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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Comparing the performances of WRF QPF and PERSIANN-CCS QPEs in karst flood simulations and forecasting with a new Karst-Liuxihe model" *by* Ji Li et al.

Anonymous Referee #1

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In this submitted manuscript, authors (1) evaluated the QPEs from PERSIANN-CCS product, and the WRF QPF developed for Karst regions, (2) developed a karst region-specific hydrologic model, termed Karst-Liuxihe Model by adding some enhancement modules to an existing Liuxihe hydrologic model. The current manuscript suffers from several major issues and the referee cannot suggest acceptance.

Major issues: 1. Reviewer is not convinced about how the proposed Karst-Liuxihe model could address the challenges of hydrologic simulation over Karst areas. Authors mentioned two shortcomings of using distributed hydrologic models in lines 156-167, with one challenge being lack of in-situ data, and another of high computational effi-

ciency of parameter calibration. Authors also summarized the way this study tries to address these two challenges by: (1) doing "field survey and tracing test" and "collect data from internet at no cost", and (2) using "An improved Particle Swarm Optimization method (Chen et al. 2016)" for parameter estimation, respectively. However, neither of these approaches are based on the innovation of modeling scheme, rather, they are commonly standards for obtaining data or calibrating hydrologic models. Therefore, the novelty and contributions of this study are questionable.

2. The organization of the entire introduction is very confusing. Sometimes, authors talked about features of Karst regions, remote sensing v.s. in-situ data, forecast models, distributed hydrologic models vs lump hydrologic models, data needs and challenges, development of the proposed Karst-liuxihe model, the lead time and resolutions, and tangibly relevant literature without any detailed summary of their experiments and conclusions. As a result, reviewer is not able to identify the following (1) background and Motivation of this study, (2) novelty and contributions, (3) methodology developments, (4) advantages of the proposed methodology, and (5) how authors demonstrate the hypothesis or conclusion by designed studies.

3. With respect to the results of Figure 3-7, which show the comparison among WPF QPF, PERSIANN-CCS QPE and gauge observation, Reviewer has two major concerns. (1) how fair it is to compare a precipitation forecasting product with an estimation product? It becomes the essential issue of comparing "apple" to "orange". Comparison can be done, however, any results or conclusions drawn from such type of comparison are based on the assumptions that "apple" and "orange" have the same mechanisms, physical dynamics, and functions. Unfortunately, they are not. QPE products shall be compared with gauge or other QPE products, such as comparing different remote sensing products over the same region. QPF products shall only be compared to ground truth or other QPF products with the same lead time. (2) A second concern is that authors have claimed several times that the reliability of gauge precipitation and the lack of data are the main motivations of this study to use remote sensing and model

forecasts data to drive their proposed hydrologic models. Now with the direct comparison with gauge observations, the gauge precipitation is used as a reference to evaluate two precipitation products, in which the underline assumption becomes that the gauge precipitation is true and accurate in the Karst areas. If this is the case, why bother using another remote sensing product to estimate precipitation over the Karst areas? Since both gauge and PERSIANN-CCS are historical or near-real-time observations. This underline assumption, though not directly mentioned, contradicts the design of experiments and motivations of using WRP QPF and PERSIANN CCS QPE as inputs to their hydrologic simulations.

4. Similar to previous concern, the post-processing steps to obtain gauge corrected average PERSIANN-CCS and WRF-QPF in equation 1-3, as well as section 3.4 (post-processing of the 2 weather models) rely on the same assumption that gauge networks are more reliable than others over the study region. Then, why bother to use another precipitation estimation product without any lead-time?

5. There are a quite amount of results presented by authors on 5 different floods simulations, using the original and bias-corrected WRF QPF and PERSIANN-CCS. It is not surprising that the bias-corrected (post-processed) WRF QPF and PERSIANN-CCS have better statistics than the simulations than the original inputs. The main concern reviewer has is that what evidence proves that the proposed Karst-Liuxihe Model is better than the original version of Liuxihe model? It seemed all simulations are with the proposed model without any baseline comparison to its original version, and repeated but same simulation do not necessarily add values to prove the advantages of the proposed model.

