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Hydrology and Earth System Sciences
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 #1**

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*** General comments ***

This is a very interesting paper studying, in detail, the effect of forecast bias on electricity production in hydropower reservoir management.

While each and every of the results presented is interesting, I am not convinced by the authors' analysis of the supposedly beneficial effect of a positive streamflow forecast bias on the generation output. While some bias appears to indeed be beneficial for this particular optimization model, it may not be beneficial in general. More on that below.

*** Specific comments ***



The comments in this section center on the generation output as a function of streamflow forecast bias, and on the results leading to Figure 7 in particular.

First of all, some things are not clear to me:

- How is the relative MW ratio computed exactly? Is the result of an open-loop application of optimized reservoir releases to the simulation over the optimization horizon? Or is a closed-loop approach used to produce these figures, where re-optimization is performed at every simulation time step?

- What is the impact of the choice of values for the parameters lambda and eta (Equation 1 on p10)? It seems to me that higher lambda values would also entail more conservative operation and would hence affect the results presented in Figure 7.

The authors point out (p16) the tendency of deterministic methods to be overconfident in their ability to manage a reservoir at high head, thereby causing larger spillage than necessary:

- This will indeed be an issue if the optimization results are applied in an open loop setting. However, if re-optimization is performed every simulation time step in a closed loop setting, the planning will adjust to higher-than-anticipated reservoir levels and spilling should be much reduced.

- Use of a soft upper reservoir water level constraint, rather than a hard constraint, would probably eliminate the spilling issue altogether (in a closed-loop setting).

- With the spillage issue out of the way, the reduced reservoir levels resulting from the positive bias should, in the long run, negatively impact generation output due to a) reduced head and hence reduced efficiency, and b) due to reduced water availability beyond the optimization horizon.

As a result, I am not convinced that the reduced spillage/higher generation output phenomenon is fundamental, and therefore I would suggest to be much more cautious in claiming that a small positive streamflow forecast bias is desirable (p18). Rather, it strikes me as a phenomenon that emerges out of the interaction between forecast bias and (perhaps, if I understood correctly) the lack of a closed loop, and too stringent reservoir level bound modelling.

*** Technical corrections ***

p7: The need to derive adequate hydrological model initial conditions is pointed out. Then, it is described that these are derived using a hydrological model driven by observed climate data. To me, this begs the question on how this model is then initialized before it is ran "once more until the forecast date"?

p9: It would be helpful to include a formula describing how exactly the relative bias is computed.

p11: Equation 5. I don't see how the fundamentally nonconvex product of discharge Q and head H can be approximated using a set of linear inequalities; consider for example the relation QH restricted to Q=H, this is a convex function, which can – after approximation with a bundle of linear inequalities – be used as a lower bound for the power generation, but not an upper bound (due to the hyperplanes intersecting below the curve). The reverse holds for the relation QH restricted to Q=H_max - H, for example, which is concave. Not sure what the impact of the hyperplane approximation is on your results, but it looks like there will be issues with the head dependence of the power generation. Consider looking at some of the recent work on the homotopy approach towards tackling the QH nonlinearity without sacrificing physical accuracy.

p16: I find referring to the scenario tree approach as being a deterministic approach confusing. Yes, the algorithm is deterministic, but it takes forecast uncertainty into account to some extent and is in that sense probabilistic.

In general, it is also not immediately clear that the "unique decision method" is the same as the "scenario tree approach". Best to make this explicit earlier on.

Figure 6: The units of panels (b) and (c) on the X axis don't make sense to me, esp.



the negative efficiencies.

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