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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A simple topography-driven and calibration-free runoff generation module" *by* Hongkai Gao et al.

Anonymous Referee #2

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This study proposed the HAND-based storage capacity curve (HSC) for runoff generation parameterization in hydrological models. I like the idea to provide parameter-reduced modules for hydrological modeling, considering the significant uncertainty in parameter calibration. However, the benefits for reducing parameter uncertainty by the HSC module were not illustrated by the current results. The authors claimed that the HSC/HSC-MCT module possess higher robustness and bear the potential to be implemented in prediction of ungauged basins, which however are not convincing from the current results. The benefits of the HSC/HSC-MCT modules need to be further discussed. First, the HSC module obviously overestimated the saturated area fraction in the Bruntland Burn (BB) basin. The improvement for the correlation from 0.5 (TOPMODEL) to 0.6 (HSC) is rather small, which does not make any sense for illustrating

the reduced deviations between the observed and simulated saturated area fractions. The model performance in validation period were not evaluated in the BB basin. Although the HSC module gained performance improvements on high flows in both calibration and validation periods in many MOPEX basins, the performance gains on low flows were not investigated. Moreover, the non-parameter HSC-MCT module produced much lower performance than the HSC module in many MOPEX cases. The gains for the HSC module should be attributed to the parameter calibration procedure, and potentially demonstrated the failure of the MCT module for many MOPEX cases. Second, it is not fair to call the proposed modules are calibration-free. The HSC module also implied two parameters for the model application. The stream initiation threshold area was not included for the model calibration, but was tested to calculate the HAND values. I guess this threshold area should be tested in calculation experiments to prepare the model results using the HSC modules. The effects of this parameter on the performance of the HSC modules were not investigated in the results. Including this parameter in the calibration procedure would most likely to improve the model performance. Moreover, model calibration procedures are required to determine the remaining parameter values in Table 1 for both the applications of HSC and HSC-MCT modules. The benefits to reduce parameter uncertainty by excluding one-two parameters by the HSC and HSC-MCT modules were not clear. Considering the preparation of HAND values using DEM dataset in the HSC and HSC-MCT modules, the computational cost should be much higher than the calibrated modules in HBV and TOPMODEL. Some other major concerns on the results are listed as follow. 1. Figure 6, why not show the saturated area fraction simulated by the HBV module? Is the HBV module spatially discretized for the model application? 2. Figure 7, why was the beta value of 0.98 used for the HBV module? Was this beta value derived from the model calibration? Have you tried other beta values for the comparison between HBV and HSC modules? Why not show this curve for the TOPMODEL module? What is your purpose to show the frequency of TWI? Could you also show the frequency of HAND? 3. Figure 8, the label for soil moisture was missed. It is very difficult to find the observed soil moisture

(or you don't have?). Can you label (a-b) for the two subplots? For the second and fourth events, the TOPMODEL matched the observed saturated area fractions very well. How to explain this? Please refine the caption, which is very difficult to understand. 4. Figure 9, as I suggested before, a correlation coefficient does not make any sense to illustrate the deviations between the observed and simulated saturated area fractions, given that only seven observation events. Could you use some other metrics to compare the bias or deviation errors between the observed and simulated saturated area fractions? 5. Figures 10-11, what do you intend to say from these figures? 6. Figure 12, I suggest to compare the values for IKGL as well. The models were calibrated on both IKGL and IKGE, there should be strong trade-off between these two objective functions. That means the HSC module possibly sacrificed the performance for low flows (IKGL) to improve the performance for high flows (IKGE). The evaluated modules mainly differed on the calculation of soil storage capacity, which has significant effect on the generation of low flows. In my opinion, performance for low flows should also be an important indicator for the validity of the runoff generation assumptions. Minor concerns: 1. I suggest to remove "Calibration-free" from the title. HSC module needs to be calibrated, and the HSC-MCT module performed poorly in many MOPEX cases. 2. Lines 30-33, I am not convinced to agree with this from the current results. What do you mean "facilitated effective visualization of the saturated area"? Is it important? 3. Introduction is too long from my taste. It is very difficult to get the motivations of this study from this section. I would suggest to refine it. 4. Lines 213-214, remove "Hydrological. . . inevitable". Line 217, what do you mean "HAND contours are parallel in runoff generation"? Is that possible derived from the DEM? 5. Line 227, could you please add more details for the calculation of HAND values? 6. Line 314, how did you define the pareto-frontier? Did you use the Euclidean distance or threshold values? 7. Section 3.1, could you please add some details on the climatic and hydrological data in the BB basin? Any ground gauged stations do you have there? 8. Also in section 3.1, could you introduce the spatial interpolation of the field mapping of the saturated areas? 9. Line 369-374, move to the methodology section. 10. Lines 415-420, move

to the methodology section. 11. Lines 455-456, 'dramatically improved' may be not fair. 'simultaneously maintaining model robustness and consistency' is also not convinced by the results. 12. Lines 491-496, it is not fair to only discuss the cases where HSC/HSC-MCT outperformed the benchmark modules. Why not discuss the reasons for the cases where HSC/HSC-MCT produced lower performance? 13. Discussion is also too much. It is difficult to get the main messages from the long text. Maybe remove lines 508-522, and lines 539-556. 14. Lines 646-647, maybe it is not so important to say as one of the conclusions here. 15. There are many sentence started with 'And', this is very strange (kind of grammatical error).

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