6. Authors first conclusion is that "The postprocessing method proposed in this study could largely reduce these relative errors." However, reviewer finds in Table 4 that even the original data was not corrected, it still leads to a relative good simulation of floods with NASH values about 0.7 or above. If this is the case, why we need to postprocessing the WRF-QPF and PERSIANN-CCS? We can still obtain good flood simulation by

tuning the hydrologic model parameters.

7. Authors conclusion No.4 on "The flood processes simulated by the Karst-Liuxihe model using the rain gauge precipitation were the best". The use of gauge should be a baseline and not a conclusion. The model is calibrated using the gauge, and of course, the rain gauge precipitation could produce the best flood simulation.

8. The authors include the lead time error of both WRF-QPF and PERSIANN-CCS in Tables 4 and 5. All simulations have negative peak time error, T, also see authors last conclusion. Reviewer is wondering how to interpret this number. Do negative values mean the peak is predicted to happen prior to the actual flood peak? or the other way? If it means a delay in simulation, how we use these data to timely predict potential floods? If this is prior to the flood peak, reviewer is wondering how PERSIANN-CCS and gauge inputs can produce a lead time given the data itself is historical precipitation estimates instead of forecasts into the future.

9. Authors seem to know but are reluctant to provide more background information on using QPF or QPE on hydrologic simulations over Karst areas. Line 77-78 says that “only a few studies of rainfall forecasting based on WRF QPF and PERSIANN-CCS QPEs have been conducted in karst areas until now, and even if there are studies, the practical accuracy is generally poor.” Are there any relevant studies? Reviewer believes there should be lots. What will be the differences between this one and other literature?

10. Another concern is the investigation of parameter sensitivity. In this study, authors applied NSE to draw the conclusion about parameter sensitivity (Line 654-656 and Table 3) without perturbing each individual parameter. The current use of NSE only demonstrates how well the simulation can represent the observation. But, it has nothing to do with the sensitivity of parameter in the hydrologic model. More explanations of how authors evaluate the parameter sensitivity are needed.

Minor issues:

Abstract Line 20-27: This sentence runs for more than 8 lines without any clear structure. Please break into 2-3 smaller sentences for better readability.

Line 66-68: this sentence is very cumbersome, The parts of "enable .... To be easily obtained" needs to be rewritten.

Line 84-86: there are a few grammar errors in this sentence. Please re-write.

Line 104-106: Ground gauge, of course, has no lead time as compared to the forecasting model. Not sure why authors emphasize on this known fact.

Line 108: grammar error. "People" cannot be "transferred". Do the authors mean "evacuate"?

Line 209: "The channel length of Liujiang river is about 1120 km and the area is about 5.8×104 km2". What area is about 5.8×104 km2 here? The area of water surface of the river?

Line 265&267: Why use the phrase "property data" here? What is the meaning of it?

Line 350-351: The reviewer believes the authors are trying to say "the rainfall distributions of WRF QPF, the PERSIANN-CCS, and the observed Precipitation data appears to be quite similar to each other".

Line 385, and Line 399-404: In equation (1), the reviewer presumes the unit of Pi should be mm/cm/m, and the unit of Fi should be a mm2/cm2/m2. Thus, Pi times Fi should give us the volume of water here. Then Fi×Pi / N is still in volume. Later postprocessing procedure cannot be continued according to the instruction. It is important to give correct and detailed steps and evaluation of the applied postprocessing of two precipitation inputs.

Line 329-344 It seems like the authors did not evaluate precipitation product for the year of 2010. Is it because there was no flood event occurred in 2010? The following question would be why evaluate two precipitation product's performance only under

several flood periods? Why not evaluate both precipitation products over as a long time period as possible?

Line 473: It might be better to use "improve" instead of "increase" in the section title.

Line 646 It might be better to say "the MPSA was modified and improved from the GLUE algorithm".

Line 842: It is very difficult to tell the difference between the three types of rain gauges on Fig1a. Suggesting change high contrast color combination.

The abstract seems to be very long without a concise focus on the scope of work and summary of novelty and motivation of this study. It reads like an introduction instead of a concise abstract.

Unsupported claim on "Among these weather models, WRF QPF and PERSIANN-CCS QPEs may be better ways to acquire precipitation results effectively in karst basins." Why WRF QPF and PERSIANN-CCS are selected for this study? References are needed.

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