being the multiplication with time).

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