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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.

I. Nalbantis et al.

nalbant@central.ntua.gr

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Response to Anonymous Referee #4

General comments

We warmly thank the anonymous reviewer #4 (hereinafter referred to as the AR4 and through the pronouns "he" or "his" without reference to specific gender) for the following reasons: (1) he recognised scientific value in our work; (2) he traced and stressed our exact intentions regarding our contribution to hydrological modelling; (3) he found that the title, the abstract and the paper's structure are adequate; (4) he expressed a favourable opinion on the kind of research presented in our paper, a fact that encour-

ages us in our further research.

Specific comments

Page 8267, line 6. "... processes at large scales (in the order of at least a few km2)" – the term large scale has many meanings in hydrology, wouldn't catchment scale be more appropriate?

The term "scale" here means spatial scale in general and does not necessarily refer to a catchment; it may refer to a cell of a grid that is used in fully distributed models for surface hydrological processes. Adding the qualifier "large" of course leads to the catchment scale. We believe that saying "large spatial scales, e.g., a whole catchment" provides the necessary clarification at this point of the text.

Page 8270, line 19. The authors may wish to add citations to such comparative studies.

Here we just gave a recent example (Smith et al., 2004) but we will add some more references from earlier studies of this kind.

Page 8271 – Section 2. The authors may think about putting a description of the organization of the paper before Section 2. When reading it first I was not sure how (or even if) the description of the modelling options (especially of the acronyms) relates to the modelling exercise (in fact only after looking at Table 1 it became quite clear to me). Adding a reference to Table 1 in Section 2 with respect to the acronyms could help.

Regarding the organisation of the paper we will add this information as the last paragraph of the Introduction. A reference to Table 1 will be added on page 8271, line 23 by adding "Table 1 summarises the key modelling options through linking them with each one of the modelling frameworks that is employed in our tests."

Page 8275 Section 3. I would recommend considering to introduce the Case study before this section and to include the overview of the alternative modelling frameworks into it as a subsection on methods before Subsection 4.2.

We agree to move the subsection 4.1 (“The study area”) into section 3 as the new subsection 3.1; thus the subsections on modelling frameworks will become subsections 3.2 and 3.3. In this case, separating material of sections 3 and 4 is of no use, so we will consider merging sections 3 and 4.

Page 8279, Section 4.2, lines 6 to 19. I would welcome a few words on how physically justified (or realistic for the modelling exercise) were the assumptions used for the model building (wasn't the model maybe too handicapped by the simplifications, weren't there other more realistic but still monomeric options available, etc.?).

We agree with the AR4 that modelling of some parts of the hydrosystem is simplistic to a point that the whole modelling exercise is a priori handicapped. We however kept these models at the simplistic level to represent cases of hydrological modelling we have seen many times in engineering practice. Undoubtedly, this is a model misuse case. This kind of model use is described from the Introduction section already (p. 8269, lines 3 – 12).

Page 8279, line 23. I would recommend adding an explanatory comment on “wrongly” – what does it mean for the modelling results, weren't there other options, etc.?

The explanations is given on page 8280, line 23 but we will add here “(wrongly, since abstractions are made also from the river)” to clarify.

Page 8280, lines 10 to 17. Since the schematization in the paper differs from the cited one, I would recommend adding some text and numbers (in order to describe the model experiments and calculations sufficiently complete and precise to allow their potential reproduction).

Regarding reproducibility, in this paper we preferred to avoid detailed description of models and compare the current and the previous version of HYDROGEIOS because this would most probably disorientate the reader from the main objective of our study, the comparison between modelling strategies. Reproducibility of results is, however,

possible, if one refers to existing literature (Efstratiadis et al., 2008 for the previous version and Efstratiadis et al., 2009 which is the user's manual for the latest version). Moreover, the input data required for the application is available to interested researchers.

Page 8282, lines 17 to 22. For the same reason as above adding a Table summarizing the results of the stochastic model would contribute to the information content of the paper.

To summarise the results of the stochastic model we will need one or more numerical criteria that measure the likelihood of predicted flows. For this, we will consider devising such a measure (e.g., the slope of a linear trend) and depicting its values for each combination of spring and scenario of abstractions for water supply.

Page 8283, lines 18 to 26. This information is also relevant to properly understand the model building procedure, therefore I suggest moving it forward (before Subsection 4.3). I would especially like to see Table 1 referenced there, since it nicely overviews the whole modelling strategies.

By merging sections 3 and 4 (as this results from a previous suggestion of the AR4) and putting this subsection in the new section 3, the latter becomes really a huge section for the dimensions of the whole manuscript. We will however make an effort to keep the sequencing of material according to the reviewer's suggestion.

Page 8284 lines 7 to 10. I was not quite clear to me, how one can compare long term behaviour, when comparison of actual values is meaningless - please add some explanation.

This is related to the likelihood of the predicted hydrographs as already said above. We agree that comparison of actual values is meaningless. If we introduce stationary signals as inputs to any model it is natural to expect that the output will be stationary. We recall that in our response to reviewer #1 we were saying “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." We believe that the above clarifies the reasons for using synthetically generated data.

Page 8286, lines 8 – 22. It is not quite clear, if the calibration exercise was revisited for Fig 11., or is it just an evaluating statement?

The calibration procedure was not actually revisited and will be further clarify this in the revised text.

Lines 24 – 26. For me the statement is too general, since the particular modelling problem is very (site) specific.

We agree that our case study does not allow such a categorical statement. The phrase will be changed into "... may be inefficient." This is less categorical and is based on our isolated case study which just gives an indication of the expected loss of model efficiency.

References

Efstratiadis, A., Rozos, E., and Koukouvinos, A.: Hydrogeios: Hydrological and hydrogeological simulation model - Documentation report, 139 pages, Department of Water Resources and Environmental Engineering – National Technical University of Athens, 2009.

Efstratiadis, A., Nalbantis, I., Koukouvinos, A., Rozos, E., and Koutsoyiannis D.: HYDROGEIOS: A semi-distributed GIS-based hydrological model for modified river basins, Hydrol. Earth Sys. Sci., 12, 989–1006, 2008.

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