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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Reducing the ambiguity of karst aquifer models by pattern matching of flow and transport on catchment scale" *by* S. Oehlmann et al.

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Dear referee,

We thank you a lot for your helpful comments and especially for the very detailed remarks in the attached pdf. In the following, we will answer your general remarks. Of the comments inside the pdf we will answer those, which include questions or which we see as arguable. We shall address also your comment/suggestions not listed here specifically.

Comments

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Interactive Discussion
Discussion Paper

1) The introduction lacks a proper overview about (karst) simulation approaches, most importantly missing recent karst studies that already used transport equations and multi-objective calibration to reduce model equifinality/ambiguity (which should be picked up in the discussion again).

Answer: We will extend the introduction a little to mention lumped-parameter approaches and to point out that they require less complex models and are therefore not only suited for multi-objective calibrations but that those have already been applied for karst systems, also including transport (Hartmann et al., 2014). The basic difference between your literature suggestions and our work is the distributed modelling approach, especially the point-to-point transport compared to the diffuse transport of environmental tracers used by Hartmann et al. (2013). Furthermore, lumped parameter approaches typically focus on the representation of system dynamics while we focus on spatial representation. We will discuss this in our introduction.

2) The assumption of steady flow in a karst system is only valid for lowflow/recession periods consequently making this analysis and its conclusion only valid for such conditions. During rainfall events, when concentrated infiltration and recharge initiate the optimum parameters may change completely. This should be pointed out more clearly in the discussion.

Answer: We agree with this point. However, the simulation of dynamic flow and transport requires a high computational effort. Furthermore, the distributive estimation of recharge dynamics is still a challenge in karst hydrogeology. In our approach we focus on the simulation of short term artificial tracer tests to perform pattern matching. Therefore, we assume steady state flow conditions and transient transport, i.e. the approach would be less applicable for breakthrough curves of tracer tests disrupted by strong recharge events. We will emphasize this point in the revised version.

3) The analysis of parameter values and the influence of their variation on the multi-objective model performance do not take in account parameter interactions. However,

parameter interactions of hydrological models, even with much lower numbers of parameters, have shown to be highly important. Varying only 1 or 2 parameters of a model as presented here could be completely be compensated by varying other model parameters simultaneously. The local type of sensitivity analysis presented her cannot consider this effect.

Answer: We agree with this point and did perform extended parameter studies in former works (e.g. Kordilla et al. 2012, Doummar et al. 2012). In this work, influence of geometrical patterns is prioritised. However, parameter interactions appear in our study as well. It can be seen from Fig. 6c that the variation of the RMSE of the hydraulic head fits due to changes in matrix hydraulic conductivity depending on the conduit geometries. For geometry 1 (blue line), there is a clear optimum while the fit for geometry 2 (black line) approaches an asymptotic value for higher conductivity values with no distinct optimum parameter. Figure 1 in this response shows this interdependency in detail.

However, parameter interaction played only a moderate role for the results because of two reasons: 1) the simulations are all for steady-state flow conditions. Due to that, a number of parameters that show high interactions during dynamic simulations (e.g. the conduit-matrix exchange coefficient, the matrix storage coefficient, the parameters of the epikarst...) are of little to no effect in our simulations. 2) The fact that the objective functions were considered together instead of considering each one separately and comparing the derived values compensates parameter interactions to a certain degree. We will discuss this in our manuscript.

4) Pointing out the realism of the parameters in subsection 5.1 makes this study still strong but I recommend either to perform a regional or global sensitivity analysis (e.g. Sobol's method) or to relax the conclusions drawn from subsection 5.2 and the respective analysis significantly.

Answer: As mentioned before, the focus of this work is pattern matching with a distributive modelling approach. An extended sensitivity analysis would prolong the already detailed manuscript strongly and not focus on the primary goal of our work. We will point out in our manuscript that the conclusion of the investigation is specific to our model area and available data (e.g. ranges of parameters from field investigations, choice of objective functions,...) and should be treated with care, if applied to other conditions/catchments.

Discussion of remarks inside the pdf attached to the referee comment:

p. 9282, l. 18:

You suggest the use of the word “equifinality” instead of “ambiguity” as it is defined by Beven (2006). We prefer to stay with “ambiguity”. Beven (2006) himself states that he only does not use the term ambiguity to emphasize that not only ambiguities between parameter sets add to the effect but also differences in model structure and processes. Since we mainly observe different parameter sets, we find the use of the word “ambiguity” more suitable.

p. 9283, l. 3-4:

Referee Comment: This is 23 years ago! There are many more recent reviews including the developments of the last two decades, as well.

