Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-589-RC3, 2020

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Coordination and Control: Limits in Standard Representations of Multi-Reservoir Operations in Hydrological Modeling" *by* Charles Rougé et al.

Anonymous Referee #3

Received and published: 16 January 2020

The manuscript presents a new large scale reservoir operations model with generic operating rules associated with the reservoir main operational purpose such as flood control or irrigation, or default. The reservoir model stands out from equivalent models in that the releases are decided daily based on the daily storage level, shapes with combined log and exponential curves that accelerate the release in times of floods when close to full capacity and slows down the release in times of droughts with differences in the thresholds and propensity to release and store based on the purpose of the reservoir. The overall release is scaled by the long term mean annual flow. The model is implemented at high resolution (.6 km, daily time step) over the Upper Snake River Basin, which is a snowmelt driven basin. The method of Morris is used to identify

the reservoir release parameters that tend to be most influential in the reservoir release and storage variations throughout an 8 year period. Upstream reservoirs are used to evaluate the approach while downstream reservoirs are used to evaluate the impact of upstream reservoirs. A flood and drought events are evaluated with respect to observed operations to categorize the error associated with the lack of representation of reservoir coordination. Authors conclude that reservoir coordination is needed to represent flood and drought in typical reservoir models, and that optimization of rules with foresight would help in this endeavor. All simulations were performed on very high performing computational resources taking 2 days for 8 year simulation over the Upper Snake River Basin.

The subject is very interesting for the HESS community and the manuscript is well written but there are a number of concerns that would need to be addressed before consideration for publication. The main concerns are about the two (great) highlights of the paper : the new model and the time sensitive analytics; i) the manuscript presents a new large scale model, with a very interesting concept for the releases that is however not enough evaluated and discussed, and ii) the approach to quantify the contribution of reservoir coordination to better represent floods and droughts needs to be improved – it is based on inference statements and the model could be modified to include information about upstream reservoir release to demonstrate the point about coordination. Minor feedbacks are that the reference to typical reservoir model is misleading and the analytics with the method of Morris is very hard to follow.

1) Reference to typical reservoir operations model seems misleading. At the scale of the Upper Snake River Basin, typical reservoir operations models have a nodal architecture and represent accurate reservoir operating rules that can be revisited in optimization mode and especially in forecast mode to mitigate reservoir and drought events. The manuscript here refers to very large scale spatially distributed reservoir models that have been developed initially to be fully coupled with hydrology model and research land-surface-atmosphere interactions. Those models are typically applied

over multiple independent large river basins. I would suggest to not refer to typical reservoir model where most of the community understand reservoir models where rules can be optimized and are applied to one basin at a time. Please refer to large scale distributed reservoir model or equivalent differentiation from nodal operational reservoir models.

2) A new large scale reservoir operations model : please provide more details - what is the river routing process for this high spatial resolution and daily time step? A recommendation in the introduction is not to aggregate reservoir storage but many reservoirs have less than 2 days in travel time. How does the reservoir model decision release algorithm adjust stability?

- How are the 6 parameters initialized? Are the necessary data widely available? What are the assumptions?

- Evaluation of the smoother release curves with other models. In other equivalent models that are cited (Hanasaki, Doell, Biemans, Voisin, etc) , releases are decided daily based on reservoirs minimum and maximum capacities, minimum environmental flow and tend to follow monthly storage and releases targets with no foresight, but using long term mean monthly inflow, which also tends to be regulated or natural flow depending on the models. What is the improvement for those rules? The obvious features are the changes in release rates - how does it improve the flow representation in general?

- reservoir coordination. Note that the use of a rolling past 20-year of mean monthly regulated inflow provides a minimum of reservoir coordination mostly during extreme events. “Some” coordination is represented through the use of mean monthly regulated flow and also the allocation of water demand to a number of reservoirs based on how full they are. This feature is not present in this model representation, and would likely not drastically change extreme events. Yet it does represent “coordination” around releases and other water management performance metrics than flow and storage, rather

coordination on meeting basin-scale water demand. There were statement throughout the paper saying that there was no coordination at all, which seemed then inaccurate and should be clarified.

- evaluation of the model and transfer to other regions: âĂć whether the coordination between reservoirs was represented or not, how does it affect the vulnerability metrics at the scale of the basin, which is what those models were initially developed for? âĂć Most of those models have been developed for application to a wide range of climatology conditions. The model here is applied to a relatively very small basin for its kind. If this manuscript will be used as reference for this large scale reservoir models, it should be either evaluated with respect to other generic rules, or the applicability to larger regions and very different regions should be presented.

3) Evaluation of the contribution of reservoir coordination – artifact of the model? - the main assumption is that the daily releases are based on storage only. All other equivalent models used an estimate of the expected monthly inflow. The main conclusion of the paper is that the coordination between reservoirs should be represented. While I do believe in this conclusion, it seems that the reference to “typical reservoir model” is not justified if the monthly inflow (proxi for foresight without forward running all the models involved) is not represented at all like in other models. My recommendation would be to modify the experiment to evaluate perhaps incremental and simple levels of coordination (aka adding inflow as parameter for the decision release, or a proxi for inflow) to complement the interpretation of the results and provide more quantifiable statements.

4) Evaluation of the contribution of reservoir coordination during extreme events I found it extremely hard to follow the text and interpretation of the drivers of the release (annual flow versus objective of this reservoir or upstream reservoirs, and shape of release) by just looking at the figures. Most of the text describes the observed operations and coordination and how the model does not capture it. It is unclear how the method of Morris helps with the interpretation during extreme events. While the visualization is

very nice to show the data, it seems that those figures could go in the supplemental material and another figure that compiles those time series and support the text would help.

5) Overall discussion and recommendation versus computational resources needs The authors conclude that foresight should be represented, which is also very sound. Yet the computational resources brought forward for such a relatively small basin are huge which decrease the feasibility at a continental or global scale. Optimization also bring other uncertainties and more computational needs. While the authors seem to indicate that this is what we should do, those were actually drawbacks and motivation for developing those large scale generic models. The recommendation is confusing and perhaps the authors could provide a clarification on new model performances to make it possible now? Please also note that nodal models that typically support reservoir operation optimizations do not provide the spatially distributed feedback into the hydrology model to represent hydrology-land-surface-atmosphere interactions. Maybe the authors meant that we need different types of large scale reservoir models? This would be very sound – just need to be clearer about recommendations then.

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