Response to Reviewer #4.

Anonymous Referee #4

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The authors assess alternative approaches for derivation of reservoir operating curves for the Columbia River Basin as a test case for a broader implementation of reservoir operations in an earth system model. Their focus is the effect on reservoir release behavior of rule priority (flood control, irrigation, or combined), the use of natural flow versus regulated flow, and use of withdrawal versus consumptive use data. In general, the work is topical and interesting, but the manuscript requires major revision before it is considered for publication. General comments are provided below.

We wish to thank the reviewer 4 for his/her comments and constructive criticism which have led to an improved manuscript. Below are answers in line with the comments.

1. The literature review focuses entirely on work to date in reservoir operations from an earth system model scale. Reservoir operations are typically conducted at a much smaller spatial scale, although reservoir operations models increasingly represent large sub-basins or even full river basins at daily to weekly scales for operational purposes. The authors are conducting a different sort of work – clearly – but it would be helpful to place the work in a larger context. The authors may wish to review operational LP models like WEAP and OASIS, among others.

The introduction section had a brief reference to LP models in general. The introduction now clarifies that this water resources management model is not meant to be operational but rather is for large scale hydro-climatological research. We added a sentence with explicit reference to existing LP models to clarify the context of this model.

2. The study objectives (page 3506) are all good and well-stated. I did not find, however, that the authors addressed their research questions sufficiently clearly in the article. The first three points state "how sensitive. . .", but there is no quantification of sensitivity, or even an explicit discussion of sensitivity in the results or discussion sections. In terms of the additional two points, the authors demonstrate the performance of combined operation rules – and show a good match with the behavior of the CRB – but they do not explain how they derived these rules, or compare results between scenarios in any detail. Finally, they show that small-scale behaviors are not matched well (section 4.4.2), but go no farther.

A schematic of the system has been added for clarity in the experimental approach. Metrics to quantify the sensitivity and the improvement have also been added. At smaller scale, the disagreement comes from the hydrologic modeling, but also the reservoir-subbasin database and the operating rules all together. A discussion on the reservoir-subbasin database is presented. We present the capability of the

WM at smaller scale in order to demonstrate areas that need to be addressed, like the partition of the demand to groundwater and surface water. At the regional scale, those needs were not obvious. This has been clarified in the text.

The results section has been revised to answer more explicitely the scientific questions.

3. The methodology requires reorganization and clarification. The first part of the methodology was relatively straightforward. However, despite a genuine attempt to follow the description, I found section 3.3 nearly impossible to follow. The authors need to explain their methodology, rather than simply presenting steps. It may also help to insert a figure or schematic of the overall approach. They should also present more of the detail: why variable timesteps? What, explicitly, are the spatial scales of the analysis and the various models? How many reservoirs are in their model (did they model 29, or 125 as in GRanD), and why those particular reservoirs? What was the form of the water resources model described in 3.4? Perhaps most importantly, they need to describe much more clearly what their contribution was to the work of Hanasaki et al., Haddeland et al., Doell et al., and Biemans et al. – what was new? What was adapted from other studies? Such a discussion is in the manuscript, but grammar and writing need to be revised so that the message is clear.

The section has been revised. A schematics and tables were added that clarify the modeling framework and the sensitivity analysis, and the improvement in the operating rules. Figure 1 has also been revised as to clarify that all reservoirs are simulated.

4. The scenarios need to be explained in greater detail. Table 1 was not sufficiently clear or detailed.

Scenarios are graphically explained in the new schematic, which is complemented by Table 1 in order to place them in previous studies context. The schematics is pretty central to the manuscript.

5. The results section should be revised to provide additional analysis. In general, the authors present "results", but do not explain them or their causes in sufficient detail. As an example, the Snake River Basin is said to be very dependent on groundwater. The authors attribute a mismatch between their results and observations to groundwater use, but they do not provide any figures to justify this argument. A second example: in section 4.2, the authors simply observe that the flood control and irrigation priorities "do not allow for a realistic representation. . .". It would be useful to explain why these rules did not work.

Section 4.2 has been revised to be more explicit in the justification of the statement.

The discussion on groundwater has been expanded as well. Extraction from confined aquifer means that the water is extracted before reaching the stream – at longer time scale. Once equilibrium is

reached, extraction from groundwater is linked to the surface water system and this is the most difficult to represent. Over the Yakima River Basin for example where a large portion of the supply actually comes from groundwater, our system overall capture it and satisfies the demand. In the case of the Snake River Basin, extraction comes from unconfined aquifer, i.e. not linked to the surface water system at scale we can simulate here. This is then represented as an additional source of water. In our modeling framework that would correspond to a demand that should be adjusted. This is an area of research that is pointed out in this analysis at the sub regional scale, but is out of scope of the paper. The discussion section has been expanded though.