

## ***Interactive comment on “Assessing water resources management and development in Northern Vietnam” by A. Castelletti et al.***

### **Anonymous Referee #2**

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Review of the submitted article “Assessing water resources management and development in Northern Vietnam”, by A. Castelletti et al.

General assessment:

This is an interesting article that presents an approach for multi-objective assessment of reservoir operation policies, using different types of simulation models (a reservoir model and a data-driven model – Artificial Neural Network (ANN) for the downstream impact) combined with optimisation using genetic algorithms and comparison with deterministic dynamic programming (DDP) approach. The area of application is Northern Vietnam (Red River Basin) with focus on Hoa Binh reservoir operation, which is one of the most important reservoirs in the country. The main value of the article is in combin-

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ing different simulation and optimisation approaches (existing and already known) for analysis of existing reservoir operation policy and recommending improvements of the policy on the basis of multi-objective optimisation. The material presented is relevant for the area of North Vietnam (as it recommends improvements in reservoir operation), but it also has broader significance because of the potential applicability of the methodology in other parts of the world with similar problems. This relevance leads to the recommendation for publishing the article in the HESS journal. However, there are several issues that need to be addressed before final acceptance, primarily related to the way of presentation of the material. The article requires some improvements in the structure and in better explanations of some important aspects so that the readers can understand better the methodology of combining the different methods (which is, in my opinion the main value of the article).

Specific comments:

1. The authors are invited to revise the structure of the article and in section 2 (Systems and models) to describe the methodology of using the simulation and optimisation models and their sequence, with clear formulation of the optimisation problems that they address. This is recommended to be done before introducing the indicators (objective functions). This will improve the clarity and the flow of the paper.
2. Most serious concern is the insufficient explanation of the multi-objective optimisation by MOGA currently described in section 3. The ANN model used for the release decision (presented in eq. 8) is not well explained. The critical question about which decision variables are being manipulated needs to be explained much better. What are the inputs to this ANN model that produce the release decision as an output? How are these inputs related to the “network parameters” (p7189, L22)? How do individuals made of combinations of these network parameters lead to a different release decisions? (“the parametrisation under exam” in L26 on the same page needs to be explained). This part needs to be elaborated much better, and each variable used in the equation(s) needs to be explained (this is also a comment for all equations in the

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article). Given that in the following of the paper the MOGA solutions are compared to the DDP it needs to be very clearly explained what is the difference in these two approaches, especially regarding the information used for optimisation (DDP uses perfect information, but what is used in MOGA is not clear – the conclusions section p7194, L22-23 mentions that only reservoir storage and time of the year are used as inputs, but this needs to be explained earlier). This section is very important for the article and needs to be explained much better. In fact most of these aspects can already be introduced in the beginning of section 2 if the paper structure is revised (see comment 1)

3. In relation to the previous comment, a clear distinction between the ANN model introduced under section 2.2.2 used for simulating the downstream impact and the ANN model of section 3 used for the MOGA optimisation needs to be provided. Use of similar notations and poor explanations in section 3 leave the reader with some confusion about these two models. My understanding is that the first ANN model (from section 2.2.2) is used for simulating the downstream impacts (at Sontay or Hanoi, because there are in fact two such models, one for qST and another for hHN), given certain releases from the Hoa Binh reservoir ( $rt+1$  in eq. 7) and other inputs. The second ANN model (from section 3) is used for optimisation (determining optimal release  $ui$ , from eq. 8) and the releases obtained are used in the simulation of the first model. In this sense the  $ui$  of eq. 8 and  $rt+1$  of eq. 7 are same. This should be better clarified, however.

3. Regarding the simulation ANN model (section 2.2.2) there are several comments as follows: a) Like any other model, the usual procedure is to separate the available data sets in calibration, testing and validation periods. The authors state that they have used the whole period of 1989-2004 as calibration period, and that this covers the horizon 1995-2004, which is sufficient for testing different reservoir operation policies (p7187, L15-17). From the point of view of ANN model development, this is an unusual procedure (without separate validation period). The authors are invited to comment

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why this was chosen. b) In the following lines on the same page (L17-21) the authors recognise the problem that ANN models are having difficulties in reproducing system states when changes in the system are introduced, for which there are no data (the discussion on river bed erosion). This may influence the usage of this model for future reservoir operations. The authors are invited to discuss this a bit more (e.g. present the needs for re-training of the ANN model as systems change) c) On the same page (L4-12), the authors recognise that inclusion of lagged values of upstream flows may improve model accuracy, but that they have decided not to use them in order to have a less complex model that can be used for subsequent optimisation. This argument is not entirely satisfactory. If the goal is simulation, once a neural network is trained, the complexity does not matter, and if better results can be obtained by using lagged flow values this should be done. In optimisation stage, one can subsequently decide which decision variables to be included (and it can be more or less complex). Moreover the ANN model used later for optimisation seems to be very different, and the argument about balance between complexity and accuracy for this model becomes even less relevant. This issue may be clarified once the differences between this ANN model and the one used for optimisation (see comment 2) are presented, but I would suggest that the authors simply recommend that the use of more complex models (using lagged flows) is a task for future improvements. d) Further improvements of the ANN simulation model are obviously needed in order to deal with the poor performance for the Sontay model. Even if for purposes of the subsequent analysis (comparison of different operation policies) the authors have decided to use the model-computed indicator values, which is understandable - the needed work on improvement of this model should be recognised.

4. The DDP optimisation and its comparison with MOGA are valuable and well presented. The authors are invited, if they wish, to provide couple of comments / recommendations regarding two issues that come out from their analysis: a) even a complex system like the one presented in the paper can be analysed with DDP after reasonable simplifications, which in turn can provide information about the limits of the optimal so-

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lutions; b) this can help in analysing solutions obtained by algorithms like MOGA, and possibly in designing more complex optimisation formulations (that in reality cannot be solved by DDP).

5. Can the title be made a bit more specific? It sounds too general (there is in fact almost nothing in the article about water resources development, so I don't think that that term should be in the title).

Smaller comments

1. p7178, L26 – "...disasters that occurred..." instead of "disasters occurred..."
2. p7180, L6 – Please show in Figure 1 the whole catchment with the catchment boundaries.
3. p7180, L21 – "...evaluation criteria that relevant stakeholders..." instead of "evaluation criteria the relevant stakeholders..."
4. p7181, L9,10 – "...energy is sold..." instead of "...energy sold..."
5. p7181, L13 – "...but without taking the timetable into account." instead of "... but not with the timetable."
6. p7181, L21,22 – "Since the indicators are formulated..." instead of " Being the indicators formulated..."
7. p7182, L12 – "...problem of computational burden." instead of "...problem computational burden."
8. p7182, L17 – "probably" instead of "provably"
9. p7183, L11 - unclear what is the question mark at the end of the line
10. p7183, L17 – "...flood propagation" instead of "... flood routing"
11. p7183, eq. 5 - is common symbol for water density – not  $\gamma$

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12. p7187, L2 – Please explain all network parameters (see comment 2). All variables in all equations need to be explained

13. p7189, L22 – Same comment as above.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 7177, 2011.

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