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Hydrology and Earth System Sciences Discussions

Interactive comment on "Assessing hydrological model behaviors by intercomparison of the simulated stream flow compositions: case study in a steep forest watershed in Taiwan" *by* J.-C. Huang et al.

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Referee #1 In this discussion paper an interesting link between hydrological model structures/parameterizations and responses (stream flow composition) are examined. Overall, this paper is well organized and the research strategy and conclusions are clearly understandable. The visual presentation of the results is good; however, there are still a lot of errors and unclear sentences in the text. The detailed and technical comments on this paper are addressed below:

We are glad that the reviewer understand the merits of our study and appreciated the constructive and detailed comments. In this revision, we rewrote the entire manuscript to avoid the unclear sentences. Below, we replied the comments point-by point. The responses in the revision were marked for easy tracking.

Detailed comments 1.section 2.1: As one of the conclusion you stated that the HBV model could be more suitable for catchments characterized by thin or highly permeable soils than the TOPMODEL (cfr. p.873 lines 17-19). Therefore it should be interesting to indicate in this section which soil types are dominant in the study catchment. This information could be linked to the calibration and validation results in sections 4.1 and 4.3, respectively, in order to check whether the assertion is correct for this catchment. Reply: Thanks for the advice. We added the soil description in the revised section 2-1. Two sentences and one reference were added to descript the soil background. 'Because of the steep slope, the majority of soils are colluvial soils (including greyish yellow and dark greyish) and lithosols with high permeability. The soil depth is various due to the frequent mass movements, but most depths vary from 40 -120 cm (Soil and Water Conservation Bureau, 1985)' in line 101-103.

2.p.859 line 13: Using the drainage area of 105 km2 (see line 4) an annual discharge of 1816.9 mm doesn't lead to a mean daily discharge of 7.94 m3s−1. Reply: Corrected. In this revision, we used the recent data (2000-2011) to describe the background. The revised sentence now is 'The annual discharge is approximately 2,129 mm (from 2000–2011 data) with a mean daily discharge of 7.09 m3/s (equivalent to 5.83mm/day).' in line: 111-114.

3.section 2.2.1 and 2.2.2: The evapotranspiration modules are turned out because it is assumed that this flux is negligible compared to the rainfall. This sounds very plausible in this study because of the high mean rainfall intensities in the selected events. To be complete I suggest to also giving quantitative information to indicate that this assumption is realistic. Reply: Yes, we added some quantitative information about evapotranspiration in the revised section 2.1. 'The annual evapotranspiration here is

estimated between 600-1200 mm and the daily evapotranspiration in summer may be as high as 6-8 mm (Water Resources Agency, 2011).' in line: 110-112. In section 2.2.1, we added the sentence 'The amount of evapotranspiration during rainstorms ($\sim$3 days) should be less than 24mm which is much small compared to the average of rainstorm precipitation ($\sim$ 430mm).' to quantitatively support our assumption. [in line: 142-145]

4.p.861 line 7: 'The soil moisture that received rainfall', do you mean the soil moisture status of the grid cells that receive rainfall or do you want to emphasize the temporal evolution of the soil moisture status within one grid cell? Reply: The original sentence was not clear enough. We want to emphasize the temporal evolution of the soil moisture status within the cell. The revised sentence now is 'The soil moisture status temporally evolves by receiving the rest of rainfall in each time step until reaching FC.' in line: 159-160.

5.eq. 4: Doesn't the second equation describe infiltration capacity based runoff, and not saturation excess runoff (cfr. line 2)? Reply: The sentences in this paragraph were not clear enough and one typo was in the equation 4 (now it is equation 5). We re-wrote the paragraph and added one reference to clarify the concept. In the equation, we set the upper limit, Td, to prevent the over infiltration, since the rainfall intensity is unusually high. Our previous application showed there was much rainfall infiltrated into the dry or near-ridge cell (high soil moisture deficit) too rapidly, because the rainfall intensity was so high. It resulted in an incredibly increase of averaged soil moisture. Besides, one geochemical study in New Zealand (Sklash et al., 1986) showed that the new water contribution in ridge top sites could reach up to 30-40% indicating the possibility of infiltration excess overland flow. Therefore, the parameter was designed to avoid the unreasonable increase of soil moisture. Although the second equation is somehow similar to infiltration capacity based runoff, it mainly followed the concept of saturation excess runoff. The paragraph now is re-written as 'In this equation, Di is only the condition to determine the rainfall converted to surface runoff. For the second equation, the maximum drainage capacity, Td, is the upper limit to avoid too much

rainfall infiltrated into dryer or near-ridge cell so rapidly, because the observational study in New Zealand revealed the larger contribution from new water in ridge top sites indicating the possible generation of infiltration excess runoff (Sklash et al., 1986). Although the second equation is somehow similar to infiltration capacity based runoff, it mainly followed the concept of saturation excess runoff in most cases.' in line 211-217.

