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Hydrology and Earth System Sciences Discussions

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 #1

REPLY TO GENERAL COMMENTS:

1. We thank the anonymous referee 1 (hereinafter referred to as the AR1 or the pronoun "he", as representing the common gender and not as a guess for AR1's gender) for his comment which characterizes the paper as "shaping up to be an interesting study." 2. We regret to have disappointed the AR1, as seen in his phrase "However I was disappointed because the comparisons which were done were too limited to allow any significant conclusions to be made". However, we wish to clarify that the aim of

the paper was not to exhaustively address every possible combination of approaches (bottom-up/top-down and monomeric/holistic). In fact, the aim is much narrower than that: 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." Within the scope of the BU-M and the top-down-holistic (TD-H) approaches we further limit the scope of 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. We agree that this should be said earlier and, in any case, after the options are defined. We will include such amendment in the revised text. 3. The AR1's general statement "the distinction between the two modelling strategies was not carried forward to the case study" is not accurate since we start with posing the general problem of using BU-M approaches, then we narrow our scope down to illustrating the effects of a BU-M which is extensively used in practice. For such illustration we used an antagonistic approach which is TD-H. At a second level we defined specific strategies which implement such approaches. At a third level, strategies are represented by specific modelling frameworks using existing computer packages. We, thus, believe that the distinction between the two modelling strategies (second level of categorisation) was effectively carried forward to the case study with the purpose of a simple illustration as said above and a critical discussion on the two strategies. Of course, treating the subject in all its dimensions including multiple approaches, multiple strategies within each one approach, multiple modelling frameworks within each strategy and exhaustive testing of all key modelling options of Table 1 is a formidable task. We think that our research offers some contribution to paving the way towards such kind of research. 4. We also regret to see the AR1's statement "No useful conclusions were made: the paper amounts to an interesting 4-page discussion, without significant findings or demonstrations", which we find rather too critical. We believe that conclusions are effectively drawn which correspond to the limited aims of the paper as the latter are explained above. More specifically we believe that: (1) we quantified the deterioration of model performance in cases that no attention is paid to all components of a modified hydrosystem; to our view this finding cannot be considered as being absolutely insignificant; no such kind of comparison is found in literature, which seems to ignore what is effectively applied in practice; yet, in practice one can frequently come across model misuse cases such as the one we tested; (2) sources of error were identified although their individual contribution to the overall prediction error was not quantified; (3) the four-page introductory material which the AR1 finds interesting did not intend to simply express our opinion but to introduce readers to what we have effectively done; we believe that it responds to such a requirement, of course, with the above-expressed reserve that the paper's goal should be narrowed down earlier in the text through revising the introduction. 5. The AR1's criticism "It is as though the authors ran out of time to do a reasonable case study, and just used whatever they had ready, although unsuitable to support their ideas" is to our view extremely and unjustifiably severe since: (1) the study represents years of work (as indicated in the cover letter), whereas the phrase that we "ran out of time" may create the impression that we made it in a cursory manner; (2) the case study is very rich in that it covers a broad spectrum of features related to modified basins as these are described in subsection 4.1; on the modelling side now, we found it reasonable to select a modelling strategy (strategy A) which is typical of model misuse in engineering practice; on the other hand, selecting strategy B was effectively dictated by our experience in constructing the model suite HYDROGEIOS in the last few years; (3) neither framework was ready before writing this paper; both models rerun using the same forcing inputs, while HYDROGEIOS has undergone further improvement in its code (e.g., to allow running in stochastic simulation mode) in comparison to the version presented by Efstratiadis et al. (2008); in addition, improved approaches were followed regarding key aspects of the case study, as explained in page 8280, lines 14-17: "We note that both the schematization and the parameterization differ from those reported by Efstra-

tiadis et al. (2008), in an attempt to significantly reduce the number of groundwater parameters"; thus all results are new.

REPLY TO DETAILED COMMENTS:

p8271, 1. The validation under future forcings is not convincing. If we discount models because they do not concur with prior beliefs about the future, much of the value of the model is lost.

The AR1 is right about the impression given by our text. In the revised text we will say that this validation "provides a supplementary verification of the credibility of the model calibration and highlights hidden artefacts." Validation on future forcing is completely different from that on historical data; its advantages are listed in material from line 25 of page 8270 to line 3 of page 8271.

Strategy A is supposed to be bottom up (BU) modelling, while Strategy B is top-down (TD) modelling (p8269-8270). But this gets confused in the case study, for example a simple empirical infiltration model is used for strategy A; while more process based methods (e.g. Penman-Monteith) are used for Strategy B. In fact, as is typical in hydrologic modelling, both strategies are a combination of TD and BU modelling.

