Reviewer Comments

Major comments/questions

1. The model is formulated as a normative tool assuming perfect hydrologic foresight and a centralized, omniscient basin authority. While this is a standard and valuable approach for identifying ideal solutions, the manuscript should more critically discuss the implications of this assumption. Please expand the discussion (e.g., in Section 5.2) to explicitly address the gap between these normative results and the outcomes achievable under real-world conditions characterized by decentralized decision-making, political economy constraints, and imperfect information. Furthermore, the conclusion's claim of the model being a "replicable method" requires support. Please discuss the preconditions for transferability (e.g., data requirements, governance structure) and distinguish between generic model components (e.g., PMP, node-link architecture) and those that are context-specific.

2. The introduction and discussion could more sharply articulate the specific methodological advancements of this model compared to existing basin-scale or multi-basin hydroeconomic optimization frameworks. What are its unique features or integrative capabilities that provide new insights not possible with previous models? The manuscript should be refocused to highlight these novel, quantitative discoveries. Several central findings (e.g., agriculture's dominant water use, importance of reservoir coordination) are well-established qualitatively in the YRB literature. The paper's contribution would be significantly enhanced by shortening the lengthy study area description and expanding the analysis on surprising, nuanced, or quantitatively new results generated by the model.

3. The finding that ecological water use carries a negative shadow price is a critical issue that arises from an incomplete accounting system which ignores the value of ecosystem services. Preferably, the authors should attempt to monetize and incorporate a subset of key ecosystem services (e.g., water purification, tourism, habitat provision) into the objective function, even via a simplified sensitivity analysis scenario. This would demonstrate how internalizing these non-market values alters the optimal allocation and provides a more holistic view of economic benefit.

4. The model treats groundwater as a simple storage node, ignoring dynamic feedbacks such as declining water tables increasing pumping costs and altering river-aquifer interactions. Does the model's optimal solution systematically encourage groundwater overdraft by not internalizing the increasing

marginal cost of extraction?

5. The sediment challenge, central to the YRB, is reduced to a constraint. The massive opportunity cost of water used for sediment flushing is excluded from the economic objective function. Please discuss how this omission might bias the optimal allocation between sectors.

6. The description of the "baseline calibration" is vague. The manuscript would be significantly strengthened by providing quantitative goodness-of-fit metrics (e.g., R^2, Nash-Sutcliffe Efficiency, Percent Bias) for key variables like streamflow at key gauges, reservoir storage, and agricultural water use against observed data. Additionally, the choice to fix socioeconomic conditions at the 2020 level while using 1996-2015 hydrology should be explicitly justified, and its potential impact on the results discussed.

7. The economic conclusions are highly sensitive to exogenous parameters (crop prices, electricity tariffs, demand elasticities). The absence of a comprehensive sensitivity analysis on these key parameters leaves the robustness and reliability of the optimal solution unclear. A demonstration of how the main conclusions hold under different parameter values is essential.

8. Please provide a justification for why a monthly time-step is sufficient to capture critical processes such as flood peaks, short-term crop drought stress, or reservoir flood control rule curves. Furthermore, a discussion on the potential for extending the framework to stochastic optimization or scenario-based analysis for future climate and socioeconomic pathways would be a valuable addition to the “Limitations and Future Developments” section.

Minor comments/questions

1. The abstract contains many summary statements but lacks powerful, concrete data. Please include 1-2 of the most striking quantitative results to enhance its impact.

2. There is inconsistent use of terms (e.g., "Hydropower" in figures vs. "Hydroelectricity" in text). A thorough check to unify terminology is required.

3. Ensure all variables and parameters are explicitly defined upon their first use in the main text.

4. Some figures, particularly Fig. 10 and Fig. 11, are data-dense and challenging to interpret. Improving clarity with better annotations, labels, or a simplified presentation would enhance reader comprehension.

5. The supplementary material is extensive and helpful. However, please ensure that all tables and

figures in the supplement are explicitly referenced at the relevant points in the main manuscript.

6. The "Limitations" section should be revised to explicitly acknowledge the points raised in this review regarding governance assumptions, dynamic hydrological feedbacks, and valuation shortcomings.