

## ***Interactive comment on* “Theoretical investigation of process controls upon flood frequency: role of thresholds” by I. Struthers and M. Sivapalan**

**Anonymous Referee #1**

Received and published: 7 November 2006

In this paper, the authors try to develop a theoretical interpretation of flood frequency curves. They do this through the use of a very simple rainfall-runoff model, in which they switch on and off a number of processes. The paper is well written. Although I like the idea of such a study, the paper has a number of serious shortcomings, which, in my opinion, render it, at this point, not suitable for publication. Some major revisions are thus needed.

- My first problem with the paper is that it has never been demonstrated that the model really can reproduce measured discharge rates. This is the major shortcoming of the paper. To me, it does not make sense to do a modeling study with a model that does not work well. A first step in this type of study should be a calibration and validation

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of the model using observed precipitation and discharge values. If it is demonstrated that the rainfall-runoff processes are well modeled for a certain study site (I am not convinced about this), and that the precipitation generator can reproduce the statistics of the observed precipitation (I am not sure about this either), then the obtained parameters could have been used in the sensitivity study using the generated precipitation time series. The conclusions drawn in the paper might very well be dependent on parameter combinations, in other words, under different parameter combinations the flood frequency curve might look different, and the obtained regions may no longer be as clearly distinguishable. A sensitivity study using a calibrated model under different climatic, topographic, pedologic, and vegetative conditions could then reveal whether there is a general shape of the flood frequency curve.

- The second problem I have with the paper is the choice of model. The conceptual model used is a model type made for short-term rainfall-runoff modeling, not for long-term simulations. The processes represented in the model are oversimplified, for example, we know that surface runoff can occur at catchment storages below the storage capacity, while the model assumes it does not. Similar remarks can be made about the modeling of the baseflow, the infiltration, and the evapotranspiration. This has the consequence that a number of the results are artifacts of the model structure and are not the results of true physics. For example, in in Figure 4a, it is obvious that a reservoir with a low storage capacity will fill up quicker than a reservoir with a high storage capacity. Taking into account that surface runoff only happens when the storage is equal to the storage capacity, the flood frequency curve will approach the precipitation curve at lower return periods for smaller storage capacities. However, if surface runoff would be modeled more realistically, the discontinuity in the graphs in Figure 4a might not be as clearly distinguishable. This issue has also more or less been discussed by the authors in section 3.8, but here again the results in Figure 7b can perfectly be explained by the a priori assumed representation of the spatial variability in storage capacity, which is not necessary realistic. It is my opinion that the results are not necessarily a reflection of true catchment physics. What should really

have been used is a physical model, in which a number of processes could have been shut off (for example stomatal resistances could have been set to a very large number to switch off evapotranspiration or the soil could have been assumed extremely deep to mimic a storage capacity of infinity). I think that some different and more realistic results could have been obtained this way.

- The title is not consistent with the content of the paper. The title reads "Theoretical investigation of process controls ...". The authors are not developing any new theories, their approach is conceptual model-based. Therefore, the title should be "A conceptual model-based investigation of process controls ..." or something similar.

- It is also unclear how the evapotranspiration used in the model simulations has been generated. Taking into account that this is an extremely important process (two thirds or more of the water balance in most places) it should not be treated lightly. A number of conclusions drawn in the paper could very well be dependent on this value, so we really need more information about this.

Some minor comments:

- After Equation 15 it should be explained what  $\omega_h$  is. - It should be stated crystal clear that the simulations are performed at an hourly time step.

In light of all these comments, I do, at this point, not recommend this paper for publication. The paper could be suitable for publication after some major revisions have been done. My suggestion is that the authors take into account the remarks (especially the model choice and the calibration/validation issue) in this study, which could then result in a good paper.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 3279, 2006.

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