Peer review assignment
ITEE

Eva van Hofslot
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Peer Review

Article reviewed: *A physically-based distributed karst hydrological model (QMG 2 model-V1.0) for flood simulations (Li et al., 2021).*

Header

In this paper a physically based, distributed hydrological model, (the Qingmuguan model, QMG), is developed to predict runoff and confluence for a karst trough valley. The QMG model has a simple two-layer structure: a surface part, for modelling runoff and confluence, and an underground compartment for the sub-surface river system. 18 floods recorded between 2017-2019 were used to calibrate and validate the model for the small the Qingmuguan karst valley in China. Sensitivity analysis on 10 of the 12 model parameters has indicated the following order of parameter sensitivity: infiltration coefficient > permeability coefficient > rock porosity > specific yield > saturated water content > field capacity > flow direction > thickness > slope > soil coefficient > channel roughness > evaporation. After model optimization, the Nash-Sutcliffe coefficient, correlation coefficient, water balance coefficient, average relative flow process error, flood peak error, and peak time error were 0.92, 0.90, 10%, 11%, 0.92 and 2h respectively. The study has clear novelty in that it proposes a new simplified model for flood simulations in karst areas that has a limited number of parameters, which is openly available and easily accessible. The paper has a good overall structure, the order of steps taken within the research are sound and well described. Next, different evaluation indices were used to assess the model performance and the model outcome showed to improve for all these indices after it was optimized. In short, the model gives a good overall view of water transported in the specific type of karst area described, as it links surface and sub-surface flow. In general, his paper provides a good starting point for the simulation of peak discharge in karst valleys that are largely affected by topography. The core research has been conducted properly, however, some recommendations are suggested that will help to also communicate this key message and will and put it into context, which need to be implemented before it can be published. The paper already contains the necessary buildings blocks, which individually are strong components, however the linkages between them need to be improved to capture the main findings of the study. The main issue to address is the assessment of model performance, here additional steps are required to claim the model performance is acceptable. Also, the explanatory power of the model is limited in that further elaboration and justification is needed to describe the link between the model and physical processes that occur. Lastly, the relevance of the model to the scientific community is questioned, as only its application to a very small and specific karst valley was tested. To conclude, the work can be accepted after minor revisions will be made.

Major argument #1

To start with the first issue on the assessment of the performance of the model, in line 43 equation 18 is given for the calculation of the Nash- Sutcliffe coefficient. The observed squared difference between the discharge minus the simulated discharge is divided by the squared simulated discharge minus the average value of observed discharge. In table 4, the Nash Sutcliffe coefficients are then given for the improved model results. In line 582 it is stated the Nash Sutcliffe values was 0.79 before parameter optimization and increased to 0.92 after optimization, which is later labelled a reasonable flood simulation result (line 588). However, based on this approach it cannot be directly claimed that the model effectively simulates peak flow in the karst valley, I think additional steps need to be added to this method. Since the Nash-Sutcliffe coefficient is computed with squared values, large discharge values as observed during peak flows are over estimated (Krause et al., 2005), which is a general remark of this method. More importantly, the Nash-Sutcliffe assessment only provides a relative indication of the model performance, nothing can be said about performance in absolute terms, it only gives an indication about the amount of noise generated by the model compared to the signal generated. Thus, the peak flow prediction of the QMG model is better compared to the reference model, in this case the average value of observed discharges over the observation period of two years. Yet, if discharge is averaged this reduces the information on peak discharge, which is the main variable of interest. Therefore, I would recommend that the model is evaluated with a Nash-Sutcliffe efficiency that uses a well-defined benchmark model, suitable for the variable of interest, for example the calendar day benchmark model that uses an interannual average value for every calendar day, as suggested by Schaefli & Gupta (2007).

