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Hydrology and Earth System Sciences Discussions

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

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We wish to thank the reviewer for his/her positive evaluation of the paper and valuable comments. We agree that the methodological contribution of our work should be better highlighted, and we are willing to modify the paper following the reviewer's suggestions. Below is a list of specific replies to the reviewer's 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 ïňĆow of the paper.

Reply: We agree that the formulation of the optimization problems should be better clarified. To this purpose, we can anticipate in the Introduction the workflow followed in our work, and introduce the formulation of the stochastic optimization problem solved by MOGA so that it can be more easily compared with the deterministic optimization problem (9).

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?

Reply: As in standard reservoir operation, the inputs are storage and time. The notation in the paper is unnecessarily complex: a general input vector I_t is used in equation (8), and later in the text it is specified that the components of I_t are the storage s_t and time t. We think that directly using variables s_t and t in equation (8) will make the paper more readable.

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 article).

Reply: Again, we agree that this part of the paper deserves better clarification. We propose to replace the general equation (8) by the following, specific equation

$$u_t = \theta_0 + \sum_{j=1}^{\mu} \theta_{1,j}\ tansig(\theta_{2,j} s_t + \theta_{3,j} \cos(2\pi/T\ t) + \theta_{4,j} \sin(2\pi/T\ t) + \theta_{5,j})$$

that is the one that produced the numerical results presented in the paper. Also, we would like to extend the description of the working principles of MOGA and add several references to the reservoir optimization literature, as also suggested by the other reviewer (C3598).

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)

Reply: When using (simulating) the operating policy by MOGA, the "optimal" release at each decision time step is computed based on the knowledge of the current storage and time of the year. This is the minimum information that is actually available to the manager in real-world operation. In DDP, besides current storage and time, the entire flow time series over the simulation horizon is assumed to be known at each decision time steps within the horizon. This corresponds to assuming an ideal manager who knows not only all past flows but, more importantly, also all future flows. We agree that this should be better clarified in the paper by adding technical details in the section on MOGA and also anticipating the core ideas in the Introduction.

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 ïňĄrst 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 q^{ST} and another for h^{HN}), given certain releases from the Hoa Binh reservoir (r_{t+1} in eq. 7) and other inputs. The second ANN model (from section 3) is used for optimisation (determining optimal release u_i, from eq. 8) and the releases obtained are used in the simulation of the first model.

Reply: Yes, your interpretation is correct. We agree that the current version of the paper is misleading on this point. We think it should be clearer if equation 8 is modified as in the first reply.

In this sense the u_i of eq. 8 and r_{t+1} of eq. 7 are same. This should be better clarfied, however.

Reply: This is not completely true. As stated on lines 6-7 on page 7185, r_{t+1} is the actual release that "coincides with the release decision u_t only if the latter is feasible, i.e. included between the minimum and maximum feasible release that can be obtained when all the gates are completely closed or open, respectively."

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 sufïňĄcient 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 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 difïňĄculties 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 inïňĆuence 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)

Reply: The underlaying idea here is that the relation between flow variables in the river network cannot be predicted by a model that does not explicitly take into account the process of river bed erosion started after the construction of the HoaBinh reservoir. However, while we have information that the process is undergoing, we do not have enough data to develop a model including erosion/aggradation processes. So, the most that can be done is to use historical time series and calibrate a model that can adequately reproduce the flow routing process over the past. Obviously, both the model and the operating policies based on it may prove suboptimal if applied in the future, under changed geomorphological conditions. However, the main objective of the paper, i.e. to assess the space for improvement of the historical operation, is not affected by this limitation; and the simulation and optimization tools here proposed are general enough to be re-applied in the future as new data become available. We will insert these comments in the revised version of the paper.

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).

Reply: This is true for MOGA but it is not for DDP. In fact, in DDP the computing time

increases exponentially with the number of state variables of the global model (i.e. including all modelling units, not only the reservoir). So, using lagged flows in the downstream network model would have added state variables to the global model and then significantly increased the computing time (consider that 3-4 state variables are already close to the limit of dynamic programming).

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.

Reply: The complexity issue discussed at this stage of the paper is not connected with the structure of the ANN in equation (8) but only wit the one in (7). This will hopefully be clearer after revision of the paper as already discussed above.

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.

Reply: Yes, improving the model of the downstream river network is by sure one of the topics of further research.

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 solutions; 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).*

Reply: We do agree with the reviewer comment. We would like to stress this methodological contribution of the paper and highlight the value of concatenating deterministic (DDP) and stochastic (MOGA) optimization tools for better understanding reservoir system functioning and potential.

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).

Reply: By the term "development" we mean the planning of capacity expansion. In fact, as discussed in the paper, application of deterministic optimization is a means to evaluate the need for increasing the storing capacity in a basin and thus support planning and siting of new reservoirs. However, we agree that the title may be improved to better focus the scope of the paper, so we think it may be changed in "Assessing water **reservoirs** management and development in Northern Vietnam".

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 "... ïňĆood routing"

11. p7183, eq. 5 - is common symbol for water density - not γ

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.

Reply: Ok, thank you

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