Paper Review:
"A physically-based distributed karst hydrological model (QMG 2 model-V1.0) for flood simulations".

This review was prepared as part of graduate program Earth & Environment (course Integrated Topics in Earth & Environment) at Wageningen University, and has been produced under supervision of dr Ryan Teuling. The review has been posted because of its potential usefulness to the authors and editor. Although it has the format of a regular review as was requested by the course, this review was not solicited by the journal, and should be seen as a regular comment. We leave it up to the author's and editor which points will be addressed.

Header

The paper describes a modelling study of the karst trough valley Qingmuguan in China. Nowadays, due to climate change more extreme hydrological events such as flooding happen. Karst areas have very complex structure and therefore it is hard to mimic such hydrological systems. Complex models are able to model such systems but have high uncertainty. Therefore, this paper proposes a simple physically-based hydrological model (QMG) to accurately predict flooding at the Qingmuguan karst area. The modelling set up of the area is explained and the main calculations are explained. Thereafter, the model parameters are calibrated and optimized using the improved PSO algorithm IPSO. The sensitivity of each parameter was analysed. Eventually, the Nash-Sutcliffe coefficient (NSC) of the model is calculated such that the model performance can be determined. The optimization is that the model performance (NSC) increases form 0.69 to 0.92. According the authors it is concluded that this model is therefore very 'accurate' and thus suitable for predicting flooding in the karst trough valley Qingmuguan.

The novel aim of the authors is to overcome the high modelling-data demand by finding a simple physical-based distributed karst hydrological model which can 'accurately' predict flooding in the karst trough valley Qingmuguan. Since karst areas are very prone to flooding due to there low water bearing capacity, it is very important to find such an 'accurate' karst hydrological model.

After reading the paper, I think the authors did a great effort writing an understandable story line explaining the investigation. The aim of the paper is to accurately model and predict flooding in the karst trough valley of Qingmuguan using the simple QMG model, which needs lower modelling-data demand and a minimized model uncertainty. The entire paper is clearly dedicated to approach the aim. The methodology of the paper deals with model input, model calculations and algorithms, an optimization algorithm and an uncertainty analyses. All these steps together are necessary creating strong results to reach the aim. The figures are strong in substantiating the text and present the results really clear. The methodology is very well interconnected, but misses here and there some details which are necessary to reproduce the investigation. Moreover, the assessment to analyse the model performance is highly debatable. In this I review critically evaluate these missing pieces in the paper and give an advice how to resolve these flaws such that the author can make the paper ready to publish.

Citation:

- Li, J., Yuan, D., Zhang, F., Jiang, Y., Liu, J., & Ma, M. (2021). A physically-based distributed karst hydrological model (QMG model-V1. 0) for flood simulations. *Geoscientific Model Development Discussions*, 1-44. https://doi.org/10.5194/gmd-2021-120

Major arguments

Reading the paper and considering the quality of especially the methodology I jumped into some fundamental flaws. It is necessary to resolve these missing pieces before publishing the paper.

Modified formulas from literature

In the paper the authors refer in the methodology a lot to formulas used from the literature. Many of these formulas are ‘modified’ from literature. This is the case for formulas 1, 2, 3, 12 and 14. However, the authors do not explain what is modified about these formulas.

The authors should explain step for step what and how the formulas are deduced and modified, such that these modifications can be checked on validity.

Darcy-Weisbach transformation

In the methodology the authors formulate the equation used to calculate the conduit water flow of underground river to the outflow basin for turbulent conditions using Darcy-Weisbach. For this formula the authors refer to Shoemaker (2008). Li (2021) used the following formula which represents Darcy-Weisbach:

$$Q_{\text{turbulent}} = -2A\sqrt{\frac{2gd\,|\Delta h|}{\Delta l\tau}}\,\log\left(\frac{H_c}{3.71d} + \frac{2.51\nu}{d\sqrt{\dfrac{2gd^3\,|\Delta h|}{\Delta l\tau}}}\right)\frac{\Delta h}{|\Delta h|}$$

Figure 1 Transformed Darcy-Weisbach formula (Li et al, 2021)

The paper of Shoemaker only describes from which previous studies he deduced a formula for Darcy-Weisbach. Looking further at the general equation of Darcy-Weisbach, I found a different formula than used in this paper (Li et al, 2021). The formula seems to be transformed but it is not clear how they transformed this formula. Therefore, I think the authors should include a description of how they came up with the formula they used as Darcy-Weisbach. Because the way it is now described I was not able to check whether this Darcy-Weisbach equation is correctly deduced and transformed.

I would propose the authors to use the formulas given in the paper Valiantzas (2008) where the formulas used for pipe flow in a turbulent state are described. Using these fundamental equations as starting point they can show how they transformed the formula and they can confirm their transformation such that the use of their notation of Darcy-Weisbach is validated.

