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Hydrology and Earth System Sciences
Discussions

Interactive comment on "On an improved sub-regional water resources management representation for integration into earth system models" *by* N. Voisin et al.

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.

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.

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.

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.

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

clear or detailed.

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.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 3501, 2013.