

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

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Nalbantis et al. (2010) compared two hydrological modelling strategies: a mainly bottom-up approach (monomeric), based on output information, and a top-down approach (holistic), based on generalized information. Thus, the paper does address relevant scientific questions within the scope of HESS. I have found the introductory part of this manuscript, which deals with two alternative philosophies in hydrology, appealing. However, I have been dissatisfied by the second part of the manuscript (see below, specific comments). Hence, I recommend a substantial revision of the text, which does not necessarily require additional numerical experiments. The next section reports a number of specific comments that should be addressed.

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SPECIFIC COMMENTS

1) Reading title, abstract and introduction, the reader gets considerable expectations that are not met at the end. The numerical study carried out by the authors is too much specific and does not really enable the authors to reach substantial conclusions (see also point 6). I think that this study should not be presented as a sort of "panacea" to overcome the scientific debate between diverse modelling philosophies. More realistically, I propose to reformulate this paper as a critical discussion of two alternative modelling strategies. The specific case study would therefore act as an application example that facilitates this critical discussion and not as the (unavoidably too weak) corroboration that a specific modelling strategy is the most appropriate in hydrology. To this end, the text of the manuscript and, in particular, title, abstract and the introductory section should be revised.

3) The authors use the term "modified" both in the title (modified hydrosystems) and in the first line of the abstract (modified basins). I think that the term "human-modified" (as in page 8269, line 13) would be more appropriate and clearer to the reader.

4) Related to the previous point, I would recommend to refer to the fact that, nowadays, especially in western countries, natural (or pristine) river basins no longer exist (e.g. Sanderson et al., 2002; Wagener et al., 2010; Figure 1). Thus, in our time, dealing with hydrological issues often implies to deal with man-made systems.

5) At the end of Section 2.2 the authors state that "Strategy B adopts model integration, which copes with the problem". This rather strong statement should either be better supported or removed.

6) As mentioned above, the numerical study carried out by the authors is too much specific and does not enable the authors to get substantial conclusions. In fact, the last section of the manuscript (conclusions) refers more to the scientific literature (Savenije, 2010; Kirchner, 2006; Klemes, 1986; Andreassian et al., 2009; Koutsoyiannis et al., 2007; Koutsoyiannis, 2010; Seibert, 2003) than to the results obtained in the case

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study (as one would expect).

ADDITION REFERENCES

Sanderson, E. W., M. Jaiteh, M. A. Levy, K. H. Redford, A. V. Wannebo, and G. Woolmer (2002), The human footprint and the last of the wild, *BioScience*, 52, 891–904.

Wagener, T., M. Sivapalan, P. A. Troch, B. L. McGlynn, C. J. Harman, H. V. Gupta, P. Kumar, P. S. C. Rao, N. B. Basu, and J. S. Wilson (2010), The future of hydrology: An evolving science for a changing world, *Water Resour. Res.*, 46, W05301.

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