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Interactive comment on "Simulation of saturated and unsaturated flow in karst systems at catchment scale using a double continuum approach" *by* J. Kordilla et al.

Anonymous Referee #2

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Review of the manuscript

Simulation of saturated and unsaturated flow in karst systems at catchment scale using a double continuum approach J. Kordilla, M. Sauter, T. Reimann, and T. Geyer

General comments: The paper tackles the problem of simulating flow in a karst system under the typically highly dynamic conditions prevailing. The authors use a dual-continuum model for this, representing both the fissured as well as the conduit system of a karst aquifer by an equivalent porous medium approach. This is actually not new, but dates back to the 1980th (Teutsch, 1988), who used this type of model for fully

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saturated conditions. The authors then introduce the unsaturated flow formulation following van Genuchten parameterization, which has actually been done before by G. Kaufmann (2001), however applying it to karst genesis rather than spring discharge, however the model approach is similar. The novelty is to take this approach to a dual porosity model. As the parameterization and quantification in karst aquifers is an important and actual topic of wide applicability, the topic is suited for the journal and a wider audience. The authors describe their model area, the model used and then present a calibrated model with some sensitivity studies concerning the parameters chosen. As such, this could be a good contribution to our understanding of karst systems.

However, I have some mayor and quite a number of minor topics, which make the paper not acceptable in the present for.

I do have really a problem with applying Richards flow models with Van Genuchten models to karst systems. These models were derived for porous media, actually soils, and this is when they are valid. I agree, that the fissured and slow system of a karst aquifer can be simulated using such an approach. However, for the conduit system, I can see no justification for this. A conduit is not a porous medium, and even as an upscaled system of conduits does not behave like a porous medium, so that this approach does not work. The authors give no justification for their approach (as actually the article by Kaufmann does not also), they just mention that this approach "strictly speaking does not apply". However, to me this is not a discussion on "strictly speaking", but a very basic flaw. Actually, later in the article the authors provide themselves the problems associated with this approach, as they have to introduce minimum relative permeabilities for the conduit system, as the high value of van Genuchten alpha otherwise makes the conduit system actually impermeable (quite contrary to the obvious facts). So I do not see the point in using and applying a model, which is obviously not based on the correct physics.

The second major problem I have with the manuscript is the use of a 2D vertical model. As can be clearly seen form the field site map, the catchment area focuses laterally

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towards the spring, so that the 2D approach chosen is obviously wrong due to the large horizontal flow components perpendicular to the main axis of the model area and the 2D cross section chosen by the authors. I think that this matters, because thus the spring discharge is not correctly connected to the precipitation, which basically drives the time variant behavior of the model, and thus the parameter identification is wrong. As the authors claim to find representative parameter sets, I can not agree here. And I see no obstacle to a 3D approach here, which would provide correct flow rates in the model and at the spring. Then, the parameters would actually be physically meaningful and characteristic of the system. So to me, a 3D model approach is clearly required.

Other comments: The paper is written very imprecisely and with a lot of mistakes. This is pretty annoying, as it makes it hard to read and understand. Also, many aspects are not explained clearly, so that I do not understand what the authors actually have done. I will outline a few of these below.

It also does not give proper references to the large amount of work done on simulating karst spring responses, as well as to simulating exchange in a dual flow system approach. Here I think also the literature form karst genesis should be taken into consideration, as the basic approaches and processes are identical. Here I think the second author can provide the adequate literature references.

The method section is poorly written. Please use the correct symbol for partial derivatives in eq. 1 and 2. Eq. 5 seems wrong to me, as this should be the total water content, not porosity. This is actually important for the later scenarios, so clarification is required. In line 12 on page 1519, the indexing is wrong, as one index appears twice. In line 1 of page 1520, the authors set residual saturations to 1, which should probably just saturations.

The formulas eq 8, 9, 10 are wrong. This is not the van Genuchten parameterization ! Please give the correct formulas. Also in eq. 11 through 13, the same index nu appears for both systems and the exchange term. Please check, if this is actually the

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model used.