6.p.865 line 14: Could you be more precise about the distributions used to sample the parameter sets. E.g. Could you provide the chosen intervals for the different parameters used in this study. Reply: We added a Table (Table 2) providing the unit, limit and distribution of parameters that used in the two models.

7.section 2.4: It is clear there does not exist a pareto front for the chosen calibration performance measures. Maybe it would have been interesting to incorporate an extra performance measure like the bias in the multi-objective calibration in order to distinguish between the well performing parameter sets, as considered in this study. Reply: Thanks for taking this issue into this study. In fact, we tried different combinations of performance measures. Some combinations actually showed the pareto front, but some did not. We understand that the selection of performance measures results in different performing parameter sets. This is an interesting issue in calibration and in parameter selections. However, we selected this combination, we wanted to address that the pareto front would not occur when the performance measures are inherently similar or the simulations have the similar tradeoff weight in the performance measures. We added the sentence, 'Notably, the pareto front may not exist when the performance measures are inherently similar or the simulation has the similar tradeoff weight between the performance measures. In this circumstance, all the simulations approach to a specific point.' [line: 285-288].

8.p.866 line 9: what is exactly meant by an awl shape? Reply: Replied in detailed comment #7.

9.fig. 4: Isn't the event with the smallest observed runoff volume in fig.4a.1 also characterised by rather low Nash EC's (cfr. p.867 line 26)? Quite often the performance of the TOPMODEL is not good for small rainstorms in fig.4b1. Therefore I would not conclude that TOPMODEL outperformed in the small rainstorms (cfr. 868 line 1). Reply: Yes, indeed. We rephrased the whole paragraph. For the small event simulation, the performance of the models are similar, but different in EQV and CC.. Now the sentence is 'In Fig. (4a.1) and 4(b.1), we found that the Nash_EC values of the 15 well-performed simulations for each event were quite diverse for the models, particularly in the small events.' [line: 316-317]

10.section 4.1: I would suggest to describe the modeling performance analysis of both models more in relative terms. E.g. instead of 'By contrast, the TOP-derived simulations held the run-off volume estimation well and remained consistent.' You could write 'the TOP-derived simulations estimated the run-off volume better and remained more consistent compared to the HBV model'. Reply: Thanks for the suggestion. We changed the sentence as you suggested in this revision.[line: 322-324]

11.p868 line 25: Real watershed responses do follow the mass balance. Could you be more precise about what you really want to indicate. Reply: Sorry for the incomplete sentence. We want to indicate that many watersheds may not follow the mass balance, but it's the basic assumption in many hydrological models. We rephrased the sentence as 'many watersheds may not follow the mass balance, but it has been the basic assumption in many hydrological models. Therefore, the water mass balance assumption may need other environmental backgrounds to support.' in line: 338-340.

12.p869 line 6-7: What is exactly meant by 'the ranges of parameters Srmax, Ks, L and Kb are limited in revealing the importance and sensitivity of these parameters in the HBV model'? Reply: The sentence was not clear enough. Now the sentence is rephrased to 'For HBV, once the parameters, Sramx, Ks, L, and Kb were fixed or determined; the similar simulations can be obtained.' [line: 346-347].

Technical comments 1.Incorrect sentence structures are not uncommon in this paper. This issue should be thoroughly addressed before considering publication. e.g.: âĂć p.856 lines 17-18 Rephrased. The new sentence is 'We suggested that a proper model selection should take the implicit environmental background into account for simulating reliable streamflow composition.' [line: 39-40].

âĂć p.856 line 8 Rephrased. The sentence is 'Results showed that both models gave satisfactory streamflow simulation in terms of the Nash efficiency coefficient, correlation coefficient, and discharge volume.' [line: 32-34]. âĂć p.858 line 25 Rephrased. The new sentence is 'This study improved our understanding on model selection and the role of the parameters in streamflow composition,' [Line: 92-93].

âĂć p.859 lines 8-9 The sentence is rephrased to 'The original and secondary forests covering nearly 87% of the area are the dominant land cover in this watershed. Most agricultural lands (e.g., orchard and vegetable farms) locate along the road or the riparian zone.' [line: 105-107].

