Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-438-AC1, 2018

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Flood forecasting in large karst river basin by coupling PERSIANN CCS QPEs with a physically based distributed hydrological model" *by* Ji Li et al.

Ji Li et al.

445776649@qq.com

Received and published: 12 November 2018

Reply to the editor's comments-AC1 Anonymous Referee #1 Received and published: 18 October 2018 General comments The paper concerns a topic consistent with the aim of the journal and interesting for the scientific community. The presented analysis could be potentially useful and I really appreciate the huge work made by the authors, but many drawbacks affect the manuscript and have to be addressed before the paper could be considered worthy for publication. Basically, I think that the manuscript has serious limits described in what follows. 1) English need a deep review: I found several unclear sentences, most of which incomplete. This makes it very difficult, especially

in some sections of the paper, to understand the analysis carried out by the authors and the results obtained. 2) structure of the manuscript and quality of presentation: the structure of the paper is basically fine, but the contents need modifications. Some results are already presented in section 5, before section 6 'Results and discussions'; the 'Methodology' section should be reorganized mainly in terms of links between the different components. However, the main limit of the manuscript is the lack of clarity in the description: for instance, the novelty introduced in the paper is not clear indicated neither in the abstract nor in the introduction and the reader can understand that the used distributed hydrological model is improved for karst basins only in section 2.3. It is necessary that the scientific novelty introduced in the work is specified. Many works have used satellite precipitation data as input for hydrological models (mainly not in karst basins), have post-processed them and used the new input data also to recalibrate the model parameters (some of them should be mentioned). So, from my point of view the main novelty concerns the type of the river basin. Further, it is very hard to infer the structure used in the hydrological model: a) section 2.3 is generally confused, e.g. it is not specified clearly what 'rapid flow' means and if eq. 2 refers to tiny pores; b) when presenting the Muskingum model the authors introduce the 'forecasting version' but, actually I cannot understand why since it seems that the analysis refers to flood prediction. The flood forecasting is mentioned many times in the manuscript, even in the tile, but I do not see 'forecasting' or, at least, this aspect is not clearly explained. 3) It is not specified how the PERSIANN data down-scaling carried out. 4) It is not clear how the sub-basins are identified in the study area. Each grid is considered as a uniform basin or a set of grid cells? 5) The post-processing of the PERSIANN data is carried out considering only 23 rain- gauges. Why? Add details. Other comments 1) Figure 2: it seems that there is some problem with the scale of the two figures. 2) Table 3 and 4 are not able to provide a synthetic and effective information. 3) Figures 5-9: What do the different colors of the lines represent? Please, use the same range on the y axes of both figures and the date for the x axes. 4) Figures 11-12: Where the results are shown? At which river section? 5) References: Chen et al. (2011) is mentioned in

the text but not in the list; Liang (1997) is in the list, but not mentioned in the text.

Author's reply Firstly,thanks very much for the referee for reviewing this manuscript. Following are responses to the reviewer's comments one by one. 1) English need a deep review: I found several unclear sentences, most of which incomplete. This makes it very difficult, especially in some sections of the paper, to understand the analysis carried out by the authors and the results obtained. Will be down in the revision. There are some syntax errors and unclear sentences throughout the paper, which makes it hard to understand the meanings of some sentences. And a native English speaker will help to carefully proofread the whole paper in the revision. 2) structure of the manuscript and quality of presentation: the structure of the paper is basically fine, but the contents need modifications. Some results are already presented in section 5, before section 6 'Results and discussions'; the 'Methodology' section should be reorganized mainly in terms of links between the different components. However, the main limit of the manuscript is the lack of clarity in the description: for instance, the novelty introduced in the paper is not clear indicated neither in the abstract nor in the introduction and the reader can understand that the used distributed hydrological model is improved for karst basins only in section 2.3. It is necessary that the scientific novelty introduced in the work is specified. Many works have used satellite precipitation data as input for hydrological models (mainly not in karst basins), have post-processed them and used the new input data also to recalibrate the model parameters (some of them should be mentioned). So, from my point of view the main novelty concerns the type of the river basin. Further, it is very hard to infer the structure used in the hydrological model: a) section 2.3 is generally confused, e.g. it is not specified clearly what 'rapid flow' means and if eq. 2 refers to tiny pores; b) when presenting the Muskingum model the authors introduce the 'forecasting version' but, actually I cannot understand why since it seems that the analysis refers to flood prediction. The flood forecasting is mentioned many times in the manuscript, even in the tile, but I do not see 'forecasting' or, at least, this aspect is not clearly explained. The structure of the paper will be modified in the revision. The original paper structure was :1 Introduction- 2 Methodology- 3 Study

