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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 #2 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. The paper lacks discussion on calibration, validation and uncertainty of the rainfall-runoff (r-r) model. As the model uses very course grid size (0.5 x 0.5 degree) and quite simplified process representation, I wonder how reliable is it in using with an operational system for making decisions involving very high stakes (billions of dollars)? It is therefore important that the paper includes discussion on the level of accuracy and reliability of the model.

The Reviewer raises an excellent point that effectively leads toward a discussion of data availability. Unfortunately, a very minimal amount of hydrology data is publicly available throughout the country, including within this basin. Additional observations and measurements would be absolutely necessary prior to final design and performance analysis of the dam. The rainfall-runoff model calibration and validation, therefore, was dependent on a longer streamflow record available at the Sennar dam, just over the border in Sudan. No calibration (or validation) is possible precisely at the entry to reservoirs, rather only as an intermediary point within the model, assuming the internal processes are satisfactory. Text highlighting this will be added to the model description section. This being said, from purely a comparison perspective between forecast and no-forecast approaches, the influence of the hydrology model is damped; the focus is on differences or the relationship between approaches, as opposed to absolute values (considering benefits, for example.) This is the intent of Figures 8 and 11, for example. Even though absolute values are listed, it is really the relationship (superiority of the forecast approach) that is emphasized.

2. The paper does include some discussion on the uncertainty of the forecast model (climatic variables) using 500 ensemble members (pg. 3771), but apparently this uncertainty estimate was not used in other components of the system, i.e. r-r model and the hydropower model. The final result on benefits should either include that uncertainty based on these ensembles or should be provided with a number of scenarios based on selected ensemble members.

A similar comment was provided by Reviewer #1. Clearly the text needs to (and will) be revised highlighting the propagation of the forecast uncertainty through the hydrology and hydropower models. 100 ten-year climate sequences are generated, drawing from June-September forecast distributions representing that uncertainty, then fed into the subsequent models. Final hydropower benefits and reliability therefore capture this climate forecast uncertainty. Please also see the response to Reviewer #1, Comment 1.

3. In the perfect forecast case (pg. 3774, lines 10-14) it is not quite clear whether the observed stream flow or the flow estimated by the r-r model with the observed temperature and precipitation input was used.

As no streamflow observations at the inlet to the reservoir are available, estimated flows were generated using observed precipiation and temperature. This is why observations in the text is surrounded by quotes. A sentence will be added to clarify this point.

4. In section 3.3, the three cases (perfect, actual and monitoring) should be illustrated with a schematic diagram showing what inputs are used in the three cases.

This is a good suggestion, as the author also advocates for clarity within the manuscript. A schematic or figure demonstrating the differences between cases will be added for visual interpretation.

5. In Eq. (2) and (4), symbol/style used does not seem to be coherent with other equations.

Yes, there do appear to be some inconsistencies, which will need to be worked out in coordination with the production editors. Thanks for pointing this out.

6. In Eq. (3), is there something missing?

All the information is there, but not presented well. This will be fixed. Here is the intended equation (or perhaps two equations):

If FBt < MBt and if FBt+1 >= MBt+1 then yt = 1 If FBt < MBt and if FBt+1 < MBt+1 then yt = 0

7. Fig. 5: both x- and y-axes should be presented in the same scale.

This is a reasonable suggestion, and could help with better interpretation of the figure. The revised version will have equal axes. The original intent was to keep the figure size to a minimum vertically (equal axes will increase figure size.)

8. The appendix should be removed (I suppose the algorithm is also presented in Block and Strzepek (2010), which is also cited in this manuscript).

The Appendix will be removed, and a reference to Block and Strzepek (2010) inserted in its place.

Again, the author would like to thank the Referee for their comments, which will undoubtedly improve the quality of the paper.

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