

We sincerely thank the reviewer for the positive evaluation of our manuscript and for providing the opportunity to revise it. We have carefully considered the comments raised by the reviewer. We hope that the revised manuscript now meets the quality standards for publication. Detailed responses to the review comments are provided below (blue text indicates our responses and black text indicates the original comments).

Specific Comments

Comment 1: Regarding the construction of the surrogate model, the current description of its input variables (e.g., which key GSFLOW parameters or optimization decision variables are included), the size and dimensionality of the training sample set generated via Latin Hypercube Sampling, as well as the method and specific proportion for dividing the training and test sets, remains insufficient. Additional explanation is needed.

Response: Thank you for pointing out this deficiency. We have supplemented the description of the surrogate model construction process in Section 3.3 of the revised manuscript.

“The input variables of the surrogate model are the decision variables to be optimized. Specifically, they include the planting areas of six major crops (cotton, maize, oil crops, vegetables, melons, and fruits) across seven irrigation zones, as well as the allocation coefficients of ecological water use during key months (May-September), resulting in a total of 47 decision variables. The output variables of the surrogate model consist of two hydrological variables simulated by the coupled hydrological model (GSFLOW), namely the change in the average groundwater depth in the study area and the area of the terminal lake. To construct the surrogate model, Latin hypercube sampling was first applied to generate samples within the input variable space. Each sample set was then used as input to the GSFLOW model to obtain the corresponding outputs, thereby forming an input-output dataset. The dataset was subsequently randomly divided into a training set (70%) and a test set (30%). The training set was used to train the radial basis function neural network (RBF-NN), while the testing set was used to evaluate the predictive accuracy and generalization capability of the surrogate model.”

Comment 2: Although the paper mentions that model results still require validation

with field data, the discussion of potential real-world constraints during the implementation of the proposed solutions is not yet sufficient. It is recommended to supplement the analysis by discussing the potential impacts of the Pareto-optimal solutions—such as the suggested dynamic adjustments to cropland scale and planting structure—on local livelihoods (e.g., residents and the economy), regional agricultural production stability, and food security. This would strengthen the linkage between the research findings and actual management decision-making.

Response: Thank you for this important suggestion. We agree that if the “optimal solutions” derived from mathematical models are to be translated into feasible management decisions, they must confront complex socio-economic realities. Therefore, we have added the following discussion in Section 5.2 “Limitations of the Study.”

“Third, the optimization model is developed primarily from the perspectives of resource efficiency and ecological benefits, without fully accounting for the socioeconomic constraints that may arise during implementation. For instance, dynamic adjustments to cropping patterns (such as reducing high water-consuming crops) could alter labor demands in specific agricultural sectors, affecting livelihoods, particularly in regions with limited alternative employment opportunities. From a regional economic perspective, while transitioning to high-value crops improves water use efficiency, it may also increase the vulnerability of agricultural systems to market price volatility. Moreover, changes in the scale and layout of cultivated land must align with national food security goals. Effective implementation of the optimized solutions therefore requires supporting policy instruments, including ecological compensation mechanisms, farmer training programs, market risk management, and cross-sectoral governance capacity. Future research could enhance the social acceptability and feasibility of these solutions by integrating farmer behavior models or regional economic models into the current framework.”

Comment 3: The paper notes that the estimation of agricultural non-point source pollution relies on a simplified model, but the explanation of the method's applicability assumptions and limitations could be further deepened. It is suggested to elaborate on the context in which the simplified method is applied in this study (e.g., its

computational efficiency advantages in strategic, large-scale multi-objective optimization analyses) and to specify its main limitations more clearly, such as the inability to depict the transport and transformation processes of nitrogen in the "soil-groundwater-surface water" system. Building on this, a more targeted discussion could be provided regarding potential future improvements, such as coupling the hydrological model with a biogeochemical process model (e.g., MODFLOW-MT3DMS) to enhance the precision and reliability of pollutant simulation.

Response: Thank you for the reviewer's insightful comment. We have provided a more detailed description in the discussion section regarding pollution estimation in Section 5.2.

“This study employs the Export Coefficient Model to estimate agricultural nitrogen loads. This method offers high computational efficiency for strategic, long-term analyses at the watershed scale, making it suitable for the rapid assessment of pollution risks under multiple scenarios. However, as a static, spatially lumped empirical model, it is inherently limited in its ability to accurately describe the transport and transformation processes of nitrogen within the soil-groundwater system and their spatial heterogeneity under different irrigation or fertilization regimes. Therefore, the model is better suited for comparative analysis and relative risk assessment among different management schemes, rather than for the precise, process-based quantification and prediction of pollution.”

Comment 4: The justification for selecting the compromise solutions (S5, S10, S15) in Section 4.2.2 is currently quite brief. It is recommended to supplement the explanation with the specific criteria and rationale used for choosing these compromise solutions, thereby increasing the feasibility and transparency of the proposed method.

