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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

S. Birk (Referee)

steffen.birk@uni-graz.at

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General comments:

This paper addresses methods for the simulation and classification of hydraulic responses of karst aquifers to recharge. This is a scientific question of interest to the readership of HESS, in particular, to those interested in karst hydrogeology. While the simulation methods (lumped-parameter soil-moisture model calculating recharge and a convolution model representing the aquifer response to recharge) applied by the authors are not fundamentally new, the general concept combining the simulation with

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statistical evaluation of the resulting impulse response function appears to be novel. However, the interpretation of the results is focused on statistical aspects; there is almost no hydrogeological interpretation of the resulting statistical parameters and thus the conclusions reached are not very substantial. Nevertheless, I feel the paper can make an interesting contribution if the discussion of hydrogeological aspects is expanded.

Specific comments:

1) Equations 1 and 2: I think there are mistakes in these equations; in eq. 1 the higher-order terms are omitted and I wonder why in eq. 2 s is multiplied by the normalizing parameter c – as I understand s is already normalized. Please check these equations.

2) p. 9581, l. 10, "T is air temperature": Is this the daily mean temperature? Please clarify.

3) Section 2.3 Nonstationarity: This section starts with two examples that used nonstationary IRFs varying according to hydraulic head or antecedent recharge conditions, respectively. Both of these approaches intuitively make sense. In the model described in the paper, however, the precipitation record is arbitrarily separated in wet and dry periods based on the long-term mean precipitation. In my view, this is less convincing than the approach represented by the two examples cited before – firstly, one might expect a more gradual change of parameters rather than a distinct threshold; secondly, even if there is a threshold behavior it is totally unclear whether this is related to the long-term mean precipitation (and not, e.g., to a threshold in the antecedent soil moisture). Perhaps the authors can provide some justification here, but I guess the main reason for this approach is its simplicity – this seems to be indicated by the comments that the "method has advantages for aquifer classification". I suggest that the authors be clear about the reason for selecting this approach.

4) p. 9589, l. 23, "a 1-yr moving average": I am surprised that a time period of 1 year is used for averaging here, since one might expect several days (or perhaps weeks) to

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be sufficient for removing variations due to water use and wastewater discharge. If you use a 1-yr moving average does this not remove the seasonal variability from your time series?

5) My main concern is that section 3.5. is heavily focused on the statistics (which in itself is fine) but not on the hydrogeological meaning and interpretation of the statistics. Hydrogeological aspects are only briefly addressed in the conclusion section. As it is, the paper proposes a methodology for the classification of (karst) aquifers based on IRF metrics but it fails to provide a hydrogeological interpretation of these metrics. I think the paper can make a much more substantial contribution to the science of hydro(geo)logy if the discussion of hydrogeological aspects is expanded within section 3.5 or in a separate section. For instance, I would expect a more detailed discussion of the finding that differences indicated by the IRF metrics are larger within each of the two aquifers than those between the two aquifers. This is only briefly mentioned within the conclusion and there is just one sentence saying that both aquifers have well developed conduit networks. I feel this deserves more discussion (within section 3.5 or an additional section), e.g., about the nature of the hydrogeological heterogeneity within these aquifers. Also, I would like to suggest a more thorough discussion of the outcome of the cluster analysis: Looking at Fig. 5 the shape of the (wet-period) curves belonging to cluster Bs and that belonging to Cs are fairly similar even if the functional type of the IRF is different – is there any hydrogeological significance in the separation of these curves? On the other hand, the individual curves belonging to cluster As appear to be quite different, as there is one with multiple peaks while there is another one with just a single peak – are these aquifers similar from a hydrogeological point of view (if so, why and in how far are they similar?)? Another issue is that the hydraulic response considered in the paper is either hydraulic head or flow: Clearly these two are related via Darcy's law but still I would not be surprised if they behaved in a different way. For instance, observation wells in karst aquifers may be situated in the rock matrix rather than in the conduit system and if they are not well connected to conduits their behavior might be different from that of the karst springs, which are always well connected to

the conduit system. Did you observe any such differences between hydraulic head and spring flow and if so are they evident in the IRF metrics? I guess there are many more hydrogeological aspects that could (and should) be discussed within this manuscript.

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

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