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Hydrology and Earth System Sciences Discussions

Interactive comment on "Tailoring seasonal climate forecasts for hydropower operations in Ethiopia's upper Blue Nile basin" *by* P. Block

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The author would like to offer sincere thanks to Referee #1 for taking time to carefully review this manuscript and provide insightful comments. They are much appreciated. Below is a point-by-point response to issues raised. Referee comments are numbered.

1. I do have a serious concern as to whether the coupled modeling system properly captures the forecast uncertainty. . .

The coupled modeling system does explicitly capture forecast uncertainty through a stochastic approach. This is exactly why the 100 ten-year climate sequences are generated, drawing from June-September forecast distributions representing that uncer-

tainty. The hydrology and hydropower models themselves are deterministic, so repeatedly run (100 times) with varying climatic conditions (and forecasts) to effectually create a stochastic approach. Except for the 1961-2000 chronological illustration, forecast means are not used, but rather the full distribution. Unfortunately this has not come across clearly in the text, which will be modified to be more understandable and obvious in the revised version. The approach is briefly explained on page 3777, line 15 ". . .for each of the ten years within the 100 sequences, the actual precipitation forecast was randomly selected from that year's forecast distribution (as opposed to using forecasted means) to represent model uncertainty." In other words, to assemble one of the 100 ten-year sequences, the ten years were randomly drawn from the historical record to represent hydroclimatic variability (sequence is important) and for each of those years, a precipitation forecast was drawn from the forecast distribution created from the model ensemble members. Here model uncertainty implies forecast model uncertainty. Repeating this 100 times develops the envelope of hydropower benefit possibilities considering both hydroclimatic and forecast uncertainty.

2. The three cases assume that future streamflow sequences (12 months) are known, therefore overestimating the benefits of system operation.

This (perfect foresight) is only true for the "perfect" case, which represents the absolute upper limit of benefits possible (e.g. perfect operational decision-making), not an overestimation. For the monitoring and actual approach, the 12 month values are not known perfectly. In the monitoring case, future month streamflow values are always climatology (historical average). Therefore all Januarys (and Februarys, Marchs, etc.) streamflow values are equivalent. So the model optimizes over what it expects (climatology) 12 months out, not what will ultimately occur (observations). In the actual forecast case, forecasted precipitation for June-September is used to forecast June-September streamflow. For the other 8 months, forecasted precipitation is simply climatology (no additional climate information), however streamflow is not necessarily simply climatology in those months due to persistence from earlier months (i.e. June-September precipitation forecast can have lingering effects on future months streamflow.) So in this case, the model optimizes over 12 months of forecasts. Only in the forecast issuing month (May) do the forecasts get updated. Again, the text will be tightened here to spell out these details (section 3.3).

3. . . .the uncertainty could at least be incorporated implicitly. In the "actual" case, this could be done by directly optimizing over the 500 members of the ensemble, and then calculating the expected benefits.

Expected benefits are derived directly, however instead of using the 500 ensemble members directly, forecasts are drawn from the distribution formed by the ensemble members. This, along with random sequencing of years, creates an envelope of expected benefits. It is entirely possible for one of the 100 sequences to consist of the same year ten times in a row (e.g. 1975), with each of those ten years having a unique precipitation forecast. Optimizing over the 500 ensemble members would address forecast uncertainty, as suggested, however the approach here combines forecast and hydroclimatic (sequencing) uncertainty.

4. Some relevant references on the incorporation or flow forecasts in reservoir operation are. . .

Thank you for the references. I will review them and incorporate them into the revised version.

5. The manuscript would be easier to read if some terms were defined: forecast vs. prediction, climatological vs. historical values, observed vs. historical values, etc.

Yes, this needs some attention and consistency throughout. Forecast and prediction are often mixed, even in the climate community, although technically they imply different products. Other terms are likely more common among HESS readers, but will be define for clarity and completeness.

6. Page 3767. Suggest one paragraph with the above references on the value of

hydrologic information in reservoir operation.

Good suggestion. The revised version will contain this.

7. Page 3771. Line 15. What happen to the 500 members that are generated by the forecasting system? Are the medians/means considered as the forecasts in the "actual" case? A diagram describing the flow of information between the different components of the modeling system would probably be useful.

The author agrees that a diagram or schematic would be useful and will include one. The 500 ensemble members (per year 1961-2000) form the forecast distribution for the "actual" approach (as stated in an earlier response.) Forecast quantities are drawn from these distributions 1000 times (100 sequences ten years long) to represent forecast uncertainty.

8. Page 3772. Line 16. The value of electricity is set to 8 cent/kWh. Does the value remain constant throughout the year?

The value remains constant throughout the year and throughout the decadal sequence. This is another feature that could be explored, but for the current analysis, it has been held constant to emphasize the value of precipitation forecasts. Keeping it constant throughout and consistent between approaches (perfect, actual, monitoring) eliminates another potentially confounding variable, easing interpretation.

9. Section 3-3. The description of the three types of forecasts is a little bit confusing. Some terms must be defined and a figure illustrating these three types would make the reading easier.

Revisions will be made to improve the language and clarity of this section.

10. Page 3774. Line 9. Why was a 12 month foresight chosen? This seems rather long to me, and the informativeness of the forecasts after a couple of months must be negligible.