area and data- 4 PERSIANN-CCS QPEs and its post-processed results- 5 Model set up- 6 Results and discussions- 7 Conclusion. In consideration of some contents in the part-4 PERSIANN-CCS QPEs and 5 Model set up should belong to the part 2 –Methodology,so the structure of the paper will be modified to make it easy to understand the sequence and the logical relationship.And after revised, the new structure of the paper will be: 1 Introduction- 2 Study area and data - 3 PERSIANN-CCS QPEs and its post-processed results - 4 Hydrological model - 5 Model set up- 6 Results and discussions- 7 Conclusion. Furthermore, the comment pointed out some results are already presented in section 5, before section 6 'Results and discussions'. And these results will be replaced in the section 6 'Results and discussions' in the revision. The comment pointed out the main limit of the manuscript is the lack of clarity in the description. And the novelty introduced in the paper is not clear indicated neither in the abstract nor in the introduction. Actually, the main novelty of the paper is to improve the structure and function of physically based distributed hydrological model-Liuxihe model by adding karst mechanism.This is also the first time that Liuxihe model has been used in flood simulation and prediction in karst basin as an attempt in this study. The description of the improved structure and function for Liuxihe model will be added in the abstract and the introduction in the revision. For instance, the sub-basins are divided into many karst hydrology respond units (KHRUs) in this paper to ensure the model structure is refined enough to suit the karst landforms. In addition, the karst hydrological process including the 'rapid fissure' and 'slow fissure' in the epikarst zone is considered a lot in the model structure. And there is lack of typical rainfall data to build a hydrological model in karst basins, the PERSIANN CCS QPEs could offer a reasonable and high-resolution rainfall data, and coupling the PERSIANN CCS QPEs with a physically based distributed hydrological model has far reaching application potential in karst flood simulation and prediction. Also recalibrate the coupling model parameters is a novelty in this study, it can largely improve the performace of model in flood prediction The comment pointed out it is not specified clearly what 'rapid flow' means and if eq. 2 refers to tiny pores. The original sentence in the paper is"The rest of the

water content will enter the tiny pores in the surface karst zone, and the water content of rapid fissure flow could be described as the following equation:"

In fact, there is a mistake in spelling, and the word 'rapid fissure flow'should be changed to 'slow fissure flow', and it will be modified in the revision. The comment pointed out when presenting the Muskingum model the authors introduce the 'forecasting version' but, actually I cannot understand why since it seems that the analysis refers to flood prediction. The flood forecasting is mentioned many times in the manuscript, even in the tile, but I do not see 'forecasting' or, at least, this aspect is not clearly explained. This suggestion is very pertinent .In this study, the karst flood process were simulated and the results analysis refers to flood prediction . It is not inappropriate to use the 'forecasting' here. So the word 'prediction' will replace it in the whole paper in the revision. 3) It is not specified how the PERSIANN data down-scaling carried out. The QPEs products of PERSIANN-CCS has been generated precipitation estimates at resolution 0.04°*0.04° scale and time interval 30 minutes since 2000. The output of PERSIANN-CCS QPEs has been downscaled at 200m*200m as the same spatial resolution as Liuxihe model in LKRB. And the down-scaling method is used in this paper based on statistical relations between meteorological variables, and DEM data using LOO (Leave-One-Out) cross evaluation method and spatial autocorrelation analysis methods(Fan et al., 2017). 4) It is not clear how the sub-basins are identified in the study area. Each grid is considered as a uniform basin or a set of grid cells? The whole catchment in this study is divided into a great number of grid cells horizontally by using the high-resolution DEM data, named sub-basins. Each grid is considered as a uniform basin, and the elevation, land cover type, soil type, and other model elements including rainfall-runoff, evapotranspiration and so on are calculated on the uniform basin. The description of the sub-basins are identified in the study area will be modified in the revision. 5) The post-processing of the PERSIANN data is carried out considering only 23 rain- gauges. Why? Add details. There are a total of 68 rain gauges and 131 grid points of PERSIANN-CCS QPEs in LKRB. However, only 23 rain-gauges are closest to the grid points of PERSIANN-CCS QPEs. And taking

the precipitation of these 23 rain gauges as the true precipitation value to revise the results of the PERSIANN-CCS QPEs in this paper. The detail revise method is shown in section 3.4 The post-processed PERSIANN-CCS QPEs. Other comments 1) Figure 2: it seems that there is some problem with the scale of the two figures. Will be down in the revision.The scale of the figure 2(a)and(b) will be modified to the same scale.

2) Table 3 and 4 are not able to provide a synthetic and effective information. Table 3. Evaluation indices of simulated flood events with the post-processed PERSIANN-CCS QPEs is to provide the information on the flood simulation results between the initial PERSIANN-CCS QPEs and the the post-processed ones; And Table 4. Evaluation indices of simulated flood events with different model parameters is to provide the information on the effect of recalibrating the coupling model parameters. In order to make the descriptions of Table 3 and 4 clearer,the title of them will be modified in the revision. For that Table 3. Evaluation indices of simulated flood events with the initial PERSIANN-CCS QPEs and the the post-processed ones;Table 4. The effect of recalibrating the coupling model parameters. Also the flood simulation result by rain gauge precipitation are deleted in Table 4. Because it is not necessary here and is already in Table 3. 3) Figures 5-9: What do the different colors of the lines represent? Please, use the same range on the y axes of both figures and the date for the x axes. In Figures 5-9, there is a mistake here. It should be the same colors of the lines represent, and I will use the same range on the x and y axes for the both figures. It will be down in the revision. 4) Figures 11-12: Where the results are shown? At which river section? The karst flood simulation and prediction results at the outlet of the basin, it is also the site of Liuzhou river gauge in Figure 3. 5)References: Chen et al. (2011) is mentioned in the text but not in the list; Liang (1997) is in the list, but not mentioned in the text. There are two redundant references in this paper, Chen et al. (2011) and Liang (1997), and both of them will be deleted in the revision.

References: Fan, K.K.,Duan, L.M.,Zhang, Q., Shi, P.J., Liu, J.Y., Gu, X.H., and Kong, D.D.: Downscaling Analysis of TRMM Precipitation Based on Multiple High-resolution

Satellite Data in the Inner Mongolia, China. Scientia Geographica Sinica, 37(9):1411-1421, 2017.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-438/hess-2018-438-AC1-supplement.pdf

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-438, 2018.