Response: Thank you for pointing out this issue. We have supplemented the method for selecting compromise solutions in Section 4.2.2.

“To objectively select a compromise solution from each set of Pareto optimal solutions, this study employs the Technique for Order Preference by Similarity to Ideal Solution (TOPSIS). First, all objective values are normalized, and the ideal solution (composed of the best value for each objective) and the negative-ideal solution (composed of the

worst value for each objective) are defined. Then, the Euclidean distance of each solution to both the ideal and negative-ideal solutions is calculated. Finally, the solution that is closest to the ideal solution and farthest from the negative-ideal solution is selected as the compromise solution.”

Comment 5: It is suggested to add a technical workflow diagram in the Methods section to clearly illustrate the overall process and data flow among the key steps (e.g., SRM, GSFLOW, surrogate model, NSGA-III optimization). This would enhance the intuitive understanding of the framework's logic.

Response: We thank the reviewer for the suggestion. In response, we have added a schematic workflow in Section 3 “Methods” of the revised manuscript. The figure illustrates the complete processes, including input of basic data, coupled surface-groundwater model simulation, surrogate model training, NSGA-III multi-objective optimization, and decision support. It shows the logical relationships among the different modules.

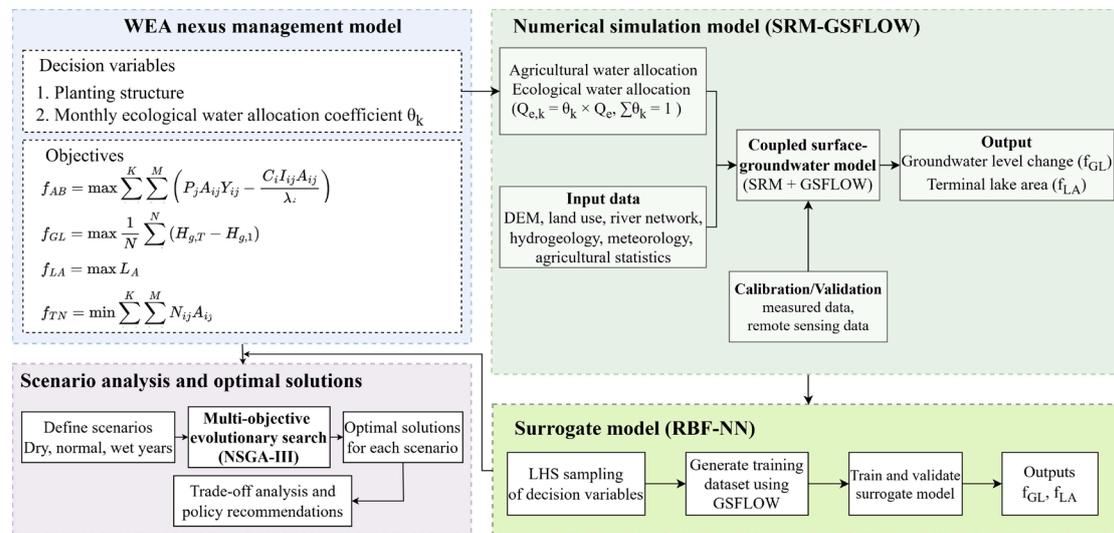


Figure R1. Framework of the multi-objective simulation-optimization for the WEA nexus management.

Minor Comments

1. It is recommended to unify the formatting for numerical ranges throughout the text, for example, consistently using the format " $11.3 \times 10^4 - 14.3 \times 10^4 \text{ hm}^2$."

Response: Line 22. We have standardized the formatting of all numerical ranges

throughout the manuscript according to the recommendation.

2. The thresholds used in Table 4 for classifying hydrological year types based on the runoff anomaly percentage (P) are not rigorously defined. It is recommended to use symbols such as " \leq " to clearly specify the interval boundaries.

Response: Line 358. We have revised Table 4, using the symbols " \leq " and " $<$ " to clearly define the interval boundaries for each hydrological year type.

3. The terminology for describing hydrological year types is inconsistent in the text. It is suggested to standardize the terms, for example, consistently using "wet/normal/dry year" throughout the main text, avoiding mixed usage with terms like "High/Low Flow Year."

Response: Line 358 and Line 502. In the revised manuscript, we have consistently used the terms "wet year," "normal year," and "dry year" to ensure terminological consistency.

4. There are errors in the sub-figure labels in Figure 9. The letters (f)-(l) overlap with (i)-(q), and the number of labeled sub-figures does not match the number of representative schemes described in the text. Please correct the labels according to the actual content.

Response: Line 504. We have checked and corrected Figure 9, ensuring that each sub-figure accurately corresponds to the 15 representative solutions (S1-S15) described in the text.