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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "On inclusion of water resource management in Earth System models – Part 2: Representation of water supply and allocation and opportunities for improved modeling" *by* A. Nazemi and H. S. Wheater

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We greatly appreciate Anonymous Reviewer #2 for their positive, constructive and thoughtful comments, which led to substantial improvements in the revised version of our manuscript. In the following, the issues raised are addressed point-by-point in the order they are asked. The reviewer's comments are numbered and our reply is immediately below each comment. Please note that our reply accompanied with a draft revisions, which is attached to this discussion as a supplementary material.

1- I would agree with reviewer #1 that there are a couple of erroneous statements which could be verified by the different modelers.

Many thanks for your suggestion. As indicated in our reply to reviewer #1, we took all reasonable steps to ensure accurate representation of the schemes reviewed. We contacted Drs. Hanasaki, Oki, Haddeland and Voisin in particular. Some have already came back to us and we hope to have other responses before we submit the final revised manuscript to HESS.

2- the last section suggesting a modeling and testing framework (5.6) seems limited in comparison to the first sections (2,3,4) describing the existing processes. The framework is not put in perspective with respect to the modeling suggestions made in the section 5 subsections. A case study of the suggested framework with one of the example suggested in earlier 5.s section would validate that framework. The point is that if a framework is being suggested in a paper, readers will expect a case study in order to get convinced that this is sound and feasible, even though the paper is already pretty long.

Many thanks for your comment. We try to majorly extend this section using your comments. Please see the supplementary draft revisions attached (lines 820 to 928) In particular, we added a table to summarize the suggested modelling improvements and the spatial and temporal scales at which this is meaningful, and the data required to make it possible in terms of parameterization and validation. We have also added a new figure on how to approach the suggested framework in a sequential manner. We included a very brief discussion on the activities we are currently doing in terms of benchmarking reservoir operation algorithms in the SRB. However, we did not provide any detail or simulation results as our investigation is not yet fully finalized and we plan to publish our result in another manuscript.

3- There is a lot of information, which comes in text, and might seem unorganized and sometimes even in opposition to previous call for improvement (especially com-

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putational burden and mismatch in space and time scales between LSS, GHS, and management models for example). I would suggest a summary table which specifies for all the suggested improved modeling, the spatial and temporal scales at which this is meaningful, and the data required to make it possible in terms of parameterization and validation at least. I think that this process would make the manuscript easier to properly cite and useful for directions in research.

Many thanks for your comment. We added a new table (Table 4) in the beginning of Section 5.6 to summarize the suggested modelling improvements and the spatial and temporal scales at which this is meaningful, and the data required to make it possible in terms of parameterization and validation. Please see the supplementary draft revisions attached (lines 821 to 837 and Table 4, page 58).

4- Section 3.3.1: Voisin et al. (2013) actually combines release targets with storage targets, $\sim$ rule curves.

Many thanks for your comment. We consulted with the original article and revised the related discussion accordingly. Please see the supplementary draft revisions attached (lines 328 to 334). We have contacted Dr. Voisin to double check our revision.

5- Section3.3.2: Although there are advantages to using optimization-based algorithms, the computational burden and need of forecast demand and inflow makes it inappropriate for full online coupling. It is unclear in the paper how the authors see further research on how to integrate them in their vision of future research.

You are absolutely right and we have also noted this in the manuscript and suggested simulation-based algorithms to move forward, especially towards online simulations. However, we feel that we still need to review the existing algorithms for completeness of our review and discuss the pros and cons of both simulation-based and optimization-based algorithms in detail. Moreover, optimization algorithms would be valuable for offline simulation, particularly for integrated impact assessments. We tried to highlight this throughout the revised text. Please see the supplementary draft revisions attached

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(e.g., lines 756 to 759).

6- Section 4: GHMs are used for hydrological application because their hydrology processes are more complex and allow for some calibration. Reservoirs have fixed characteristics and the main driver of uncertainty for reservoir modeling is the bias in the inflow (Muller Schmied et al. 2014). This would need to be put in perspective in terms of direction of research, in the sense that there is a workflow in the modeling improvement; Some things need to be improved first before we can improve other concepts. The idea of workflow could be introduced in the summary table suggested above.

Many thanks for your comment and the reference you introduced. We have incorporated the reference in Section 4, where we discuss the uncertainty related to the inflow to the reservoir. Please see the supplementary draft revisions attached (lines 526 to 529 – please also see lines 530 to 535). We have also suggested a sequential framework to approach model development and testing framework suggested in Figure 2. More specifically, Figure 3 divides the model development into four sequential steps related to (1) benchmarking individual algorithms, data support and host models; (2) building various settings for offline simulations; (3) further improvements and configuring data, algorithms and host models for online simulations; and (4) building various setting for online simulations. Please see the supplementary draft revisions attached (lines 889 to 900 and Figure 3, page 61).

7- Section 5.4: Even in local see regional operational water resources management, different decision support systems are used for handling events at different time scales: i.e. hydropower with a 5 minute market, floods with subhourly to hourly time step, and monthly seasonal water supply. The suggestion to move large scale water management to a sub hourly time scale seems i) irrelevant and ii) in contrast with the need of data for calibration when operation are driven by the market for example, and in constrast with the need to balance computational needs.

Many thanks for your careful reading of our paper. You are absolutely right and the dis-

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cussion was irrelevant. We have revised the section and also included your discussion on the decision support system in the following call for investing on system identification frameworks. Please see the supplementary draft revisions attached (lines 756 to 769 and 784 to 786).

8- The demand-supply dependency term of “upstream” is confusing. The dependence links the grid to places where water can be withdrawn, i.e. the grid and a couple of reservoir upstream. But those reservoirs are note defined at “ 5 grid upstream”. Rather, the dependent grid cells are downstream from a reservoir and within 5/10 grid/ 200 km from the impounded river (downstream). Please clarify.

Many thanks for your comment. This issue was noted by reviewer #1 as well. We revised the column to avoid further confusion. Please see the supplementary draft revisions attached (Table 1, page 55).

9- Entries for Voisin et al. are inaccurate: “Dynamic priority in operation” should be changed to irrigation, flood control, hydropowers and others.

Many thanks for careful reading of our paper. We corrected the Table 1 accordingly. Please see the supplementary draft revisions attached (Table 1, page 55).

10- The source of data for Voisin et al. (2013a,b) include USGS, USBR and GRDC as in Haddeland et al. There should be another row for Voisin et al. (2013b) which actually used the Community Land Model (CLM) instead of VIC.

Many thanks for your comment. We included the data sources in the table and added a new row for Voisin et al (2013b). Please see the supplementary draft revisions attached (Table 2, page 56).

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/11/C4913/2014/hessd-11-C4913-2014-supplement.pdf

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