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Discussions

Interactive comment on "Impact of the quality of hydrological forecasts on the managementand revenue of hydroelectric reservoirs – a conceptual approach" *by* Manon Cassagnole et al.

Anonymous Referee #1

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This is a review of "Impact of the quality of hydrological forecasts on the management and revenue of hydroelectric reservoirs – a conceptual approach" (please not there is a space missing between "management" and "and" in the title in the Copernicus manuscript submission system) by Cassagnole et al.

The manuscript presents an analysis of ensemble streamflow forecast quality and how it relates to hydropower value in a rolling horizon test bench, using controlled mechanisms to induce biases and under-dispersion over an unbiased reference. The ensemble forecasts that are generated are modified to be overestimated, underestimated and under-dispersed. The impacts of the ensemble spread are also investigated. The

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analysis is performed on 10 hydropower systems in France, using historical energy process to estimate gains/losses in revenue of having forecast biases compared to a perfect forecast. The authors find that the unbiased forecast performs the best in terms of quality and value, and that the overestimated forecasts (positive bias) perform the worst, especially at high dispersion levels.

I thoroughly enjoyed reading this paper as it is presented in a very clear manner and well structured, and is a good foray into the little-explored world of forecast value in hydropower systems. I found the analysis to be robust and sound, with the literature review being both necessary and complete. I am particularly fond of the method to generate streamflow forecasts using a statistical distribution that is reshuffled into a reasonable temporal pattern using an ECC-based method.

After reading the manuscript a few times, I have highlighted a few points that I think are worth investigating as they could change the interpretation of some of the results for some users. Therefore I think that these few points I suggest below would make the paper even more solid and allow users from a broader audience to find interest in the study results. I recommend minor revisions as the changes are mostly all to the text and I don't think more simulations or analyses are required.

Specific comments: 1- Line 205-206: Here it is written that three additional values of D were used, which corresponds to the spread factors of 0.01%, 1%, 2.25% and 4% (4 values). It should be integrated with the previous sentence or explained better that the first value of 0.01% is from the D value of 0.01.

2- Figure 2: The forecasts for the UnE and the UnD are eerily similar, which is explained later in the discussion for other aspects but the discussion should refer to figure 2 to show how the forecasts are similar by reducing the high peaks (Figure 2 is not discussed after line 219). Making this link near lines 366-370, lines 379-382, lines 443-445, etc.

3- Lines 220-226: Could the LP solver be cited? Is it a commercial software such as

XPRESS, CPLEX, GUROBI, etc.? or is it in-house? Perhaps open source?

4- Line 239-240: Here, do the authors mean that the ensemble mean of the streamflow is fed to the solver (I think this is the case)? Or is the solver run on all members and the average decision taken? The latter could be interesting also to consider non-linearities in the ensemble forecasts. If the former is true, which I think it is, then I wonder why bother to generate ensemble streamflow forecasts at all? Why not simply generate deterministic forecasts with the desired properties (slightly biased, over/under -stimated, etc.?) under-dispersion, if it is symmetric, would be the same as the unbiased system, as seen in many figures. I understand it is not necessarily symmetric but the method employed (skewness of the log-normal distribution) could be corrected by another distribution... Basically, I think the authors should justify the use of an ensemble-based methodology if the solver only uses the mean value and the rolling-horizons test-bed only uses the ensemble average as well. Any difference between the unbiased and under-dispersed values would be due to the generation of artificial streamflow. If the authors used real-world streamflows (or simulations of streamflow using observed/forecast weather) then this point would not bother me as much...

5- Line 244-245: This means that there is no terminal water value, correct? i.e. the system would want to empty the reservoir at the end of the period to maximize benefits IF there was no constraint on drawdown volumes equal to the expected inflows? Perhaps the authors could comment on the impacts of this, as there would be no consideration of marginal head gains.

6- The fact that the optimization method is deterministic should introduce a deterministic bias, by which the optimization method does not account for uncertainty and thus is over-confident that it can maintain high-head without spilling (Philbrick and Kitandis 1999). Therefore slightly increasing the bias "tricks" the model into thinking there will be more water, forcing it to produce more energy and thus counteracting it's over-confidence on high water levels. In a real-world scenario, this would be observed as the reservoir head would increase efficiency and entice the optimization algorithm to maximize revenues this way. But the setup in this study uses constant efficiency (line 252) with no change caused by reservoir head, making it much less representative of an actual system. The optimization has no need to optimize water levels, just make sure the reservoir doesn't crash or overtop. I think the authors need to add a section to the discussion to highlight these differences, because the way the abstract, discussion and conclusion are written, it could seem like the authors are saying that overestimation of forecasts is the worst possible solution, whereas in the real world it is probably the best option if using a deterministic optimization algorithm to counteract the optimization deterministic bias. I think the presented results would be much more in line with what would be seen if the optimization algorithm where stochastic (SDP or in the same vein), as the uncertainty is inherently included, which is not the case for deterministic solvers. [Philbrick, C. R. and Kitandis, P. K.: Limitations of Deterministic Optimization Applied to Reservoir Operations, J. Water Res. Pl.-ASCE, 125, 135–142, https://doi.org/10.1061/(ASCE)0733-9496(1999)125:3(135), 1999.]âĂĆ

7- For the soft constraints, is there a penalty term in the cost function? Or is it considered as a hard constraint during the solving and then dealt with during the simulation part? If there is a penalty, could the information be provided?

8- Could the authors perhaps give an example (figure?) of the impact of increase in dispersion/spread using the D2 factor, compared to the effect of the under-dispersion? How does this affect the ensemble mean? Perhaps an example with a random date would help understand these impacts and differences (as the increase in spread is counteracted by the process of under-dispersion).

9- Figure 13: I think this figure could be changed to a 2x2 panel figure as in the previous figures, to keep things simpler and cleaner.

Typo/precisions needed: 1- Line 17: "...approximately 3% to 1% (in M€..." Here, I suppose the authors mean that the value of 1-3% represents Millions of Euros, however the way I read it is as if the units were M€ which would be moot as the difference is relative. I suggest writing it as “which represents millions of Euros”

2- Line 28: squared kilometers → square kilometers

3- Line 281: non-respect → violation

4- Lines 487-492: These few sentences were quite confusing to read. I think they are technically correct, but reading them and parsing the information was somewhat difficult. Perhaps separating into a few more sentences and clarifying? Especially for the last sentence (lines 490-492).

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