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Hydrology and Earth System Sciences Discussions

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Interactive Comment

Interactive comment on "Holistic versus monomeric strategies for hydrological modelling of modified hydrosystems" *by* I. Nalbantis et al.

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Response to Anonymous Referee #3

Response to general comments:

We thank the Anonymous Reviewer (herein referred to as the AR3 or with the pronoun "he" and "his" referring to a common gender) for his detailed and constructive review, as well as his positive general comment, which refers to the most essential elements of our work. We are particularly glad to see his comment that our study is an "illustration that some preconceived ideas commonly seen in the literature can be wrong."

Below we have copied all his detailed comments and replied to each one separately.

Response to detailed comments:

1. Page 8266, line 7: The term "monomeric" could be defined here in a few words

The following definition will be added in the text: “A modelling approach is defined as monomeric when some, one or a few, components, processes or information regarding the studied system are examined in detail, while at the same time others are roughly accounted for or even omitted.”

2. Page 8267, end of abstract: A few sentences could be added on the main outcomes of the comparison

In the end of the Abstract we will add the following text: “Our work allowed for investigating the deterioration of model performance in cases where no balanced attention is paid to all components of human-modified hydrosystems and the variables related to these components. Also, sources of error were identified and their combined effects were quantified.”

3. Page 8268: The discussion opposing the monomeric and the holistic approaches could probably be made more balanced at this stage. There are numerous "integrated" physically-based models (e.g. the SHE model, see the recent discussion by Refsgaard et al., 2010) that can help solving complex water management issues. Some comments could also be added on the contrasted results obtained in past studies of comparisons between TD and BU type models (see e.g. Refsgaard, 1997).

We agree with the AR3 and we will amend the manuscript according to his suggestion. Also, the references will be added in the revised manuscript. We thank AR3 for these references, which enhance the ideas examined in our paper.

4. Page 8268: Some more examples could be given on the attempt to account for human influences following TD approaches (see e.g. Ivkovic et al., 2005; Payan et al., 2008)

The examples will be added to the revised manuscript together with their references;

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in fact, the work of Ivkovic et al. was not available; yet we found a related publication in a peer-review journal to cite:

Ivkovic, K. M., R. A. Letcher, and B. F. W. Croke, Use of a simple surface-groundwater interaction model to inform water management, Australian Journal of Earth Sciences, 56(1), 71-80, 2009.

We thank the AR3 again for this constructive comment.

5. Page 8269: I found it would help the reader if names more explicit than "A" and "B" had been given to the two modelling approaches (e.g. BU-M for bottom-up-monomeric and TD-H for top-down holistic). Sometimes one feels lost on the meaning of A and B. If this is changed, this should be changed throughout the text and in figures and tables.

As already noted in our reply to the general comment 2 of the Anonymous Reviewer 1, on page 8269, lines 14 and 15 we state “To represent the BU-M approach we will consider a particular modelling strategy, called here strategy A.”. Later on page 8270, lines 8 and 9 we define strategy B by saying “An alternative modelling strategy, called here strategy B, will be used to represent a top-down/holistic approach.” In fact, strategies A and B are only instances of approaches BU-M and TD-H in the sense that their characteristics are specific (e.g., in strategy A only groundwater processes are modelled in detail). Within the two strategies we further restrict our research by employing specific modelling frameworks. On page 8271, lines 4-10 we give a brief description of the frameworks used. Later (page 8283, lines 18-24) we explicitly say what we did, i.e., our research was limited to the combined effects of all key modelling options defined. It was therefore our desire to avoid confusion in defining progressively narrower tool categories that dictated our choice. The distinction between approaches and strategies required some differentiation between these two kinds of entities. So, we will ask the AR3 not to insist on changing the symbols for strategies, although we recognise that using symbols A and B may slightly harm readability.

6. Page 8269, second paragraph: Some examples from the literature could be given

on strategy A and shortly discussed (see e.g. Hingray et al., 2010)

We will add such examples in the revised manuscript.

7. Page 8270: Multiple citations should appear by chronological order.

We will implement this suggestion.

8. Page 8270, line 22: Add reference to Klemeš (1986)

We will add this reference.

