Hydrology and
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Discussions

Interactive comment on "Large watershed flood forecasting with high resolution distributed hydrological model" *by* Yangbo Chen et al.

Yangbo Chen et al.

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Anonymous Referee #1 In this work, a physically based distributed hydrological model was used for flood forecasting in a large watershed to validate the feasibility of distributed hydrological model's application for large watershed flood forecasting. The research objective is significant. A suitable revision is needed before it can be accepted for publication by HESS, and the following comments below should be addressed:

Reply: Thank the reviewer for his/her comments, revisions have be down based on the reviewer's comments. Following are responses to the reviewer's comments one by one.

1. It is insufficient that discharge from only one station were used for validation in such a large watershed with 58270km2. Because the same effect may come from different

combinations of parameters, more hydrological variables need to be checked seriously. Hence, it is suggested that validation with more observation at different river locations in the area should be added or that spatial observations, such as evaporation and soil moisture from satellite data, could be utilized for check the model performance in this large watershed.

Reply: If there are more information available, such as the reviewer recommended, it will surely improve the model performance, but in practice, the mostly available data is the discharge at the watershed outlet, it is the available data for most large watersheds. Results in this paper has shown with this data, the model parameters could be optimized reasonably well, and the practice in this paper is acceptable. For Liujiang watershed studied in this article, there is no other observation data, so the authors will not be able to do more works on this aspect.

2. In section "Parameter optimization", some are unclear. Are the parameters of PSO fixed for once? What is the objective function? As the paper tells it is set to minimize the peak flow error, but from the Fig 4(c), the peaks have not yet been captured well enough. The result of optimization could be not the real optimal. More trainings are needed. In addition, Nash–Sutcliffe coefficient may be more suitable for the objective function.

Reply: The parameters after optimization will be fixed, but they could be re-optimized if there are new data, or with different optimization strategies. Different objective function could be employed, it is flexible. Actually, we have tested a few objective functions including minimizing Nash–Sutcliffe coefficient, maximizing correlation coefficient, minimizing process relative error, minimizing peak flow relative error and maximizing water balance coefficient. The results shown that with the objective function of minimizing peak flow relative error, the overall accuracy for peak flow is the best one, that fits the large watershed flood forecasting's concern, so the objective function of minimizing peak flow relative error is employed.

3. How did the simulation consider the reservoir regulation in the work?

Reply: In Liujiang watershed, there are no big reservoirs with significant regulation capacity, so in this study, reservoir regulation was not considered.

4. The authors compared model performance with model resolutions at 200m, 500m, 1000m. Although the result is significant, it is better to add some comparisons with model resolutions with smaller interval, such as 300m or 400m. This manuscript does not explain the reason why this work has just chosen those model resolutions.

Reply: Computation is a big burden for distributed modeling, computation in this article took more than a year as it needs a few tries to finish a complete run for one DEM resolution. Thanks the reviewer for a rapid comment, and in the past three months, the authors tried two more model resolutions at 400m and 600m, the results did not change the conclusion of this article based on the previous results. In this article, these new results has been added, please see the revised article, Fig. 6 and Fig. 7, Table 5 and Table 6.

5. In this paper, the abstract should be more concise and the motivation is not very clear.

Reply: The abstract has been rewritten, please see the revised article.

6. From line 116 to 133, the related works should be classified and then summarized concisely.

Reply: As these works have been published for some time, so to make this article concise, they are only mentioned in this paper, and will not be further summarized in detail, so revision is not done for this part.

7. If this work did not modify the Liuxihe Model published in the previous works, I suggest section 3.1 and 3.2 should be merged into one part. The description of the model could be reduced. Some contents in Section "introduction" and 3.1 are repeated.

Reply: The other reviewer suggested a more detailed introduction to the model. To combine the comments of both reviewers, considering Liuxihe Model employed in this study has been published in internationally refereed journals, so only a briefly introduction to the model structure, the components and algorithms used is kept, and move to the second section: Method and data. Please see the revised article.

8. The coordinate information in the maps of Fig. 1-3 and Fig. 6 should be displayed. The plotting scales should be the same for all maps.

Reply: The authors think there is no need to add the coordinate, so no revision be done.

9. The font in some figures should be accord with that in the manuscript. The units in some figures look unprofessional. For example, in Fig. 4(c), the title of the x axis should be "date" without unit.

Reply: Done in the revision.

10. The trend lines in Fig. 5 and 7 are not clear, especially during the flood. All the plots should be rearranged in the panels.

Reply: Done in the revision.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-489/hess-2016-489-AC1-supplement.pdf

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