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Hydrology and Earth System Sciences Discussions

Interactive comment on "Predictability, stationarity, and classification of hydraulic responses to recharge in two karst aquifers" *by* A. J. Long and B. J. Mahler

Anonymous Referee #2

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The paper presents an interesting study on how groundwater levels and flow rates in karst aquifers respond to recharge. The authors bring together data from various sites. They simulate observed responses with a linear-system convolution model combined with a nonlinear moisture accounting model, and then try to classify hydraulic response at the various sites based on the calibrated model parameters at each site. Convolution modeling of karst aquifers has been done before; the paper contributes by accounting for nonstationarity in the system response, and by classifying responses from different sites using principal component and cluster analysis.

The paper is well written and the content fits HESS. Below I list several comments, more or less chronologically, that deserve attention before publication.

- Overall message: I think abstract and conclusions should be improved to better align with the title of the paper, i.e. explicitly report and summarize results on:

(1) predictability: Nash values or root-mean-square errors; abstract, line 10-11: it's good to know that performance in validation was comparable to that in calibration. But how well or how badly did the models do? Some quantitative info on goodness-of-fit should be added.

(2) stationarity: 38% of sites are stationary (no difference in dry/wet IRFs); a conclusion that is now buried somewhere at the end of the paper.

(3) classification: abstract line 18 suggests that response behavior differs by site type, can you be more specific; this also relates to comment on section 3.5 (physical interpretation of the classification results), see below.

- p9588, line 1-2: how did you determine recharge areas for the different sites?

- p9588, line 9; remove "temperature"?

- section 3.2: several sites included recharge to the aquifer from local streams in addition to direct precipitation. Description of how this was modeled should be added to the recharge modeling section (2.1) which now only deals with direct precipitation.

- p9590, line 16: note that Nash values can also be negative.

- p9590, line 22-25: this does not make sense to me. By using the entire observation period (wet plus dry) in the denominator but only dry or wet period in the numerator you artificially inflate the Nash values.

- p9591, line 25: the Nash value for the validation period is likely a too optimistic estimate of future Nash/error values because model selection was performed on the validation period (which then basically acts as a "calibration" period for picking the right

HESSD

9, C4922–C4924, 2012

Interactive Comment

model structure). A better estimate of future errors can be made by testing the final model structure on a third independent data period.

- Instead of Nash, why not use RMSE (root mean square error), which provides a more direct measure of modeling errors?

- section 3.5: discussion of the classification results would benefit from making a link with physical/hydrological characteristics of the sites; in other words, do the results confirm physical knowledge of these sites (e.g. flow vs head site; insights from previous studies etc).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 9577, 2012.