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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Process-based karst modelling to relate hydrodynamic and hydrochemical characteristics to system properties" *by* A. Hartmann et al.

Anonymous Referee #2

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This manuscript describes the application of a process-based hydrological model to five karst systems. The calibrated model is used to examine the sensitivity of so-called system signatures, i.e. metrics derived from hydrological and hydrochemical data. The results are used to identify correlations between model parameters and signatures, which provides information about dominant processes and controlling parameters at the field sites and may help transfer the model to ungauged sites. This is an interesting approach, which – to my knowledge – is new and potentially significant in the field of karst hydro(geo)logy. The paper is strong in that it proposes a hydrological approach to address hydrogeological questions. At the same time, however, this appears to be

some cause of the weakness of the paper, in particular with respect to the inaccurate and sometimes confusing use of hydrogeological terms, e.g. within the discussion section, but also the description of the methods (model approach, sensitivity analysis), which in my view does not provide sufficient information at least for readers with a hydrogeological rather than a hydrological background. In addition, I feel that the English language needs to be slightly polished (but I am not a native speaker) and that there is some confusion in the description of the results from the sensitivity analysis. Another minor issue is that the attempt to correlated climatic and topographic factors to system signatures is not successful and one is tempted to say that this is not surprising given that only five sites are considered. Thus, while the idea to extend the approach towards climatic and topographic (and other factors such as geological descriptors) deserves to be discussed, its application in the given case appears to be unpromising and thus can and should be omitted. All of these concerns can be easily addressed by moderate revisions to the manuscript. Some examples illustrating the aforementioned issues are provided below:

1) Introduction – examples of distributed models applied to karst aquifers: The reference Geyer et al. (2007) is rather inappropriate here, as it describes the application of an analytical (1D) model. For the same field site Birk et al., Ground Water (2005) describe the application of a hybrid (continuum-discrete) model in a simplified way for a similar purpose; another adequate example is Hill et al., Ground Water (2010); of course, there are also many applications of standard groundwater models to karst aquifers, but evidently the intention is to mention karst-specific approaches here.

2) Section 3.3 "The karst model", including Fig. 3: I understand that the modeling approach was published elsewhere, but since the model has not been widely used so far, most readers will be unfamiliar with it and since the description is very brief and incomplete they will have to consult this other paper. In my view, this section should be extended (and perhaps accompanied by an appendix to the paper) such that the information needed to understand the subsequent application and analysis is provided.

This includes information about the model equations – the main point of the paper is to analyze the sensitivity of system signature to model parameters and thus it is essential to explain the way these parameters are used in the model (preferably by showing the equations either in the main text or in an appendix). Further, I would like to mention that I did not recognize anywhere a clear statement saying whether this is a lumped or a distributive model – from the description given in the text I understand that the model considers mean values of (lumped) parameters and their (statistical?) distributions, but Fig. 3 is designed in a way that it suggests the model is distributive (as it shows a water table and its spatial relation to the spring) – in fact, it appears like a visualization of a finite element or finite difference approach, but this is obviously not intended here. Perhaps the term "compartment" used in the appendix adds to the confusion.

3) Section 3.4: Although less confusing, this section also seems a little short. At least I would like to see more information (perhaps equations) about the total sensitivity and the first-order sensitivity, as these two are extensively used for discussing the results.

4) Section 3.5, p. 2844, l. 11-17: The wording of this sentence is misleading. I do not think that the intended meaning is that all these parameters are "detected" – I am not sure what the authors would like to say here but "detected" does not seem to make sense to me.

5) Sections 4.1, 5.2 and elsewhere in the manuscript: There is some confusion in the usage of "X is sensitive to Y": On p. 2846, l. 3-5 it is said that the signatures S_HF and RQ100 are sensitive to model parameters (groundwater dynamics) – this makes sense to me, because you varied the model parameters and observed the changes in the signatures. Later in the manuscript this is frequently turned around (model parameters are sensitive to signatures), which does not seem to be correct. Even if you want to suggest that you believe the signatures provide information about the parameters, this is not an appropriate way to do so – instead this should be explicitly done in the discussion.

6) Section 5.2: In this section (but not only here) there is frequent misuse of hydrogeologic terms or at least some unclear wording. For instance, please say just "aquifer" instead of "groundwater aquifer"; what is meant by "increased karst behavior" – please say clearly what you mean (flashy discharge behavior, fast solute transport, ...?); "the groundwater properties control the flow behavior" – groundwater properties can (and probably will) be understood in terms of water properties such as water temperature, solute concentration, etc., which is clearly not the intended meaning here – probably the intended meaning is aquifer properties; "the deeper geology" – I guess you intend to address the thickness of the aquifer or the depth to water table, but the term you use is unclear and inappropriate. Similarly, I think terming a model parameter "groundwater dynamics" is misleading (the parameter is a coefficient in one of the model equations, which unfortunately are not shown, but this term has a much broader meaning) and also I wonder if "soil depth" is synonymous with "soil storage capacity", and if so I suggest that the latter term is consistently used throughout the paper. There are a number of other, mainly minor language mistakes here and elsewhere in the manuscript that need to be corrected.

7) Section 5.2, p. 2853, l.10-11, "hydraulic conductivities" (cf. l. 4-6 on the same page): The model parameters listed in Tab. 4 do not include hydraulic conductivities, so it is unclear to me how you can infer this parameter from your model. A couple of lines earlier you refer to the recession behavior and indeed there are approaches to infer hydraulic conductivity from the recession coefficient. These approaches are usually based on analytical solutions of the groundwater flow equation (or approximations of these). Thus, one might think about transferring the parameters of your model (e.g. the "conduit storage constant") in a similar way to hydraulic conductivities but I doubt that this is correct, because I guess (since I have not seen the model equations) that your model is not based on the groundwater flow equation (is it?). Also, the text seems to suggest that the "high flows" and thus e.g. the fast, early stage of the recession can be interpreted similar to the long-term recession in terms of aquifer properties – please note that this stage of the recession is influenced by the recharge event preceding the

recession and thus analyzing it in terms of aquifer properties is not straightforward (see e.g. Birk and Hergarten, J. Hydrol., 2010).

8) Fig. 6 and text related to correlation between climatic or topographic factors and system signatures: As indicated in my general comments I think that this should be removed from the paper (which may compensate for the increase in length due to my above comments). I think this is an important topic but it needs to be addressed by a separate study. In fact the relation between climatic, topographic, geologic, or any other factor can be investigated independently from the karst model examined here. I think it is adequate to discuss that the correlation between model parameters and system signatures is just part of a more general concept and that the next step will be to derive correlations that can be used for ungauged basins. However, I feel that neither the data is sufficient to do this in the present paper nor does it help the paper, as it just makes it more complex without providing significant results.

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