

***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.**

Anonymous Referee #1

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“Flood forecasting in large karst river basin by coupling PERSIANN CCS QPEs with a physically based distributed hydrological model”

by: Ji Li, Daoxian Yuan, Jiao Liu, Yongjun Jiang, Yangbo Chen, Kuo Lin Hsu, Soroosh Sorooshian

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

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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 title, 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.

Please also note the supplement to this comment:

<https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-438/hess-2018-438-RC1-supplement.pdf>

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

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