Major argument #2

The next issue is the relevance to the scientific community. The model aims to provide a simple model to simulate floods in karst trough valleys. In section 698 hesitance regarding its applicability to other regions is addressed. The model was calibrated and validated only for this specific karst area, which is a small basin of 13.4km2 (line 118). Many other karst areas occur in China, let alone globally, which would vouch this model's relevance. However, as noted in this study and many others, for example Bakalowicz (2005), modelling water flow in karst areas is very complex due to the heterogeneity of site characteristics such as the extent of network of conduits. Consequently, the QMG model might not generate accurate results for larger areas, due to the spatial variety present in such areas. Therefore, I would like to recommend researching additional methods that can be used to guarantee effective simulation of flow through larger areas, that add to the tracer test method that was already mentioned.

Major argument #3

The next issue is that it is not well supported whether the model represents reality accurately, the performance of the model is not put into context. In line 685 it is stated that the flood simulation after parameter optimization with IPSO was much better than simulation of the initial model parameters, and that the six indicators of model performance demonstrated increase overall outcome of the model after optimization (line 687-698). However, this indicator only shows that the optimized parameters score better compared to the initial parameters, the actual physical processes are not directly involved in this. The gap between the conceptual model constructed and the actual physical processes is unaddressed, so these optimized parameters are no guarantee for exact representation of the physical reality. This has consequences in the sensitivity analysis, which is the next part of the study. In line 694 it is concluded that the rainfall infiltration coefficient is the most sensitive parameter. However, this is based on the representation of reality that is constructed in the model. Therefore, I would recommend to firstly add a sketch of the conceptual model in the methodology section, after the separate equations for the main processes are discussed. Secondly, I would add additional sources that justify this conceptualization of the system, so previous studies that adopted a similar approach, for example Epting et al. (2018).

Minor arguments

- In line 451 a Nash-coefficient of 0.85 is mentioned as threshold, above which the model performance is labelled acceptable, although this number intuitively makes sense it needs be explained or supported by a reference.
- To add to the assessment of the model performance for dry period simulation the log-transformation of the Nash-Sutcliffe coefficient can be calculated.
- It is not always explicitly stated what is meant inexplicitly. For example, the aim and RQ are included in the first section of the paper, and from reading the overall paragraph you can deduce the aim/RQ yourself, but it is not directly stated, so this needed to be stated more clearly.
- In equation 2, the parameter *fi* is not explained in the text below

- It is not clear why 10 cycles were chosen for the IPSO parameter optimization (line 412), a justification for this number should be added to, why does 10 cycles result in global convergence of the model?
- For figure 1a, the legend is quite difficult to identify, especially icon 4 and 5 are very hard to find in the map → increase size of those figures in the map
- For figure 6 the dates displayed on the x-axis are very hard to read. I would suggest displaying less dates (for example only months). Next the label of the simulated Q, both with IPSO and the initial parameters are misleading; it seems as if the *simulated Q* is *divided* by the *IPSO* and *Initial parameters*. This same confusion of labels is present in the graphs of figure 7.

Typos

The language needs some extra revision, below a list of example typos is given that should be improved:

- Line 19: strcutrues → structures
- Line 39: the "." before "Because" should be a comma
- Line 44: add a comma between "increased" and "that", the sentence is really long now
- Line 94: "which make distributed model may need": there is an error in this sentence, it is not readable
- Line 108: "work" → add 's'
- Line 109: between parameters and we, the dot needs to be a comma
- Line 177: "Berry et" → 'al.' misses from the reference
- Line 675: after "follow", change the dot to a colon

References

Bakalowicz, M. (2005). Karst groundwater: a challenge for new resources. Hydrogeology journal, 13(1), 148-160.

Epting, J., Page, R. M., Auckenthaler, A., & Huggenberger, P. (2018). Process-based monitoring and modeling of Karst springs–linking intrinsic to specific vulnerability. Science of the total environment, 625, 403-415.

Krause, P., Boyle, D. P., & Bäse, F. (2005). Comparison of different efficiency criteria for hydrological model assessment. Advances in geosciences, 5, 89-97.

Schaefli, B., & Gupta, H. V. (2007). Do Nash values have value? Hydrological Processes, 21(ARTICLE), 2075-2080.