Hydrol. Earth Syst. Sci. Discuss., 9, C724–C727, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C724/2012/

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

Received and published: 6 April 2012

General comments:

The article presents a new model for karstic systems, with emphasis on how to account for the spatial variability of processes. The model is applied to a karstic cave in Israel, where detailed measurements are available.

The article is generally clear and easy to follow. It makes an interesting contribution to the difficult question of modelling karstic systems.

However, I have three major concerns on this article:

1. The article proposes a new model for karstic systems, although the literature review shows that other approaches already exist. The author should better show the added value of their new model over existing ones, e.g. by testing another existing (bench-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

mark) model and showing to which extent their approach performs better or provides a more realistic representation of processes. Without this comparison, it is difficult to judge the real value of this new model. This would better justify the proposal of a new modelling approach.

2. The results presented in the article are only obtained in calibration. This provides a biased evaluation of model performance. The author should perform a split-sample test (Klemeš, 1986) by dividing their data set into calibration and validation sub-periods (ideally, calibration should be performed on sub-period 1 and validation on sub-period 2, and vice-versa). The analysis of validation results would give a better indication of the possible implications of model overparameterization on model robustness.

3. There is no evidence from what is shown in the article that the proposed model is general and can be actually applied outside the case study presented here. Although this is very shortly discussed in the conclusion, I think a better discussion should be provided on this issue, considering what is known from the literature on past applications of model on karstic system, the peculiarities of each karstic system and the problem of data availability (the approach is applied on a well-instrumented site, to which extent the approach can applied and validated on other less instrumented sites?)

These aspects are crucial when proposing a new model. I think the authors should better account for these aspects before their article can be considered for possible publication in HESS. I advise major revision. I give below further comments that should be accounted for by the authors when revising their manuscript.

Detailed comments

p. 2447: Further comments could be given on Fig. 2 which is hardly analyzed. What does it show? What can be concluded from these graphs? The periods of data availability should be more clearly mentioned. It should be clarified what are the "seasonal" data and "experiment" data.

HESSD

9, C724–C727, 2012

Section 3: Please indicate the units of the variables used.

p. 2448 (and later in the text, especially in the conclusion): To avoid confusion, the authors should more explicitly say which variability (spatial or temporal) they consider.

p. 2449: Symbol "n" is used for the number of drips as well as for the porosity. To avoid confusion, please use two different symbols.

Eq. (10): Should not it be Eacti(t) indicating that it is the evapotranspiration from reservoir i?

p. 2452, line 4: write "Qin,surf,i+1"

p. 2452: Can Vold,max and Cold be considered as initial conditions?

p. 2453: Gupta et al. (2009) showed that the Nash-Sutcliffe efficiency (NSE) has several defaults and that a more balanced criterion (called KGE) should be used instead. What would be the implications of using the KGE objective function instead of NSE on the results presented here?

p. 2453: I don't think that the drop in NSQ,exp value (from 0.96 to 0.85) can be considered small.

p. 2454, lines 6-8: What does it mean for these parameters?

p. 2454, line 24: The authors could test the model by simply fixing these parameters. What would it change in terms of model efficiency or model robustness (if the model is tested in a split-sample test)?

p. 2455, line 4: write "More rapidly"

p. 2455, line 15: write "flow is non-significant" Section 5.1 partly repeats what was already said in the previous section and therefore could be shortened or removed.

p. 2458, second paragraph: This probably indicates that the parameters try to compensate for other errors (e.g. errors in the model structure). The authors could make

HESSD

9, C724–C727, 2012

Interactive Comment

a test by removing the upper bound and try to interpret the obtained values mean in terms of potential sources of other errors in the model.

p. 2459: As said in my major comments, I think the few lines of discussion on the transposability of the model to other case studies is not convincing and should be strengthened.

p. 2467: Indicate the source of the graphs if they originate from a past publication.

p. 2470: Which population of parameters is used to draw the distributions?

Cited references:

Gupta, H.V., Kling, H., Yilmaz, K.K., Martinez, G.F. (2009): Decomposition of the mean squared error and NSE performance criteria: Implications for improving hydrological modelling. Journal of Hydrology, 377(1-2): 80-91.

Klemeš, V. (1986): Operational testing of hydrological simulation models. Hydrological Sciences Journal, 31(1): 13-24.

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