

Interactive comment on “A framework for the quantitative assessment of climate change impacts on water-related activities at the basin scale” by D. Anghileri et al.

Anonymous Referee #2

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General comments:

This paper addresses the issue of climate change impact assessment on water-related activities in the Lake-Como region, Italy, by proposing a framework in which regional climatic models (RCM) that use as input the outputs from a Global Circulation Model (GCM) are downscaled to provide inputs to a hydrological rainfall-runoff model, which then provides inflows to reservoirs that are used for water-related activities in the area. The focus is on hydropower production and irrigation – two main water-related activities in the region. With indicators for these two objectives developed via stakeholder consultation process, a multi-objective optimisation procedure is used with a reservoir

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model, which allows for analysis of optimal policies of reservoir operation under conditions of climate change. In addition to the proposed framework, the paper discusses different sources of uncertainty that may influence the impact analysis on the chosen indicators. While different sources of uncertainty are discussed, the paper presents results on uncertainties arising from choice of simulation horizon, choice among different RCMs and, to a limited extent - uncertainties from modelling error in the rainfall-runoff model.

The paper is well structured and very well written. It is within the scope of the journal and is recommended for publishing, after addressing the specific comments listed below:

Specific comments:

1. A valuable contribution of this paper is the recognition of the uncertainty arising from future development of the socio-economic system. Even though there is no quantitative analysis of this kind of uncertainty, it is clearly indicated that in fact it may be the most significant of all sources of uncertainty. It not only arises from the choice of the climate scenario (only one such scenario was used here -A2, IPCC (2000)), but also from conditions such as agricultural and energy policies, which may affect costs and prices, as indicated in the paper. However since no specific analysis of such uncertainty is presented (the corresponding section only contains re-optimisation procedure under new inflow scenarios), it is recommended that the abstract of the paper is somewhat modified:

Page1, lines 14-17: In the sentence “An uncertainty analysis is performed... “, should be modified because no analysis of impact of approximations in modeling the socio-economic system are presented. A sentence that it was “discussed” may be included.

2. The authors have stated on several occasions that the indicators were developed in consultation with stakeholders. Since this is mentioned as one of the characteristics of the proposed approach, it will be good to mention in couple of sentences, who

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were these stakeholders and how did they agree on these indicators (not just pointing to existing literature). Page , line 164, may be a good location for inserting these explanations.

3. More generally, the actual stakeholders may be many, and they may be spatially distributed (if we just think of the many farmers, who are using the irrigation water from the reservoirs). Effects of optimal policies, derived with the demonstrated procedure, with or without climate change impacts, may be varying for different stakeholders. Any attempt to address such effects would require development of more detailed, spatially distributed simulation models that can then be used in participatory planning processes with larger number of stakeholders. This may become even more important when other objectives (such as ecology) are included in the analysis, as the authors anticipate. Without this step, the whole framework proposed, may still be experienced as a 'top-down' decision making process by individual stakeholders. The authors are invited to consider including comments about this, preferably in section 5 "Conclusions".

4. Also in relation to the previous comment, the authors are invited to give few comments on their perception about the use of their proposed framework for actual decision making in future planning, especially in light of all the uncertainties discussed and presented. It seems that the objective of the paper is solely impact analysis, and that the gap between impact analysis and planning decisions is expected to be bridged by political decision making (but this may be a wrong impression). Section 5 ("Conclusions") may again be appropriate for this short discussion.

5. Figures 3b and 3c need to be corrected. The data in Figure 3b cannot be total monthly precipitation expressed in mm/day (the values are too big), and the values in 3c cannot be annual precipitation in mm (also too big).

6. Pages 7-8 (section 3.2) The hydrological rainfall-runoff model used is very simple lumped model. The authors are invited to comment why they did not use more complex, spatially-distributed model. In addition to the coefficient of determination as an

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indicator for good calibration, the authors are invited to present a graph with observed and modelled results in calibration and validation periods. The implication that the absence of a glacier melting model may be a reason for model error is a bit misplaced, because the simplicity and lumped nature of the model are probably much more responsible for model errors. A table with calibrated parameters of this model is also recommended to be included.

7. Pages 9-10 (lines 332 – 354). The authors are invited to comment on the decision to use 10 or 14 years of simulation horizons. Will sliding windows of different simulation horizons change the results presented in Figure 5?

8. On several occasions the authors give reference to (Anghileri et al., Submitted). This is unusual as that reference cannot be checked yet. Please check the policy of the journal, but it is recommended to avoid such references and include relevant information directly in this paper.

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