Use of IPSO

Beside describing the model environment and the main model calculations, it is of interest to describe how the authors approached to make the model as accurate as possible. Here, in this paper there is an algorithm used to calibrate and optimize the model parameters such that the uncertainty is minimized. This paper clearly describes how they started thinking about which algorithm to use for this explicit task. They came up with the algorithm PSO. However, PSO has some weaknesses and therefore they decided to improve this algorithm and they came up with the IPSO algorithm which includes random processes. A formula for this algorithm was given without any citations. Moreover, it is not entirely clear how they came up with the improvement of PSO by adding the chaotic behaviour resulting in IPSO. Therefore, I consulted literature to get acquainted of the use of ISPO algorithm (Abdi et al, 2013). Reading this paper and its formula for the IPSO algorithm, I saw that there are two main differences compared to the paper about karst modelling (Ji Li et al, 2021).

$$X_{ij}^k + 2 \times rand \times (Pbest_{ij} - X_{ij}^k) \quad \text{if } rand() < \gamma;$$

Figure 2 IPSO algorithm equation (Abdi et al, 2013)

$$\begin{cases} X_{ij} = X_{\min} + (X_{\max} - X_{\min}) * Z_{ij} \\ Z_{ij}' = (1-\alpha)Z^* + \alpha Z_{ij} \end{cases}$$

Figure 3 IPSO algorithm equation (Li et al, 2021)

The first difference is that the paper of Li (2021) uses x_{min} where the other paper uses the notation $x^k{}_{ki}$ and where Li (2021) uses $Pbest_{ij}$ where the other paper uses x_{max}. . After further reading I found out that $x_{min} = x^k{}_{ki}$ and that $x_{max} = Pbest_{ij}$, so this first difference is only a notation difference.

The second difference is the equation used to describe the random processes. Li (2021) used an equation where an algorithm 'a', which is a variable determined by the adaptive algorithm, is used together with Z_{ij}, which is the variable before the disturbance is added, to describe the chaotic processes. The other paper just uses the regular formula 'rand()' to describe the chaotic processes. It is not clear in the paper of Li (2021) why they used this different function for Z_{ij} to describe the random processes. It is also not clear how he came up with this formula (deduced by himself or from literature).

They should mention this in the methodology and not just introduce this formula out of nowhere. I propose to the authors to either show how they came up with the formula describing the chaotic processes substantiated by a reference or to use the same formula for the chaotic behaviour as used by Abdi (2013).

<u>*Model performance; debatable indictor NSC*</u>

In the paper the authors used the well known Nash-Sutcliff coefficient (NSC) to asses the model performance. However, is this NSC sufficient to assume whether a model is 'accurate'. In order to get an answer to this I consulted literature about the NSC and found a clear conclusion from this *(e.g., McCuen et al, 2006)*:

"Many factors influence a sample value of Nash–Sutcliffe coefficient (NSC), and high values of NSC may result even when the fit is relatively poor, such as when the variance of Y is very large. Values of NSC also depend on the sample size, such that the interpretation of "good" versus "bad" fit depends on the sample size. A value of 0.7 may or may not be indicative of a good fit. Therefore, if the Nash–Sutcliffe index is to be used with some sense of reliability, more knowledge about sample values of NSC is needed".

This makes the statement of the author unreliable. This is very important since this NSC determines the model performance and thus the accuracy of the model outcome. Thus, the statement/conclusion of the authors, that the QMG model is 'accurate' in predicting flooding in the Qingmuguan karst trough valley, questionable. This is the answers to the main research question of the investigation and therefore it is crucial that the outcome is reliable.

To resolve the shortcoming of the NSC, Schaefli & Gupta did investigation and published a paper (2007). To properly communicate how good a model really is, it seems necessary to establish appropriate reference or benchmark models; models having an easy-to-apprehend explanatory power for a given case study and a given modelling time step (e.g., Schaefli & Gupta, 2007:, e.g. Seibert, 2001). Therefore, I propose to use, instead of the mean of the observations, a benchmark model which could serve as reference. Such a benchmark model is a very simple model, applicable on the investigation, which simulates the scenario of interest in a very simplified manner. In this case, where we have in and out flow through the karst trough valley, we can use as benchmark model for example the linear reservoir model. The linear reservoir model is a simple mathematical model describing the rainfall–runoff relations of a rainfall catchment area/basin. Using this simple model as benchmark model, to obtain the NSC (instead of using the mean of the observation), we provide an analyses which should fulfil the basic requirement that every hydrologist can immediately understand its explanatory power for the given case study and, therefore, appreciate how much better the actual hydrologic model is (e.g., Schaefli & Gupta, 2007). This would improve the quality of the analyses made in the paper. Thus, I would suggest to add this method to the model analyses.

ITEE - Paper review of *"A physically-based distributed karst hydrological model (QMG 2 model-V1.0) for flood simulations"* – Daan Stalenburg

Minor arguments

Minor corrections in the paper can be done as follows:

P1, line 1: The title seems to suggest that you model floods in karst area, but in the paper you propose a model explicitly for the Qingmuguan karst trough valley. This should be added in the title otherwise it is misleading.

P5, line 113: Here you state that especially the sensitivity of the parameters are analysed. Say here that this is very important to do but that this is subservient to the main aim of this research: Accurately predict flooding in the karst trough valley Qingmuguan using the simple QMG model.

P17, line 415: Add reference for the use of IPSO formula.

P18, line 448: Add reference for the use of the parametric priori distribution.

P25, line 637: Unclear what do to with the sensitivity classification of the parameters. Explain briefly the purpose of this analyses/classification and how you accounted for this considering the outcome.

P24, line 594: When discussing the lack of accuracy on predicting the dry season runoff, explain briefly the definition of this feature.

P27, line 686: I propose to mention the Nash coefficient obtained for the model performance using the initial model parameters (NSC=0.69) and the Nash coefficient obtained for the model performance after calibration and optimization of the model parameters (NSC=0.92). Now the relative improvements you mention seem new and confusing to me.

P41, line 941: Redundant figure. You can consider to remove this figure. You can easily show the KHRUs on figure 1a or 1b.

Instead of listing all figures and tables at the end of the document, it is better to put the figures in line with the text where it corresponds to. This way it makes the reading process more fluent and pleasant.

References

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