

Interactive comment on “Modeling hydrological processes influenced by soil, rock and vegetation in a small karst basin of southwest China” by Z.-C. Zhang et al.

Anonymous Referee #1

Received and published: 18 February 2010

The manuscript deals with distributed modeling of a Chinese karstic system. The interest of this work is twofold (i) it gives access to the poorly known Chinese karstic systems (ii) a distributed high resolution model provides information not only on the general behavior of a hydrological system but also to the distribution its transport properties. Then the topic of the paper is fully in the domain of interest of Hess.

However, the manuscript does not provide valuable information of both point (i) and (ii) in its present form.

The authors rely on the assumption of a "typical" karst system, which to my knowledge does not exist. Every author making comparison between different karstic systems

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even a few km apart from each other has to elaborate on their different responses to rain (see for example Jemcov and Petric, J. of Hydrol, 2008 or Labat et al., J. of Hydrol., 2002). There should therefore be an extensive description of the Chenqi basin and in what extent it could be considered as typical of the 540 000 km² Yunnan-Guizhou Plateau province.

The geological and climatologic information is extremely poor. The map given in figure 2 in the absence of any cross section give the (wrong?) feeling that limestone formations lie above marlite formation. So how thick are the limestone formations? What is exactly the distribution of rain and of temperature? It is said that the rain season is in summer but the heaviest rain occurs at the end of May during the validation phase. Is there some snow in this region? What is the yearly variation of interception of rain and of energy by deciduous shrub?

The model itself seems reasonably stated, but as said by the authors, it relies on adjustment of a large number of parameters and on some reasonable, but questionable assumptions (the exponential dependence of fracture aperture with depth, the validity of soil hydraulic properties measured with Guelph method, the distribution of fractures for example). It does not provide a significantly better adjustment of flood hydrograms than lumped models published previously (see for example : Scanlon et al., 2003, in the reference list, Grasso et al., J. of Hydrol. 2003, and the large literature on karst hydrograph analysis). Then why to use such a sophisticated model? The reader might be convinced if for example the model could be applied easily to other natural examples, or if it can reproduce a number of different kind of data, or if application of this model provided an insight on detailed hydrological properties of a karstic system, which is not the case in the present version of the manuscript.

In its present form, the manuscript is just another case study which cannot be considered as presenting a broad interest for the scientific community, except for those who are working on the same natural example. However, owing to the large work on the model and on the karstic example, I would like to encourage the authors to present

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a new version revised to broaden the area of interest of the paper. I would therefore recommend a major revision of the manuscript.

I hope that the detailed comments listed below will help the authors in preparing an improved version of their manuscript.

- abstract : what is a typical karst ?
- p 563, last paragraph "rainwater is retained near the base of the epikarst leading to the formation of an aquifer". Can you prove this in your natural example? It depends probably of the permeability distribution in the upper karstified levels.
- 2.2.1 "infiltration via vertical shaft system". How is this accounted for in the model? Are there swallow holes? Where?
- end of section 2.2.3. Modeling of the fracture distribution. Did you try different kind of fracture distributions? clustered (Sisavath et al., 2004), fractal (Bour et al., JGR 2002), enlarged by dissolution (Kaufman and Braun, WRR, 2000).
- end of page 571 Pen et al, 2008 is absent from the reference list. I assume that you meant Peng et al. Then as not many European researchers can read Chinese, you should summarize the results of this paper and explain how "Average thickness of the epikarst zone is estimated based on overland flow discharges".
- p 573 last paragraph : How do you infer that large underground channels are located in the lower areas of the basin. Can you provide some proof for that?
- end of section 5.1. In a karstic system submitted to a winter / summer regime, the calibration should use at least one complete year. In any case a calibration during the rainy season can be used to model data outside the rainy season. This is one main flaw of the paper.

According to the paper, there is a threshold of 80 mm of rain for surface runoff. Could you explore this threshold with the model? Does this imply in all cases that the epikarst

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is saturated with 80 mm of rain?

- simulation of soil moisture content. It is not clear how vegetation is coupled to groundwater. I suspect that trees are able to extract water to a several meter depth. How is this coupled to groundwater flow?
 - section 5.3.2 It would be interesting to give some hints on the influence of the vegetation package of the model on discharge (similar comment as the previous one).
 - section 6 : very interesting !
 - your model should be compared with other models or other karst or other kind of observables (correlogram ?)
- Yuan, 1997; Xiao et al., 2003 are not in the reference list. Please cross check the reference list.
- Fig. 2 check the label of level curves. Where is the superficial drainage network? Where are the major underground channels ? Also provide one or several cross sections
 - Fig 3 : consider merging this figure with figure 2.
 - Fig. 9 Enlargement of some rain and/or some flood event might be interesting. Also elaborate on the wrong recession curve in the validation period.
 - Fig. 10 very interesting.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 561, 2010.

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