

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

Anonymous Referee #1

Received and published: 31 October 2010

GENERAL COMMENTS: I enjoyed the first four pages of this paper: good ideas, well-written and shaping up to be an interesting study. However I was disappointed because the comparisons which were done were too limited to allow any significant conclusions to be made; and the distinction between the two modelling strategies was not carried forward to the case study. No useful conclusions were made: the paper amounts to an interesting 4-page discussion, without significant findings or demonstrations. 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.

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

C3264

future, much of the value of the model is lost.

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 TB and BU modelling. The case study does not adequately reflect the ideas put forward in the introduction.

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

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”

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

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. “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 than Strategy A?

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).

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).

p8285, lines 18 & 22. This seems contradictory.

C3265

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

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.

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.

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

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