We would fully agree with the AR1 if the aim of the paper was as general as to test the BU versus the TD approach; but here we test the BU-M approach versus the TD-H one; the monomeric character of strategy A lies precisely in using a detailed hydraulic methodology for groundwater modelling, which is in full contrast to the simplistic (if not naïve) lumped infiltration model used to represent the percolation mechanisms while enhancing, at the same time, the strategy's monomeric character; on the other hand, the method for calculating potential evapotranspiration (i.e., Penman-Monteith) refers not to the models themselves but on their inputs; the latter were the same for both strategies to ensure the fairness of comparison; evidently, since the Penman-Monteith method is by far the most accurate for the estimation of PET, there is no reason to use other semi-empirical models; finally, the method is applied at the sub-basin scale and not at the micro-scale, thus avoiding mixture of BU-TD strategies within a common framework.

The case study does not adequately reflect the ideas put forward in the introduction.

We think that the reply to general comment 2 above responds to this question also.

p8273, line 1-3. I could not understand what this means.

We agree with the AR1 that this material is confusing; this will be clarified in the revised manuscript.

line 5. It is not clear how it "...precludes automatic calibration" ... maybe the authors mean something more like "...introduces uncertainty and subjectivity into automatic calibration."

We agree with the AR1. This will be better explained in the text by saying that "... the above problems introduce high uncertainty in parameters obtained through automatic calibration or even make calibration impossible, since models with unknown parameters have to run individually."

line 10 "which copes with the problem" It may help address the problems, but how can the authors claim it "copes" with the problems.

We agree with the AR1: We will amend this into "An effort is made towards coping with this problem in some manner in Strategy B."

p8274, 8-9. "schematization and parameterization are disconnected" By using lumped parameter values over HRUs and regions, this is done in both strategies A and B.

We agree that both strategies use HRU-type (or zonation-type) parameterization; their difference lies in the fact that in strategy B this is true for both the surface and groundwater processes while in strategy A only groundwater modelling is benefited from such type of parameterization.

"thus ensuring that models are by construction parsimonious" "ensuring" is the wrong word. In the case study, was Strategy B (with 52 parameters) more or less parsimonious that Strategy A?

The two strategies use numbers of parameters which are of the same order and rather close to one another; so, neither strategy is clearly more parsimonious than the other; yet, in strategy B, the parameters to optimize refer to the whole hydrosystem components (river network, HRUs, groundwater cells, springs); from this point-of-view, it is more parsimonious, since it achieves a much more extended (and thus holistic) representation of the system processes, using a similar number of control variables to a modelling strategy focused on a single part of the system (i.e. the karst aquifer). We will stress this point in the revised manuscript.

p8279, line 19. This is confusing: previously the authors said that a man-made system is inherent to this strategy (p8269) and in the conclusions they say it included a model of "water management processes" (p8287).

We agree with the AR1; this will be said differently.

p8284, line 15. But this is supposed to be a BU approach; and MODFLOW does include a channel component and stream-aquifer interactions were modelled? (line 18, p8279).

The river stage for months from October to April is estimated based on the rainfall using a multiple regression formula. For the rest months this is considered equal to the river bed elevation or, else, water depth is zero. This is a BU-M approach which, unfortunately, includes scale incompatibilities due to its monomeric character; in this case the BU character of approach is restricted to the groundwater model, while other components are simplistic and thus very far from the BU approach.

p8285, lines 18 & 22. This seems contradictory.

The phrase "Although one could expect that under an extensive pumping a systematic negative trend could be possible, it is unlikely that such trend is encountered under the actual abstraction policy." will be amended to avoid confusion. An explanation is given in the response to the following comment.

p8286, line 11. But previously it was stated that the model parameters in strategy A were optimised to levels (p8281, 10) .. confusing.

The AR1 is right. What happened is that, in fact, manual calibration through visual inspection used in the BU-M approach yielded model parameters that erroneously introduced a mild trend in levels; this is hardly noticeable in the six years of the calibration period. Using synthetic data helped identifying this trend. We will clarify this in the revised text.

p8286, 26. "in general" How can the authors claim a "general" conclusion on the basis of their case study? Their methods (e.g. ignoring internal variable dynamics (groundwater levels) and assuming demand is met by abstractions) are far from general practice.

We will amend that in the revised manuscript. Yet, from our experience, the lack of groundwater and real abstraction data is not a rare problem in the everyday engineering practice.

p8287, 22 Usually it's the other way round – the more process based groundwater models will exploit internal variables (e.g. groundwater levels) while the more BU methods will only use output variables. The reality of the case study does not match the ideas proposed in the introduction.

Actually no internal variables were used in the form of hard information, i.e., in the form of exact values of a time series. Only soft information was used in strategy B: this is related to coarse-scale spatial trends characterizing the hydraulic conditions of the aquifer as a whole. Such trends cannot be easily taken into account in detailed models. Contrariwise, the statistics of the outputs of TD models, which examine the processes

at large scale, can be compared directly against macroscopic data thus enabling the incorporation of soft information into the calibration procedure.

PRESENTATION: p8272. From here onwards, the quality of the English deteriorates. There are too many errors to cover here.

We thank the AR1 and we will try to improve the English in a revision of the paper. .

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