I do not understand the explanation to equation 24. The authors state, that the root mean square error is calculated from the difference of “the spring discharge derived by the model” and the “calibrated model value”? As far as I can see, there is measured data available, so you could compare to measured data. And maybe you mean that you compare to the spring discharge simulated using the calibrated model? Also, please state on which basis you calculate this error, i.e. daily, weekly, monthly ?

Remove Fig.1 – this is not needed. It also has a wrong legend (2)

Why is recharge added “at the bottom of the conduit continuum” (page 1524, Line 17). Please justify.

When describing your model results in section 4 for the spring discharge comparison, please use dates to refer to individual events. I am not clear about the peaks the authors refer to, as their time is not given.

Please also show the water tables during the simulation. I suggest you show water tables with time at an observation well i.e. at -8000m. This would allow a comparison.

I do not understand why there are non-vertical flow paths in the unsaturated zone of the matrix continuum. (Figure 2). Why is this? This is an unusual behaviour for such a large scale porous medium.

The comments on applying van Genuchten parameters should be moved to the introduction or model section, and put into perspective there.

In section 5, page 1527, line 23, the authors argue that a similar behavior as for K_c can be seen for theta_c. I do not agree, the discharges for theta_c look very similar in Fig. 7. Clarify! nAlso I do not understand what you really vary. Theta is the water content, so what do you really vary when you vary theta_m? Also you state in this section that K_m is not sensitive. Again I do not agree, as in Table 2 this is one of the highest values. However, because I did not understand table two at all, I may be wrong here. This is actually the only reference to the table, and it is neither explained what is shows, not is it discussed. Here explanations are required and an exemplary discussion of a few cases.

Section 5.3 just describes the Figures in Fug. 9. However, no explanation is given, why the model results becomes sensitive on some parameters only for certain parameter combinations. This is a very interesting point, and should be explained with detail and care.

The authors write of non-linear RMSE in the case of two parameters, but this is actually not true. Also for single parameter variations the results is non-linear, so this has nothing to do with varying two parameters at a time.

The authors use a very simple model to represent the spring catchment (2D, homogeneous, steady state in the saturated zone). They should discuss, how this affects the parameter identification and the parameter space investigated.

On page 1531, line 10, you state that the model could "successfully" simulate the spring response. You have to justify this statement. Please compare the modeled spring response to prior work (Sauter, Birk et al.) and state, where the improvements are. When looking at Fig. 5, it douse not look convincingly like a good fit. Please also state why you regard this as a successful fit.

In the introduction, the authors state that this manuscript is aided at providing hints for a better characterization of a karst aquifer. However, many of the parameters (even for the homogeneous model used) can not be measured. So please discuss, how this work might contribiute to characterization effeorts, and what is to be learned. The Conclusions section is rather a summary than a conclusion.

Table 1 is not complete. Please provide values for the total porosity used, as well as for theta_c. Units for alpha_c are probably wrong, as well as the indexing of the footnotes. Footnotes should be integrated into the Table heading.

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Fig. 3 gives a 3D impression of the study area. This is correct, if a 3D model is used. For a 2D model, show the geology along the cross section actually used.

In Fig. 5. Why is spring discharge given in mm/d? Units should be m^3/d, so probably this is normalized. Please explain how spring discharge is normalized.

Fig. 6. The upper part of Fig. 6 is not used – it is not referred to in the text, and it is not explained in the legend what is shown. Also, label sub-figures, so that clear referencing is possible – this will also improve readability of the text. Show saturations form 0 to 1, nnot just 0.4 to 0.9. In the Figure caption you state, that the water table height is nearly equal, but this is not shown in the Figure.

Fig. 7: Why did you chose the parameter ranges shown? Tab 1 e.g. gives different values for ranges? Please explain your choices. Also, it would be good to give the RMS of the cases shown, so that the reader gets an idea of what the RMSE means.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 1515, 2012.