9. Page 8271, last paragraph: It would be interesting to make the parallel between these various options and some modelling guidelines proposed in the literature (see e.g., Refsgaard and Henriksen, 2004; Scholten et al., 2007). To which main stages of the modelling process do these options correspond?

The correspondence of the key modelling option to the main stages of the modelling process is outlined below:

1 Model study plan: None

2 Data and conceptualisation: Key modelling option SW-GW: link between models for surface and groundwater processes; key modelling option SW-GW-WM: link between models for hydrological processes and water management

3 Model setup-up: None

4 Calibration and validation: Key modelling option SCALE-PARAM: link of spatial scale and model parameterization; key modelling option SCHEM-PARAM: link between hydrosystem schematization and parameterization; key modelling option OPT: appropriate use of optimization in calibration.

5 Simulation and evaluation: None

The following text will be added in the revised manuscript (page 8271, line 23):

“These options relate to the specific stages of the modelling process as the latter is proposed by Refsgaard and Henriksen (2004) and Scholten et al. (2007). In short, these stages are: (1) Model study plan; (2) data and conceptualisation; (3) model setup-up; (4) calibration and validation; (5) simulation and evaluation. So, the first two options will refer to model conceptualisation (stage 2) since they deal with selecting hydrological processes and the interactions thereof, while the last three options will have to do with selecting calibration parameters thus referring to stage 4.”

10. Pages 8271-8275, part 2: I found this part of the article too long. I think it could be presented in a more concise way, only keeping the major ideas without discussing them in too many details. A few lines each time should be sufficient. It would be more interesting to discuss these aspects in light of the results presented in the article, i.e. towards the end of the article.

An effort will be made to shorten section 2.

11. Pages 8275-8277: The level of details given on the two approaches could be more balanced. Here the description of approach B is much more detailed. It could be reduced to make things more homogeneous.

It is the structure of the modelling frameworks themselves that led us to such imbalance. Although features and components of framework B are very briefly described, this results in an imbalance. We will make an effort to shorten the description of framework B without dropping any of its essential features.

12. Pages 8277-8283, section 4: I also find this part too long. There are probably too many details that are not essential here. This could be shortened somehow.

We recognise that this material is too long for readers who will be interested in the general ideas of the paper and not its details; however, it would be impossible to significantly reduce this material, since it describes the major assumptions of the two modelling frameworks. Nevertheless, we are willing to make an effort to separate the two levels of detail so as to enhance readability.

13. Page 8275, lines 18 and 22: References describing each algorithm could be added.

References will be added.

14. Page 8279, line 26: The location of rain gauges could be shown in Fig. 2 (or reference to a previous publication where they appear could be given).

Rain gauge locations will be indicated in Fig. 2 of the revised manuscript.

15. Page 8282, section 4.4: I find this approach very interesting. However, it could be acknowledged that it involves somehow some expert judgement that may be difficult to have on some systems simply because it is difficult to know what could be the actual catchment behaviour. For example, in some climate change studies, some models show clear trends whereas others don't, while it is difficult to know which one is right!

We thank the AR3 for his encouraging statement "I find this approach very interesting." We think that he is right in saying that expert judgement may be required to define what one could expect from a model's behaviour on synthetic data. We will add this as the following epigrammatic comment in page 8282, line 18: "This is however a matter of expert judgment, which may be difficult to implement in complex hydrosystems and is certainly not applicable for situations involving non-stationarities in model inputs."

16. Page 8282, section 4.4: It could be said in a few words how model warm-up was made to avoid errors due to unknown initial conditions.

No explicit warm-up was taken into account; thus initial conditions were empirically assigned. This was dictated by the fact that we wanted to exploit input data to the maximum possible extent for purposes of calibration and validation. In particular, for the surface model, initial soil moisture depth was set to zero for the all basin partitions (i.e., combinations of sub-basins and HRUs), given that simulation starts at the end of dry period (October). For the groundwater model, the cell levels at the beginning of simulation were estimated on the basis of topography, spring elevations and average piezometric information for the plain (karst) aquifer and using arbitrary yet realistic values for the peripheral cells, which are fed by the percolation of mountain areas. Preliminary trials were necessary to establish steady-state conditions during the entire control period (1984-1994). For both strategies, we applied similar initial conditions.

