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Interactive Comment

Interactive comment on "Simulating typhoon-induced storm hydrographs in subtropical mountainous watershed: an integrated 3-layer TOPMODEL" *by* J.-C. Huang et al.

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1.Both in the Abstract (page 1102; lines 12, 13) and in Section 4.3 (page 1115; lines 7, 8), the authors write that the obtained validation results demonstrate the applicability of the presented 3-layer TOPMODEL in subtropical watershed. For me, the presented validation results don't allow a reader to evaluate the model applicability and the aforementioned conclusion looks too optimistic. The point is that the number of flood events used for the model validation (4 events) is too small, especially, in comparison with ones used for the calibration (14 events). In other words, the presented results of the model validation are deficient: the overall model performance based on these results is very sensitive to the errors of the individual floods and the performance assessments can be rather casual. The conclusion on the model applicability would be more convincing if the authors validated the model by approximately the same number of floods as ones used for calibration, for instance, by splitting the available flood sample into the equal parts (9 for calibration and 9 for validation)

To convince readers, we added 4 more storm events (now it is 22 events), of which 11 events were used for calibration and 11 for validation. To make it more convincing, we even separated the 22 events based on amount of total rainfall thus the two subsets now hold similar rainfall ranges. All the related tables and figures were revised according to the new results. We appreciated this comment.

2. Additionally, the results for the 18th event (one of the four validation floods) should be revised. As it follows from Table 5, the simulated peak discharge of the 18th flood is 270.2 m^3s^{-1} (about 10% higher than the observed flood peak discharge which is indicated as 245.9 m3s-1) and the time of the simulated peak coincides with the observed one. However one can see from Fig. 5 that the observed peak discharge of the 18th flood is actually much higher (more than 350 m^3s^{-1}) and occurs later than the simulated flood peak discharge. Taking into account these circumstances, the simulation errors for the 18th flood should be changed in Tables 4 and 5 as well as the overall validation results should be changed in Table 4.

The Event 18 (Event 11 in revised version) that editor pointed out was a double peak case. In this kind of rainfall pattern, sometimes the rainfall peak and discharge peak are decoupled. This inconsistency actually confused the calculation of the error of peak flow, EQP, which is defined as the ratio of simulated peak flow over observed peak flow. Since our evaluation was based on observations, the first peak was selected. Indeed, some multi-rainfall-peak storm simulations have this kind of pattern. We pointed out this phenomenon in this version. *However, there are still some events falling out of $\pm$ standard deviation. For example, those events hold two-peak rainfall pattern with the second peak smaller in observation (Event 5, 11, and 16). Yet, sometimes the second peak in simulation is larger than the first peak. The cause for such decoupled rainfall-*

runoff response remains unknown and apparently it cannot be simulated by models if the decoupling is real. Heterogeneous rainfall spatial pattern is plausible, that is, one raingauge station is insufficient though the watershed area is small (P.14 Line 22 to P. 15 Line 2).

3. Summarized the preceding, I suggest the authors to give short discussion on the selection of floods for the model calibration and validation (by the way, why the highest floods of 1996, 1998, 2000 mentioned in the Introduction were not simulated?) and to mitigate the conclusion on the model applicability.

The four typhoons (Event 6, 7, 20, and 21) described in the Introduction were now applied in this version. For clarification, we add one more column, typhoon names, in Table 1 to identify those cases.

4. I agree with the authors that the analysis of the confidence intervals of the simulation errors, which is presented in the Section 4.5, may be useful for the model users. However, I disagree with some interpretations of the obtained results. Particularly, it is obvious that the wider confidence intervals estimated after the calibration phase (and, consequently, the worse the model) the more probably validation results fall into these intervals. In this sense, it is not very important that the validation results are enveloped by the confidence intervals. More importantly is to analyze the confidence intervals and to show that they are not so wide to be able to hold any validation results. For example, the confidence intervals for low flow look too wide for me. The confidence intervals for high discharges are much better but one can see from Fig. 6 that there are systematic underestimations in simulated high discharges. I suggest paying more attention on the analysis of the obtained confidence intervals in Section 4.5 and I believe that this analysis would be really useful for the model users.

Certainly, wider confidence intervals were useless though they completely enveloped validation results. By contrast, the narrower confidence interval which cannot bracket the validations is also useless for unknown circumstances. As editor recognized, to

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probe why the confidence interval is narrower is important. However, few studies discuss how to evaluate the confidence interval. We have submitted a paper regarding this issue to other journal. In this revised version, we have more discussion on confidence interval (in 4.4). We described this method briefly and referred to our submitted manuscript.

5. The authors rightly noted in Section 4.4 (page 1116; lines 24-26) that the rating curve method may result in significant errors, especially in the cases of insufficient records and changes on channel characteristics. Considering these circumstances, in my opinion, the results on the peak level prediction don't look valid and I suggest removing these results from the paper.

As editor suggested, we eliminated section 4.4 in the original version.

6. Is Q_0 in Eq. 6 the same as Q_0 in Eq. 3? If yes, than the saturated discharge of interflow is the same as the discharge of base flow. For me, this assumption looks too rough. Please clarify.

This question was raised due to our unclear description. The two are not the same. In TOPMODEL, the definition of $Q_0 = Ae^{-\lambda}$ is the discharge when deficit variable equals zero. In this formula, λ is the average of topographic index (ln[a/(T_0 tanβ)]) within the whole watershed. In our study, the T_0 in the middle layer is the integral of K and D (from ground to D), and the T_0 in the bottom layer is the integral of K and the depth from D to infinite. To clarify it, we add a sentence *Note that Q_{i0} and Q_{b0} are different owing to different transmissivity but share the same K and D.* in P.9, Lines 7-8.

7. Figure 3c demonstrates that simulated flow is very slightly sensitive to changes in the surface roughness. This result looks rather unexpected and it would be perfect if the authors give some comments.

In our model, the surface roughness only affects the surface flow velocity but not alters the amount. Therefore, we added more descriptions. *Surface roughness (n) does*

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not alter the discharge volume and only affect the hydrograph shape in surface flow (Fig. 6a.1-4). This parameter determines surface flow's traveling time (or surface flow velocity). The smaller n values cause faster response and consequently sharper hydrograph. Each parameter has its own effects on EC and/or total discharge. (P. 16 Lines 6-10)

8. The conclusion that 1.0% of change in D, K, and mi may give 0.27, 0.20, and 0.15

Reviewer is correct. We removed the sentence in conclusion.

Technical Comments
(1) The abbreviation *cms* should be changed by m^3s^{-1}

(2) Y-axis in Fig. 6 should not include negative values

(3) Page 1106; line 11: SD should read S2

(4) Page 1115; line 3. The standard deviations are not shown in Table 3.

1. The unit was changed.

2. Corrected as suggested. Figure 6 is now Figure 7.

3. Corrected.

4. We added standard deviation in the Table 3 and

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