The 12 month foresight does contain some information beyond a couple months for the actual approach, but relatively minimal, as the Referee suggests, except in May, when the forecast is issued for the following 4 months. More importantly, the longer foresight helps to eliminate effects of constraining reservoir levels to prevent dumping for short-term gains in the optimization. Allowing the reservoir levels to be dynamic (not tied to a rule curve) exploits forecast benefits. Other approaches are likely valid. The overall emphasis is on consistency between approaches for a fair comparison.

11. Page 3775. Line 7. The forecasts modes are evaluated by comparing the sum of monthly hydropower benefits (generation) over the several periods of 120 months. Comparing the firm energy is also important, especially for risk-averse managers.

The author agrees. Firm energy, while not explicitly reported here, may be inferred from the section of reliability and risk. More research on this front is ongoing.

12. Page 3777. Line 15. The mechanism used to generate the 100 decades does not preserve the temporal persistence between December and January. How does the author address this important issue?

Realistically, this has minimal to no effect. The perfect approach is the only case where some discrepancy may occur, however likely negligible. Firstly, this is the dry season, and variability (let alone quantity) is small; secondly, for the actual and monitoring approaches, climatological values for precipitation are used in both months. The precipitation forecast in the actual approach is for June-September only. Therefore there is no effect in those two cases.

13. Page 3778. Line 3. Negative benefits can be observed because low storage levels and outflows are penalized. Could penalties be removed and replaced by constraints so as to avoid negative benefits?

Penalties are low in comparison to benefits of selling electricity. Penalties act as a loose constraint giving the model more slack to find optimal solutions. Hard constraints were

originally coded in the model, but seemed to give the model troubles or even produce infeasibilities, which were inappropriate. Therefore the decision was made to utilize the penalty approach. This only comes into play in a few of the 100 sequences.

14. Page 3779. Line 14. It is still unclear to me how the "observed" and "observed with error" series were generated. Why did the author choose to add 25 mm of monthly precipitation? What does 25 mm correspond to?

This text will be clarified in the revised version. The goal here is to create synthetic series for sensitivity analysis. One series acts as observations (constant over the ten-year sequence for simplicity) and another as a forecast with error, in which some systematic bias is added. 25 mm/month is arbitrary – just a reasonable quantity aggregating to 100mm over the June-September season. It could be different each month or for each year, but the idea is to gauge effects of forecast errors in wet and dry years and compare fairly.

15. Page 3776. Line 20. Fig 4. Figure 4 presents the cumulative benefits for the four decades (1960-2000). Why working with four independent decades and not a single period of 40 years? Why 4 periods of 10 years and not, say, 8 period of 5 years? I have a similar concern with the forecast horizon (12 months). Those modeling choices should be better explained and justified. They should be based on the hydro-meteorological characteristics of the system and on the storage capacity of the reservoirs. No need for long lead times if the storage capacity is small...

Regarding the 12 month forecast lead (discussed in an earlier response), the reservoir storage capacity (for the single dam case) is actually quite significant, on the order of 1.6 times the annual average inflow. Thus a longer lead was selected. This will be added to the revised version. Regarding sequence length, decades represent a reasonable time frame for assessment. It is common knowledge that a forecast may be wrong in any given year (or season); the emphasis here is that over time, the forecast driven system for this application prevails over commonly utilized monitoring approaches. Similarly, managers do not need to wait 40 years to realize these benefits. Ten years is also a reasonable planning period (versus 1 or 40), so it also matches from that perspective. Forty years just happens to be the observational record length. It may also be easily inferred from Figure 4 that a single period of 40 years (roughly summing the four decade benefit totals) favors the actual forecast approach. Similar text will be added to the revised manuscript.

16. Figure 5. The difference between forecast and monitoring benefits is positive for low benefits, i.e. dry years. The opposite is observed during wet years. The discussion in the paper focuses on points A and B but not on the overall pattern. Above normal forecasts are then corrected to improve the performance of the system during wet years. One alternative to avoid this poor performance during wet years is to impose a target storage at the end of each year. Why was this option not implemented?

Just for clarification, Figure 5 reports aggregated decadal benefits (not annual), so drawing conclusions regarding dry years and wet years is not trivial. Even from Figures 6 and 7, this is not straightforward, as a forecast error may not play out in terms of benefit losses until the following year. The same is true for the climatological approach. This is partially due to the large reservoir storage capacity. Imposing a target storage at the end of each year is a viable option, but limits the dynamic reservoir management component possible when considering dynamic climate. (Earlier research not reported confirmed this.) That being said, a target storage may be the preferred operational choice, and is being evaluated in ongoing analysis. Wet year forecasts are not corrected to improve performance, per say, rather they are relegated to climatological values, not dissimilar to the monitoring approach. Dry year information is found to be most critical, giving rational for tempering negative impacts from errant wet year forecasts.

17. Page 3779. Line 25. We need more information on how the forecasts are “modulated”.

This will be expanded in the text. Simply, if the forecast quantity is in the near normal or above normal terciles (based on historical observations) then the forecasted precipitation is replaced with the climatological (average) value.

18. Page 3782. Line 3. I would also add that a forecast system may be considered useful if the inherent uncertainty is properly accounted for, which is not yet the case here.

Hopefully, based on the above responses, this point is now moot.

19. References. Reference Ziervogel is missing.

Noted and added. Thank you.

Again, the author would like to thank the Referee for their substantive comments, which will undoubtedly improve the quality of the paper. Their careful attention is evident.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 3765, 2010.