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Hydrology and Earth System Sciences
Discussions
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Interactive comment on "Optimum Water Quality Monitoring Network Design for Bidirectional River Systems Using Modified MOPSO" *by* Xiaohui Zhu et al.

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The paper by Zhu et al. deals with optimizing sensor placement in a (river) network considering BIDIRECTIONAL flows, which to my knowledge is novel and warrants publication. Also, the use of the MOPSO algorithm seems justified. Subsequently, I summarize the main points, which I think deserve careful consideration before acceptance.

Content:

1) Only one river catchment. Why do the authors only present one hypothetical river catchment? I understand it is for the sake of comparability with another study, but

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a more general discussion on how the topology of a river network may influence the optimal placement of monitoring stations would add value to the paper. One or two other (hypothetical) river catchments of different structure would be valuable – or a discussion on why the presented river network is covering all (most) typical real world cases.

2) Number of monitoring stations. As far as I can see, there is no discussion/justification for the choice of number of monitoring locations. Only the case with three monitoring locations is presented. Why not make it more general (maybe also in combination with the point above, the number of required monitoring stations is likely to depend on size of the river network and number of branches)? Or at least provide a justification for the selection of three locations (costs?), which would help the reader to understand this selection better.

3) Criteria for combinations of monitoring stations. For river network A, 13 combinations of three monitoring locations are presented in table 3. Are these all combinations tested by the authors? I tried finding a corresponding explanation, but did not find one in the text. For river network B, 9 combinations are listed (table 5) and they differ from the combinations tested for river network A. For the bidirectional river network it is a number of 10, again different combinations (table 6). Also for assessing the effect of different detection thresholds, the combinations tested differ (table 19). Only for the comparison of different probability ratios, the same combinations are compared (table 7). Without an explanation and without knowing if these are all tested combinations (and why almost all are different selections), the choice seems arbitrary. If there was a pre-selection step for the identification of the most suitable(?) combinations for each scenario, then this should be written somewhere. Otherwise it hampers understanding how the authors reach their main conclusion " [. . .] bidirectional water flows in a river system have a significant effect on the optimum design of water quality monitoring network and the deployment result is quite different from the same river system with a unidirectional water flow."

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4) Aspects of pollution detection. Pollution detection probability and detection time are addressed. Implicitly, I think the authors also wish to identify location of pollution. If yes, I believe this third aspect would warrant more explanation.

5) SWMM model. The suitability of the SWMM model maybe open for discussion. A brief discussion of advantages and drawbacks of using a 1D or 2D model would support the decision to use SWMM (depending on river width/mixing/curves/confluents and type of sample collection across the river section). It appear as if the application of SWMM was ok for the main purpose/aspect of this work.

6) Threshold considerations. It seems evident that a detection threshold higher than the actual pollutant concentration affects detection probability and time. However, this is primarily a sensor sensitivity issue and not a monitoring station placement aspect. In analytical chemistry this is discussed with the terms limit of detection and limit of quantification and is a well-known phenomenon. I suggest not presenting this as one of the main conclusions.

7) Conclusions. Summarizing the above, I think the informational content and novelty of the paper would gain if points 1, 2 and 3 could be addressed and corresponding findings would strengthen the conclusions.

Formal:

8) Terminology: I am not sure what the authors mean with “accumulation” (page 16 line 17and page 17 line 7). Is it really accumulation (as for pollutants related to particulate matter – e.g. sedimentation – or in fat tissue of organisms) or is it the overlap of pollution from different sources (increasing the concentration if they coincide in space and time)? This comment also applies to the interpretation of figure 7 (right panel).

9) There are many similar tables and figures. Many of them could be combined to facilitate comparison and minimizing the number (e.g. figures 1 and 3, side by side; tables 3, 5 and 6 could be combined similar to tables 7 and 10). Mainly the large

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number of similarly appearing tables interrupts the reading flow. Particularly tables 2, 4, 8 and 9 and the description algorithms could be transferred to SI. The readability of fables 2, 4, 8 and 9 would gain if “INF” was replaced with e.g. a “-“ or nothing at all (explaining it in the caption).

10) Please label all multi-panel figures properly and provide meaningful captions (e.g. the caption of figure 7 seems wrong, describing another figure).

11) Please check language carefully, I noted some typos, grammatical mistakes, but the list is likely incomplete.

12) Cited literature: Some pertinent general (books) and more specific (also from wastewater sewer system water quality monitoring) literature seems missing. Instead of proposing potential citations myself (which is often seen as a means of promoting certain articles by reviewers/editors; see link below*) I kindly ask the authors to perform a more exhaustive literature search. If the authors wish, I am happy to provide relevant pieces of work that I am aware of on their request.

13) Additional: I made a series of comments/suggestions in the attached PDF which the authors may find helpful to further improve the manuscript.

14) It would be helpful if the manuscript came with unique line numbers (rather than restarting numbering at each page). This would facilitate efficient referencing of comments.

* https://static2.egu.eu/media/filer_public/07/79/07798eae-e4e4-48f2-a9d0-6b8ce0110302/egu-copernicus-report-about-citation-stacking.pdf

Remark:

I thank two of my colleagues who provided two small pieces of information not warranting writing an individual comment. I included both in my review.

Please also note the supplement to this comment:

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http://www.hydrol-earth-syst-sci-discuss.net/hess-2017-124/hess-2017-124-RC1-supplement.pdf

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