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Hydrology and Earth System Sciences Discussions

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

J.-C. Huang et al.

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General Comments:

1.General Comments: This paper describes and tests a version of TOPMODEL (or STOPMODEL) that incorporates interflow in a novel way. The model is tested against storm hydrographs associated with typhoon events in a watershed in Taiwan. Perhaps the most innovative part of this work is uniquely linking interflow with baseflow (Eq. 3), a concept that appears to build nicely on work by Peter Troch et al. (1993 Water Resources Research 29(2), 427-434) [which should be cited in this paper]. Troch et al. (1993) suggest that baseflow is an indicator of the landscape8217;s antecedent wetness, which plausibly also indicates the magnitude of interflow as suggested by

Haung et al. In essence, this paper more fully develops the theoretical work put forth by Walter et al. (2002) and Scanlon et al. (2000) to make TOPMODEL more realistically incorporate interflow. Conceptually, this paper is perhaps the most interesting such work to date.

We appreciate the comments and encouragement. We have more discussions and added suggested papers in References.

2. Its major shortcoming is in testing the model against typhoon-initiated runoff events. These events are very intense and it is not obvious that variable source area (saturation excess) runoff is the primary process; e.g., Hortonian flow may be important for these types of events. At the very least the authors should compare their rainfall intensities to saturated soil hydraulic conductivities (e.g., Walter et al. 2003, ASCE J. Hydrologic Engineering 8(4): 214-218) to convince the readers that they have chosen a mechanistically appropriate model.

Reviewer's comment is useful and this reference is cited. We compared the rainfall intensities with our calibrated saturated hydraulic conductivity. More discussions can be seen from P. 13 Line 23 to P.14, Line 5.

3. Additionally, extremely large events are generally the easiest to simulate because most of the precipitation generates runoff. I speculate that even a very simple rainfall-runoff model like the so-called "curve number" or "rational method" would capture these events similarly well (regardless of the actual underpinning hydrological mechanisms). Scanlon et al. (2000) were unable to substantially improve hydrograph predictions with their interflow-version of TOPMODEL (I think Walter et al. 2002 did not even try) and it is unclear that this proposed version of TOPMODEL improves hydrograph predictability any better. It would be good to show whether or not the more complicated 3-Layer TOPMODEL (or STOPMODEL) performs any better than the more basic versions.

We fully agreed with Reviewer. In hydrological modeling, various models with different hydrological mechanisms or structures can perform well, particularly, after proper cali-

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bration. In most cases, we can not determine and validate the hydrological processes inside the entire watershed because the discharge is the convolution of rainfall and landscape. This is a challenging problem. We proposed 3-layer TOPMODEL is mainly because it is more realistic basing our field observations. Also many previous geochemical tracer studies have shown that the stream discharge is composed of three components. Recently, we carry out an intensive sampling during typhoons in central Taiwan. Preliminary result also likely supports the three components structure (P.14, Lines 2-5). We had compared this 3-layer TOPMODEL with the original version. The 3-layer TOPMODEL just performed a little bit better than the original version.

Minor points

1. In Eq. 1, presumably S_1 is always less than or equal to S_{1max} I think in the original TOPMODEL the storage could exceed the available storage with the excess going to overland flow. Somehow this detail needs to be explained here.

The S_1 can exceed the available storage and then form the overland flow or infiltration. We clarified this in Eq. 1.

2. Is it sufficient to test a distributed hydrologic model against watershed discharge only? Is there any evidence that internal water distributions or fluxes are correct? See Steenhuis et al. (1999, Water Resources Research), which shows that models based on different conceptual processes can give essentially the same integrated outputs. I believe Keith Beven has also written extensively on this topic. This comment relates back to my general comments regarding matching the model with the appropriate, actual, physical mechanisms.

We agreed with Reviewer's comment. It's not sufficient to validate the hydrological model structure only by discharge. We also recognized that different compositions can generate the similar stream discharge, so called "equifinality". We have more discussions (P.13, Lines 18-23).

3. Can any of the eight or so global variables be determined a priori or is this essentially

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a purely fitted model?

This is the first study to introduce the applicability of the 3-layer TOPMODEL in sub-tropical climate. Those parameters were calibrated (fitted purely), because we focused on the model applicability and parameter sensitivity in this study.

4. It is a little strange to see the sensitivity analysis before the comparison between observed and modeled streamflow. Perhaps switch these two parts of the manuscript.

We moved the sensitivity section backward as suggested.

5. On pages 1106-1107, the authors note that they do not calculate evaporation for storm events because "the vapor pressure is almost saturated." I agree that evaporation (and interception for that matter) can be neglected during most large rain events but not for this reason. Evaporation is driven by the difference between vapor pressure at an evaporating surface and the vapor pressure of the overlying air mass, i.e., even when the air is "saturated," evaporation will occur if the evaporating surface is at a high enough vapor pressure. For example, we can boil (evaporate) tea-water when the air is at the saturated vapor pressure. Please omit this misleading and physically incorrect sentence... this common misconception is a "pet-peeve" of mine.

This misleading sentence was modified to *For event-based simulations, the potential evapotranspiration estimation is neglected due to the much lower proportion between evapotranspiration to rainfall.* (P6, Lines 15-16).

6. Some of the English is a little awkward, albeit very understandable; perhaps have this manuscript edited for a little smoother or better readability.

As reviewer suggested, this revised manuscript has been polished by a native speaker.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 1101, 2008.