17. Page 8283: I found sub-section 5.1 not well placed in the Results section. As it explains methodological aspects, it maybe better placed at the end of section 4.

The sub-section 5.1 will be moved to Section 4 as sub-section 4.5.

18. Page 8284, section 5.2: The title of the section should be modified as "... performance in calibration and validation" (see line 19).

We will consider making this correction.

19. Page 8285, line 18: Could the authors add a few words to explain why this trend is unlikely?

Logically, if such trend appears, its cause should be identified. In the absence of trend in rainfall, the evapotranspiration, and in the land uses (mainly those related to crops), and following a “conservative” abstraction policy from boreholes to satisfy the actual irrigation needs (in contrast to the intensive use of groundwater to fulfil both agricultural and water supply demands), one naturally expects a stationary output.

20. Page 8286, line 4: Change to "through"

This will be corrected in the revised manuscript.

21. Page 8286, end of section 5: I think the author should add a discussion subsection, to replace the results of their experiment in the context of other modelling studies. To which extent does it corroborate/contradict previous results? The authors could also discuss to which extent their results are general or not. Do they think similar conclusions would be drawn on other case studies and/or using different models representative of modelling frameworks A and B? Their case study is only an illustration and

cannot be considered as a demonstration of the superiority of modelling framework B in general. For example, if the authors had chosen a less simple hydrological model and/or parameter estimation strategy in modelling approach A, would the conclusions have been similar? For example, a lot of work was done over the past years on the calibration of physically-based distributed models. Could this help improving strategy A?

We believe that the results are general, at least from a qualitative viewpoint, i.e., in poorly gauged modified hydrosystems the top-down/holistic approach (TD-H) is expected to perform better than the bottom-up/monomeric (BU-M) approach. Differences are however expected to depend on the kind and degree of the hydrosystem modification, the degree of data scarcity, and the kind and degree of the monomeric character of the approach that is followed. This is the reason why our research simply illustrates the kind of model deterioration for the selected two modelling frameworks. Thus, our research results cannot be considered general from a quantitative viewpoint. In regard to physically-based distributed models, we think that these change category of approach when they are calibrated: from their starting category, which is the bottom-up one, they become useful tools of the top-down approach. High level of integration of such models into a unique modelling framework certainly allows for a top-down/holistic approach to be implemented. We think that this is effectively a promising route to tackle the problem of modelling modified hydrosystems.

22. Pages 8295-8296, Tables 2 and 3: For the third spring, instead of not making validation tests, it could be preferred to split the available record in two parts to make calibration and validation (even if records are short). This would give more insights than solely having a calibration result.

There is significant uncertainty associated with those springs, whose runoff is poorly correlated with precipitation and the observed hydrological responses; this should result from the inherent complexity of local groundwater mechanisms, which cannot be represented by the simplified modelling framework, based on a linear (i.e. Darcian)

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scheme. Also, data errors enhance the uncertainty (unfortunately, we do not have access to raw measurements and only published discharge values were available). In addition, the contribution of these springs' runoff to the total runoff is rather small, if compared to other karst springs (Melas, Mavroneri and other springs not mentioned in the specific analysis). Given the too limited size of the sample (i.e., six years), we believe that further focusing on those springs would not provide further insight, unless we adopt a much more detailed schematization and parameterization; but this would be incoherent to our modelling philosophy, which emphasizes on the holistic representation and the water management problem, and not on a specific area or process, especially if the later is of limited practical interest. We remind that, regarding the groundwater system, the great attention is paid to the representation of Mavroneri springs, in the neighbouring of which withdrawals are made with the purpose of providing drinking water to Athens.

23. Pages 899-8303, Figures 2 to 5: A scale should be added in each figure. The meaning of colours should be explained (e.g. altitude in Fig. 2).

The scale, the North direction sign and a legend indicating the correspondence of colours to various categories will be added.

24. Page 8299, Figure 2: Maybe a small location map of the catchment within Greece could be added in one corner of the figure.

We will add such a small map showing the basin location within Greece.

25. Page 8300, Figure 3: The limits of the watershed could be added on the graph to better see the differences with the geological boundaries.

The surface water divide will be superimposed on this map in the revised manuscript.

[List of references]

All references proposed will be added in the revised manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 8265, 2010.

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