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Hydrology and Earth System Sciences Discussions

Interactive comment on "A new approach to model the variability of karstic recharge" *by* A. Hartmann et al.

Anonymous Referee #2

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I thank the authors for their detailed reply to my comments. I think they convincingly answered to most of my queries and the proposed modifications should improve the clarity of the text.

However I would like to make two additional comments on their reply:

1. Comparison with already existing approaches:

The authors wrote: "Commonly, new model approaches have been introduced without comparison to alternatives: Perrin et al. (2003) for the GR4J model or Lindström et al. (1997) for the HBV model", as one argument for not introducing a comparison to a reference model. I totally disagree with this argument.

First, I think that if other authors introduced new models or approaches without compar-

ing to existing ones, this is a methodological mistake, which contributes to the plethora of existing models that are not sufficiently tested and validated. This makes it very difficult for end-users (and even researchers) to identify which models are appropriate to specific objectives and conditions. So this bad example should not be followed.

Secondly, it is wrong to give the two citations "Perrin et al. (2003)" and "Lindström et al. (1997)" as examples of publications introducing new models without comparing to existing ones. This is exactly the contrary! Perrin et al. (2003) compared the performance of their new model with several existing ones (incl. a previous version of the model) (see section 3 of their article). Lindström et al. (1997) compared the performance of the new HBV model with the former model version (see Table 1 of their article).

So I think this argument does not hold and the authors should change it in their discussion.

2. Necessity of a split-sample test:

I could not find arguments in the article by Kuczera and Parent (1998) showing "that a Monte Carlo Markov Chain sampling is even more rigorous than the spilt-sample test." What these authors showed is that a more rigorous application of the split-sample test should account for parameter uncertainty, which should help better diagnosing model deficiencies. These authors did not at all advise not to do the split-sample test.

So I did not find their reply on this point really convincing. I think that evaluating the transposability in time of environmental models is a basic requirement that should help evaluating their predictive capacity. Without that, the usefulness of a model for practical applications is often very limited. So I still encourage the authors to introduce split-sample (calibration-validation) tests in their study.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 2443, 2012.