Answer: We do not understand the critic on citing original literature. Teutsch and Sauter (1991) provided a first overview of distributive modelling concepts for simulation of karst hydraulics, which is still valid. Hartmann et al. (2014) give nearly the same classification in their chapter 5.

p. 9283, l. 15:

Referee Comment: In this paragraph it appears that distributed modeling approaches are the only way to model a Karst system. They are mainly applied at well studied test sites (as the Gallusquelle) are for theoretical calculations. Please provide some wider overview at least mentioning that lumped process-based approaches are applied when data is scarce (as in most cases).

Answer: We agree that lumped parameter models are useful tools for simulating spring responses and did add some literature.

p. 9283, l. 17-18:

Referee Comment: Many of the studies mentioned above used a priori estimates of the parameters rather than automatic calibration. Please mention the problem of data availability (lack of head observations, no distributed measruemnts of hydraulic parameters, geography etc) that is faced by distributed models in many cases.

Answer: We wrote: "A large number of calibration parameters is usually opposed by a relatively low number of field observations leading to several parameter combinations, which give the same fit to the observed data but sometimes very different results for prognostic simulations (Li et al., 2009)." This implies already the reviewers concern.

However, we will change the sentence to:

"A large number of calibration parameters is usually opposed by a relatively low number of field observations (hydraulic parameter field, distributed process variables) leading to several parameter combinations, which give the same fit to the observed data but sometimes very different results for prognostic simulations (Li et al., 2009)."

p. 9284, l. 4-5:

Referee Comment: For that reason inverse modeling that requires calibration is applied in many cases. It would make sense to move this part up to the paragraph describing automatic calibration and ambiguity.

and

l. 9-14:

Referee Comment: Please provide some more detail. How are multi-objective calibration and computationally expensive calculations related to the simulated variables?

Answer: We restructured the introduction slightly. However, the reviewer should note that the focus of this work is not automatic model calibration and inverse modelling and that the introduction just gives a short overview about previous work.

p. 9284, l. 22:

We added:

Hartmann, A., Wagener, T., Rimmer, A., Lange, J., Brielmann, H., and Weiler, M.: Testing the realism of model structures to identify karst system processes using water quality and quantity signatures. Water Resour. Res., 49, 3345–3358. doi:10.1002/wrcr.2022, 2013.

Dörfliger, N., Fleury, P., and Ladouche, B.: Inverse Modeling Approach to Allogenic Karst System Characterization, Ground Water, 47(3), 414-426, 2009.

p. 9287, l. 28-29:

Referee Comment: Considering the high temperal dynamics of Karst recharge and subsurface flow, how can a steady state simulation be justified? This is picked up in the discussion but some words should be added already here as well

Answer: We would like to refer you to the beginning of the chapter, where this fact was already explained (p. 9286, l. 2-5). We believe that a repetition at the end of the chapter would not enhance comprehensibility.

p.9291, l. 9:

Referee Comment: Does this mean that there is no bidirectional exchange between conduits and matrix? If conduits can also recharge the matrix, direct recharge is definitely important. The authors should point out more clearly that the assumption of steady flow is only valid during recession/low flow periods. And they should discuss that their interpretations are only valid for them.

Answer: We agree with the reviewer that recharge dynamics are of high importance and that we have to clarify our assumption. According to the reviewer we did change the sentence to:

"It is not expected that recharge dynamics exhibit significant influence during recession period on the flow field."

p. 9291, l. 19-23:

We prefer to keep the explanation that matrix contribution is neglected with the apparent conduit volume estimated by Geyer (2008) according to Ashton (1966). The calibrated conduit volume with a physically based modelling approach has to be smaller. However, we will remove the equation and shorten the paragraph to this single statement.

p. 9292, l. 15-17:

Referee Comment: Did you validate the steady state model for its transferability to this quasi transient state? Is the assumption about the insignificance of the conduit system still valid at this shorter time scale?

Answer: The steady state models for different recharge conditions are validated by the water balance in the model area. It is not completely clear to us, what you mean with the "insignificance of the conduit system". We show that the properties of the conduit system play an important role for artificial tracer transport. We expect you refer to the fact that direct recharge was neglected (p. 9291, l. 8) since you mentioned before that this is a critical assumption. An additional simulation of recharge dynamics would require detailed knowledge about temporal and spatial distribution of recharge on an event time scale, which is usually not available. Since tracer test 1 was conducted under low-flow conditions, there should be no influence of direct recharge. Tracer test 2 was conducted under higher flow with recent recharge so direct recharge might have an effect. However, we performed a simulation with the assumed maximum percentage of direct recharge of 10% (Sauter, 1992, Geyer et al., 2008). There was no significant change in the results (Fig. 2, this response), but future studies should incorporate transient flow and transport. We have added this point to the discussion.

p. 9292, l. 21-23:

Referee Comment: Please provide some more detail why these parameters and factors can be regarded as most relevant? And: What is a factor?

