

Interactive comment on “Basin-scale multi-objective simulation-optimization modeling for conjunctive use of surface water and groundwater in northwest China” by Jian Song et al.

Joseph Kasprzyk (Referee)

joseph.kasprzyk@colorado.edu

Received and published: 11 July 2019

I am serving as a requested referee for this manuscript. The paper presents a new optimization algorithm, linked to hydrological models for the purpose of informing water management in China. Overall, the paper does provide an interesting case study. However, the authors could do a better job of contextualizing their work relative to the state-of-the-art literature in this field, summarized in my general comments below. I also provide specific comments referencing lines in the manuscript itself.

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General comments:

I. The need for a new MOEA should be justified. Moreover, since MOEAs are typically designed for general case studies outside of water management, the authors should indicate whether the new algorithm is available for use.

II. I would like to see more description of the optimization in general, since the calibration of hydrological models is not really the focus of the analysis.

III. The results should be generalizable to a broader context. What are the take-home messages for the HESS audience? This is hinted at in the Conclusion, but could be better motivated in the Introduction.

Specific comments, where line numbers refer to the PDF version of the HESSD paper:

1. The authors should consider editing lines 25-29 to clarify the novelty of the paper. A study of one basin in China may not be compelling to an international audience, so if there is something new about the coupling of optimization to model, that should be highlighted. The same comment is relevant for the introduction; the scientific contribution of the paper is not sufficiently stated.

2. line 48: There are several grammatical errors in the beginning of the paper ("In arid and semi-arid basin,") as well as a disconnect between talking about water management in general and moving quickly to the specifics of China. A native English speaker should proofread the manuscript throughout.

3. line 73-74: "to tackle intricate SW and GW management model": Is this a typo? I think the intended word might be "SW and GW management problems". Also "tackle" is probably not an appropriate word to use.

4. line 77: Before the first mention of "bi-objectives", the authors should provide a very brief introduction to optimization. Otherwise, readers may be confused by what is meant by "objective" throughout this paragraph.

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5. When introducing MOEAs, it would be good to cite Maier et al (2019), which is an introductory overview appropriate for readers to be introduced to the topic.

6. lines 102-113: I am glad the authors have brought up some recent and relevant topics in many-objective optimization. However, the paragraph was confusing and will be difficult for readers to follow. For example, the Borg algorithm is briefly mentioned, but there is no clear transition to the next algorithm ("In order to enhance the local optimality..."). Did Sindya et al. add to Borg or create a new algorithm? Moreover, it is unclear whether the authors made a new algorithm, and whether it builds on the work of Hadka and Reed and Sindhya. Moreover, given that these new algorithms have been extensively tested (e.g., Reed et al 2013), it is worth justifying why a new algorithm is needed.

7. line 114: The section would benefit from a better transition between the MOEA material and the GW modeling material. Also, since the GW modeling is being done in the context of decision making, I would like to see a clearer discussion of the decision variables and objectives of the optimization as the problem is being introduced.

8. line 125: Ideally, a paragraph would express one idea at a time. Here, the authors have transitioned from discussing their method to providing details about their case study. This material should be separated.

9. line 168-169: What is meant by "decision makers" here? In many systems, different people make decisions about the irrigation diversions, lake storage, and groundwater pumping. Without a clear context for decision making, this section is too vague.

10. line 179-186: There is some repetition here compared to the introduction. Although I agree with the points about nonlinearity, nonconvexity, etc., it is more useful at this point in the paper to explain the details of the proposed new algorithm.

11. Is this the first introduction of the e-MOMA algorithm? If not, it would be very helpful to have a citation to the original reference, since there is not enough detail given here.

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At the least, the authors should justify how their algorithm differs from Hadka and Reed, and others.

12. line 253: The discussion of the Ecological Water Conveyance Project is interesting. I'd like to see it integrated better within the text. Is this study supporting that analysis?

13. Equations 3-5: Why were different metrics used for different variables?

14. line 322-323: This statement should be justified. It speaks to the wider question of how the hydrological modeling is serving the ultimate goal of the management problem, as well as the general contribution of the paper itself. If the focus of the paper is too diffused, it becomes hard to follow its details.

15. line 348: To what extent can the groundwater extraction rate be controlled? In some systems, farmers have jurisdiction on how much to pump. If there is an implicit assumption about a set of water managers who can dictate water usage, this should be stated.

16. lines, 353, 357, etc.: Is there a citation to the water price data? Or was this just an assumption?

17. line 406: Guidance on interpreting parallel plots should be provided.

18. line 534: When the authors say "This study implemented...", were they implying that this occurred across the entire study? Or only in one part of the study? This should be clarified.

19. line 541-542: My impression is that hypervolume analyses are usually done to compare optimization runs with the true Pareto set. Is this known? In general, since the optimization seems to be the focus of this paper, items such as Hypervolume Analysis should be covered in the Methodology (which means that some hydrological modeling detail can be removed)

20. line 546: "obvious" is usually not appropriate in technical writing.

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21. Conclusion section: The quality of writing here is much better than in the introduction. Some of this material should inform the Introduction, since this more clearly articulates the purpose of the study than the beginning of the paper did.

22. In spite of comment #21, I would like to see slightly more discussion about the management implications of this study – in the local case study as well as how the results can be transferred to other basins (especially given different legal and regulatory structures).

23. Table 1: Was random seed analysis performed? If so, the parameters of this analysis should also be provided here. The epsilon values seem quite small – were larger epsilons attempted?

24. Figure 4: If the paper is too long, I could imagine this figure could go into supplemental material. Also, I noticed that the NSE values appear in the figure but were not referenced in the text.

25. Figure 9: If possible, the other solutions that are Pareto optimal in 4 dimensions but not in two, should be shown on this plot. Otherwise, the idea that the highlighted solutions fall "outside the front" will be confusing to readers. The Kollat and Reed (2007) paper referenced in this manuscript shows how to do this.

26. Figure 12: The figure would be easier to understand if the authors reminded reader what these scenarios represent (see also comment #18 – the scenario analysis could be better explained overall).

References

Maier, H.R., Razavi, S., Kapelan, Z., Matott, L.S., Kasprzyk, J., Tolson, B.A., 2019. Introductory overview: Optimization using evolutionary algorithms and other metaheuristics. *Environmental Modelling & Software* 114, 195–213. <https://doi.org/10.1016/j.envsoft.2018.11.018>

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ary Multiobjective Optimization in Water Resources: The Past, Present and Future.
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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-278>, 2019.

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