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Hydrology and Earth System Sciences Discussions

Interactive comment on "Integrated versus isolated scenario for prediction dissolved oxygen at progression of water quality monitoring stations" *by* A. A. Najah et al.

Anonymous Referee #2

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The paper objective sounds interesting: it promises to evaluate the use of Artificial Neural Networks (ANN) for assessing water quality. There are very few applications concerning ANN in aquatic studies and therefore the applicability of ANN in assessing environmental quality is not known as yet. The subject addressed is within the scope of the journal. However, the manuscript, in its present form, should be improved in the light of the following comments in order to be more suitable for the readers. The overall assessment is that the manuscript could be accepted with "minor revision". Addressing the following comments/modifications could be satisfactory in order to justify recommendation for publication.

Hereafter I am giving my recommended points that should be considered in the revised version of the manuscript.

Comments:

1:- It is highly recommended to enhance the introduction section by introducing citing several researches for Artificial Intelligence application for environmental and / or other fields

2:- The introduction section should be re-arranged by splitting it into three parts, background, problem statement and objective.

3:- Problem statement should be revised to show the contribution of this research in clearer way.

4:- In the description of the study area, it should be better to introduce schematic diagram on the river main stream and tributaries and the location of the monitoring stations under study.

5:- A thorough discussion on the advantages and limitations of the method should be provided.

6:- The key ANN parameters are not mentioned. The rationale on the choice of the particular set of parameters should be explained. Have the authors experimented with other sets of values? What are the sensitivities of these parameters on the results?

7:- The discussion section does not really address the significance of these results or set them in the context of the cited. The authors should rewrite the commentary to better explain how the results could be used for better modeling, lessons learned etc. At present the ms reads more like a general description of the work carried out, rather than taking the reader deeper into the processes and mechanisms involved.

8:- Why are the three performance indices (i.e. Coefficient of determination (R2), Mean Absolute Prediction Error (MAPE) and Coefficient of Correlation (CC).) adopted in this

study? What are their advantages over other indices in this case?

9:- The author verify the efficiency of the model using field data during the period 2009-2010 using the proposed model using the integrated model ONLY. The author should provide the performance of the isolated scenario for these data as well.

10:- In the conclusion section, the limitations of this study, suggested improvements of this work and future directions should be highlighted.

11:- There are a few formatting issues that would improve clarity, which mainly involve figure font sizes.

12:- Line 26, Page 6076, xi: there is no xi in the mathematic formula. Clarify?.

13:- There are a few typos in the text that the authors should address.

In conclusion, this reviewer believes that the manuscript would be considered for publication after performing the above-mentioned comments.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 6069, 2011.