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Interactive comment on "Extending flood forecasting lead time in large watershed by coupling WRF QPF with distributed hydrological model" *by* Ji Li et al.

Anonymous Referee #2

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General comments: This paper aimed to show that the performance of hydrological forecasts can be improved and the lead times of hydrological forecasts can be extended by coupling a distributed hydrological with the WRF model. Through a series of numerical experiments, the paper showed the WRF QPFs have an over-bias in precipitation forecasts. It proposed a post-processing procedure that aims to reduce the biases. It also showed that model calibration using post-processed WRF QPF can lead to better hydrological forecasting performance than using historical observed hydrological data. Finally, the paper showed that forecasts for shorter lead times have higher skills than forecasts with longer lead times. I think coupling NWP model with a process based hydrological model is a viable way to improve the skill of hydrological

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forecasts and to increase forecast lead times. However, I found some major problems with the manuscript. First of all, how the WRF model is setup is not described at all. This is important for readers to understand how the WRF model is run and how to interpret the WRF forecasts. Second, the forecast results from the coupled models are based on a very limited sample of events. This makes it very hard to believe that the conclusions can be generalized. For example, the correction factors computed by the post-processing method presented in the paper is based on the three simulated events and the results can be arbitrary due to its dependence on the specific selection of the events. Therefore, I don't think the method can be generalized to other events or in real-time forecasting setting. There is a similar problem using data with a limited sample size to conduct model calibration. Model parameters must be determined based on a large sample of data for the calibration results to be robust. If parameters are optimized based on a single event, you would obtain different optimized parameters every time a different event is used. Third, there are a lot of language problems with the manuscript. A serious editing by a language proofing company or by a native speaker is warranted to remedy those problems. Below are some specific comments. Specific comments: Lines 36-40: There should be periods between sentences. Lines 42-48: these sentences here suggest that hydrological forecasting based on observed rainfall only has a limited lead time. I agree with this point, but for large basins (e.g., the Three Gorges Basin), the forecasting skill gained from hydrological routing based on upstream and downstream streamflow information can be very significant (up to 72 hours or more), especially after major storms. This skill will of course diminish after several days. Therefore, I think the sentences should be modified to reflect this point. Lines 98-101: I think parallel computing has made distributed hydrological modeling less computationally demanding. But to say computational burden does not exist is an over-stretch of the fact. We still face challenges when running hyper-resolution distributed models. Lines 101-06: on automatic calibration, I don't think there is a well-established way to calibrate distributed hydrological models, even though there are plenty of attempts to do it. I think more careful wording should be used here. Lines

108-122: I think this whole paragraph jumps into conclusion by presenting the results first. If you indicate WRF QPF over-estimates the precipitation, wait until you present the results of this numerical experiment. By the same token, if you suggest that post-processing of WRF QPF helps, you also need to present the results first. At this point in your paper, you also need to discuss what scientific problem you intend to solve, what is unique about your approach. Line 124: “studied area” should be “Study area” Line 138: “focus” -> “focuses” Lines 139-141: the number of events is too small to prove the effectiveness of this approach. A much large sample should be used. It is especially true for the post-processing to be presented later. Line 163: what is “hiemal”? Line 191: the “Li et al. (2014)” paper is not shown in the reference list. Lines 206-211: there is no discussion on the WRF model setup in terms of spatial domain definition. What exactly is the grid domain used for the study? How are the lateral boundary conditions or the initial conditions set? How can you justify that 20x20 sq km grid is adequate for the LRB region? Normally people utilize a nested domain approach in order to better capture the local features of the basin. Why this approach is not used here? Section 3.4: I understand it is necessary to correct the biases in WRF QPF. But how can this correction be applied in real-time forecasting? As shown in section 3.3, the biases change from storms to storms and for different lead times. It is impossible to perform correction consistently because the correction factors change all the time and cannot be determined a priori. I think post-processing is necessary. But the correction factor must be determined on the basis of a large sample, not an individual event. Usually post-processing can be conducted by making use of a big archive of long hindcasts (preferably 20 years or more) using the WRF model under similar setup. I understand it is a huge undertaking and probably cannot be executed by a graduate student. Line 264: the numbering of the section should be 4, not 3. Line 289: I find the model parameter optimization done here is problematic. First, model calibration should be done using a larger sample of hydrological events. Otherwise the parameters are not going to be robust under different conditions. In this study, one event is used to calibrate the model parameters. If another event is used, it is highly likely that different optimal pa-

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Discussion paper

rameters would result. Second, I find the post-processed WRF QPF to be problematic as well for reasons I gave in comments related to section 3.4. In theory, if the hydrological model is reasonable and observed hydrological data are reliable, the calibrated parameters based on this kind of information should be fine when used in real-time forecasting. Here in this study, I am not sure if the authors have articulated clearly why calibrated model parameters based on the use of historical hydrological observations should not be used. Lines 308-312: I am not sure how the downscaling is performed. The authors should explain what is the nearest downscaling method. Lines 314-351: I don't dispute that the hydrological forecasts using post-processed WRF QPF is better than raw WRF QPF in all measures of performance metrics. As I stated before, it is not possible to determine the correction factor in advance, it is hard to justify that we can use the post-processed WRF QPF in real-time forecasting. Lines 352-386: Again, I return to the question of how robust the optimized parameters are. Model calibration must use a large sample of data to obtain consistent parameters that work under a variety of conditions.

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