âĂć p.859 lines 18-19 Rephrased. The new sentence is 'The total rainfalls varied from 184.5 mm to 836.4 mm and the maximum rainfall intensity ranged from 10.7 mm/h to 39.5 mm/h.' [line: 116-117]

âĂć p.860 lines 16-17 Changed to 'Although the elevation in the study site is high (> 3,000m), very little or no snow appears in subtropical summer' [line: 141-142]

âĂć p.861 lines 14-16 Eliminated the last phrase. The sentence now is 'When the water level in the upper reservoir exceeds the threshold value (L), surface runoff (Qs) occurs.' [line: 164].

âĂć p.862 lines 8-9 Eliminated the 'and' in the original sentence.

âĂć p.864 lines 19-20 Corrected the typo 'measured' to 'measures'.

âĂć p.868 lines 13 The whole paragraph was rephrased.

âĂć p.869 lines 11-15 The paragraph is rephrased as ‘It seems that more than one type of parameter combinations can achieve similar performance which indicates that more than one type of streamflow compositions can be obtained. In this regard, a model which gives the more types of parameter combinations with similar performance has high flexibility.’ [line: 350-353]

2.p.858 line 9: ’Hydrologiska byrans vattenbalansavdelning’ instead of ’Hydrologiskabyransvattenbalansavdelning’ Corrected.

3.p.858 line 17: ’to calibrate the parameter sets’ instead of ’to calibrate the well performed parameter sets’. Corrected.

4.p.858 line 19: ’in terms of the Nash efficiency coefficient’ instead of ’in terms of efficiency coefficient’. Corrected.

5.p.859 line 10: ’varies with distinct seasonality’ should refer to the ’annual precipitation’ and not the ’average annual precipitation’. Rephrased. The sentence is replaced by ‘The annual precipitation is as high as 2,551.1 mm (based on 2000–2011 data) with distinct seasonality.’ [line: 108-109].

6.p.859 line 22: It is probably better to consequently use the same units for a certain variable (cfr. line 13). We used the same unit for discharge [line 119].

7.eq. 1: SM and FC should be put in italics. Corrected.

8.fig 2: I suggest to alter the left panel. For the moment it cannot be deduced from the figure that Qi is also modelled as a linear reservoir output. In this revision, the runoffs modeled by linear reservoir were represented by bold and italic symbol.

9.p.862 line 2: I would recommend to always use the same variable names for Ks, Ki and Kb (cfr. p.861 lines 20-21) Corrected. We used the same name ‘recession coefficient’ in the manuscript.

10.p.862 line 5: ’L2’ instead of ’L2’, ’discharge’ instead of ’runoff’. Corrected.

11.p.862 line 10: It would be better to give the exact equation of the topographic index like in p. 863 line 11 instead of giving a brief description that is not complete in the context of the TOPMODEL. As reviewer suggested, we added equation 4 to show the calculation of topographic index.

12.p.862 line 19-24: The units of the variables are missing in this part. Corrected. We added the unit of the variables.

13.p.862 line 14, 22, 24: To avoid confusion it would be better to consistently use one term for the first layer: ’upper layer’ (cfr. line 14), ’root zone’ (cfr. line 24), ’root zone storage’ (cfr. line 22). Thanks for the reminder. Now we used upper layer to keep the consistency.

14.p.863 line 1: The units for variable Td should be added. Corrected.

15.p.863 line 13: ’a’ should be ’_’. Corrected in equation 5, because ‘a’ is more common in the literatures.

16.p.863 line 17: ’flow’ instead of ’runoff’. Corrected.

17. eq. 4: The condition of the second equation should be reversed Sorry for the mistake. We corrected it.

18.p.864 line 1-2: I suggest using the previously used terminology for Sb and Kb. We rephrased it and used the same terminology.

19.p.865 line 17: ’selected for’ instead of ’selectedfor’ Corrected.

20.eq. 8: NashEC should be in italics. Qsim,i in the denominator should become Qobs,i. Checked. In our document, we didn’t have this typo. Nevertheless, we confirmed it again.

21.p.865 line 21: ’the total of time steps’ instead of ’the total time step’ Corrected.

22.p.870 lines 15: ’average soil deficit’ instead of ’average soil deficit decreases’ Cor-

rected.

23.fig. 10, 11: In the figure labels events 21 and 23 are mentioned. Shouldn't these be 15 and 17? Corrected. There are event no. 15 and 17.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/10/C1023/2013/hessd-10-C1023-2013-supplement.pdf

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