Answer: We have added: “to identify parameters determining the hydraulic parameter field in the model area”. We removed "factor“.

p. 9292, l. 27:

Referee Comment: Please clarify this sentence.

and

p. 9293, l. 4:

Referee Comment: The procedure could be described better and shorter by a simple flow chart.

Answer: We do not believe that a flow chart is necessary to explain the procedure since the approach is not too complex. We rewrote the first paragraph of Sect. 4 to clearly state the fitting parameters and the objective functions and thus give a better overview over the pattern matching concept we applied.

p. 9293, l. 9:

Referee Comment: please define "slope of radius increase" do you mean "rate of radius decrease"?

Answer: The "slope of radius increase“ is the increase of conduit radius (m) per unit conduit length (m). It is defined in Eq. 1 (p. 9285).

p. 9293, l. 27:

Referee Comment: Mixing up scenario description and their results is slightly confusing.

Answer: We did discuss the manuscript structure at the beginning of manuscript writing and decided to present scenarios and results in one chapter to reduce repetitions and to keep the flow during reading. Since the scenario descriptions are quite long in some cases, one might not remember the first scenario when getting to the results chapter. Therefore, we prefer to keep the current structure.

p. 9297, l. 10:

Referee Comment: Some of the conclusions about the model performance appear to be rather the subjective choice of the authors. Defining clear (quantative) rules/tresholds first and using them to evalaute the scenarios would make this study and its conclusions much stronger.

Answer: We added a clear description of the objective functions and the ranges where the fit was considered acceptable at the beginning of chapter 4 to clarify the approach. Here is a short summary:

Gallusquelle spring discharge: acceptable difference between observed and measured: 10 L/s

Tracer test velocity: judged by the difference between the measured and the simulated peak arrival times. Acceptable fit if difference was smaller than the simulation or measurement interval (tracer test 1: 2.7 h, tracer test 2: 6 h)

Hydraulic head distribution: fit measured by Root Mean Square Error (RMSE). A RMSE of <10 m was considered acceptable if the qualitative fit of the gradient distribution (Fig. 3) was adequate.

p. 9299, Eq. 15:

Referee Comment: Why exactly this equation? Is there a physical explanation for it?

Answer: The basic idea of the equation to find a reasonable increase of the maximum conduit radius to analyze the potential effect of conduit increase towards karst springs on simulation results. The given equation considers a systematic increase of conduit radii towards a karst spring. In general, an increase of conduit diameters can be expected based on the knowledge from karst genesis simulations (e.g. Liedl et al., 2003). However, the equation itself is not derived from a karst genesis simulation in the area, which would be the ideal approach. We did add this limitation to the discussion chapter as an outlook.

p. 9300, l. 17:

Referee Comment: Computational efforts should not be the criterion to judge about model realism or adequateness.

Answer: In our opinion, numerical models are always an imperfect realization of the real nature. Surly, the models should incorporate most important physical parameters and processes. However, computational efforts determine the applicability of complex approaches. For this reason we have to judge the most effective modelling approach. We will clarify that computational effort are not a criterion for the models realism but for its applicability.

p. 9306, l. 21:

Referee Comment: Wouldn't other hydraulic parameters modify the optimum solution for the transport parameters? Since solute transport is dependent on water flow this definitely the case.

Answer: The flow parameters certainly influence the optimum solution for the apparent dispersivities, but not the other way around. Therefore, our approach is in first calibrating the flow parameters for the other objective functions and then calibrating the dispersivities for the shape of the breakthrough curves. Since in this case the flow parameters are already set when the dispersivities are calibrated, ambiguities regarding the parameters for breakthrough curve shape can be avoided. We will state this more clearly in our manuscript.

References:

Ashton, K.: The analysis of flow data from karst drainage systems, Transactions of the Cave Research Group of Great Britain, 7, 161-203, 1966.

Geyer, T., Birk, S., Liedl, R., and Sauter, M.: Quantification of temporal distribution of recharge in karst systems from spring hydrographs, J. Hydrol., 348, 452 463, 2008.

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Liedl, R., Sauter, M., Hückinghaus, D., Clemens, T., and Teutsch, G.: Simulation of the development of karst aquifers using a coupled continuum pipe flow model, Water Resour. Res., 39(3), 1057, doi:10.1029/2001WR001206, 2003.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 9281, 2014.

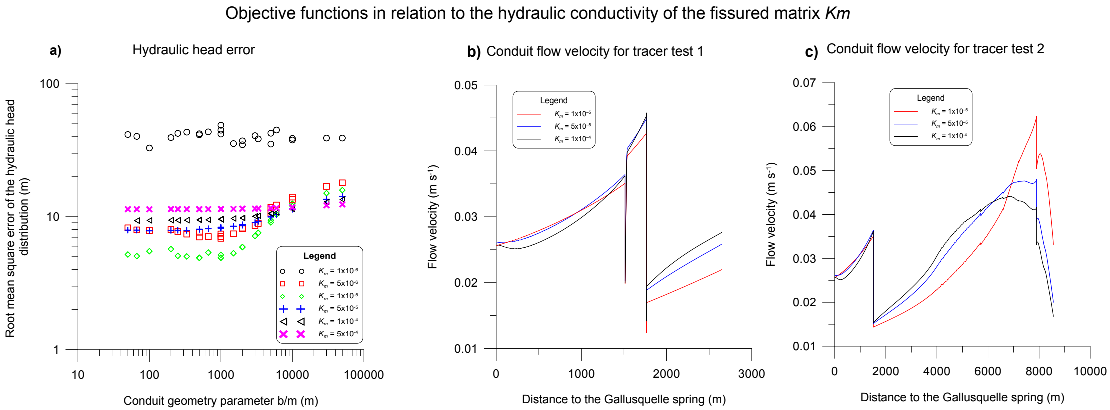

Fig. 1. Influence of the fissured matrix conductivity a) on the hydraulic head distribution in relation to the conduit geometry, b) and c) on the tracer velocities.

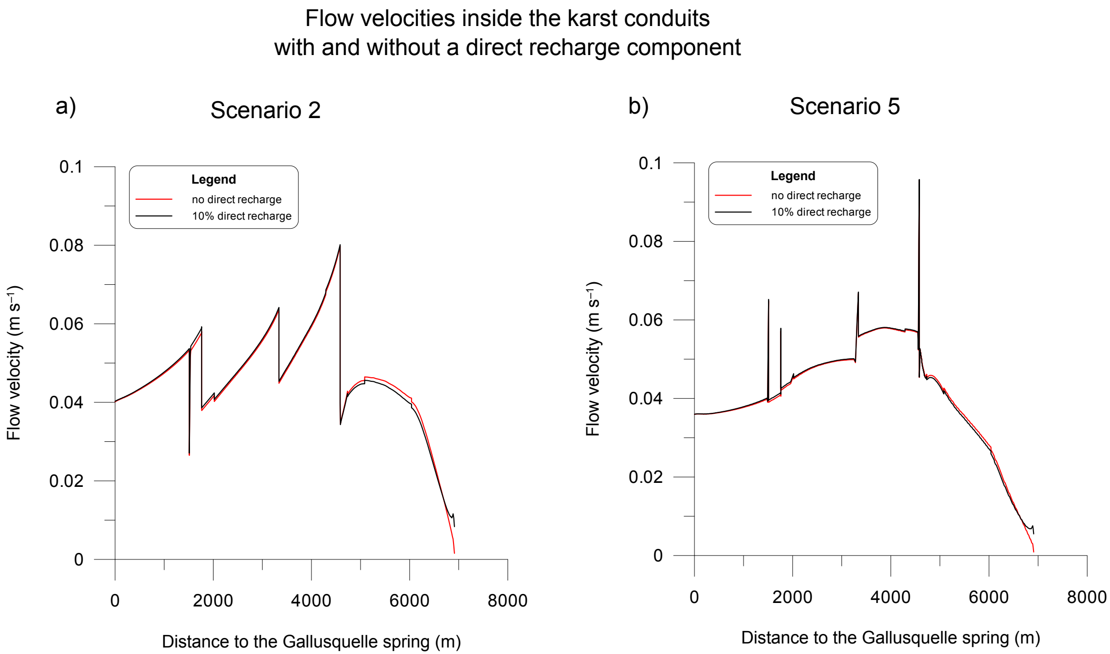

Fig. 2. Flow velocities inside the main conduit branch with and without direct recharge for the best-fit scenarios.