Hydrol. Earth Syst. Sci. Discuss., 7, C3695–C3698, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C3695/2010/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Large-scale runoff generation – parsimonious parameterisation using high-resolution topography" *by* L. Gong et al.

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Received and published: 25 November 2010

We appreciate your positive comments on our manuscript. Below we first reply to your general overview:

We agree that in the manuscript we have dealt with both theoretical derivation of topography-based storage capacity distribution curve, and also the insertion of such distribution curve into a VIC-type parameterisation framework and, application in selected large river basins. We are also fully aware that the similarity between VIC and TOPMODEL has been recognised by several previous works. We have, in fact developed our algorithm based on one of them (Sivapalan et al 1995), and we also cited work from Kavetski et al 2003. The major differences between our work and previ-

ous ones are, we have not attempted to parameterise the storage capacity distribution, but instead to derive the distribution solely based on high-resolution topography data; and we have implemented this concept in a complete macro-scale hydrological model which has the potential to be used at global scale. We found this comment particularly interesting: that climate variability could be one influencing factor of the relationship between geomorphology and catchment response. In the manuscript this idea is reflected in the definition of "driest condition", which is usually set by climate. An interesting further study could be made, to investigate the spatial and temporal variation of climate on the sensitivity of the tail of the storage capacity distribution. However, since this manuscript has already addressed enough aspects, we feel it is better to make this investigation in a separate paper. The reviewer also mentioned that the trimmed distribution looks very similar to the VIC infiltration capacity curve. As a reply to this comment, with the left tail remains and the right tail trimmed, the storage capacity curve could be simplified as a VIC infiltration curve. However, our result showed that it is important to trim the left tail and to remain information in the right tail, which makes the resulting distribution curve different from VIC. The reviewer pointed out from Fig. 7-9, it can be seen that although VIC infiltration curve and the TRG distribution curve differs in their shape, the performance of the model are very similar. We think this is a result of equifinality. In Fig. 7 and Fig. 9, we only choosed one instance of parameter m for the TRG method and one pair of im and B for VIC model that gives acceptable model result. Looking at Fig 8, it is obvious that m parameter is well defined but a compensation effect (equifinility) exists between im and B. Often in hydrological applications the final lumped efficiency measure can not indicate the actual model performance in term of reproducing correctly not only discharge but also the state variables...

and now we reply to your specific comments:

1, About the interpretation of the parameter m. The reviewed questioned parameter in Equation 3 as representing transmissivity. As stated in the manuscript, m in equation 3 represents the shape of exponential decay transmissivity profile, but not the transmissivity itself. In fact, by assuming a constant saturation transmissivity, our model does not deal explicitly with transmissivity. In the manuscript we did an extended derivation based on the original TOPMODEL equation that links local storage deficit to catchment average deficit by topographic index. So the parameter m remains the same physical explanation as in TOPMODEL. The difference with TOPMODEL is, 1) we have not linked the exponential delay transmissivity profile to groundwater recharge rate 2) in TOPMODEL there is no explicit definition of a maximum storage capacity, but in our extended derivation we related the maximum storage capacity to parameter m and the critical topographic index value which define the river channels. Physically, the transmissivity profile does define the effective storage capacity of the catchment, i.e., a catchment with shapely declining transmissivity profile would generally have less storage capacity compared with catchments that have slower decline. We fixed parameter m spatially constant because at the scale we are dealing with, even at much smaller scales, there is usually no consistent measurement to derive m. parameter m can be derived either directly from the transmissivity profile, or from recession analysis. Because neither of them is available in a spatially distributed form, the parameter m has to be calibrated as in TOPMODEL. Of course, one may argue that even if m is a calibrated parameter, it could be allowed to vary from cell to cell, or from sub-basin to sub-basin. We have, in fact done such test to allow spatial variability of the model parameters in general, however we discovered unsatisfactory result because in general model parameters have their own spatial autocorrelation, but it is very hard to take this into account. On the other hand, we found it computationally impractical to deal with spatially distributed parameters. Also, we found it is less important to allow m to be distributed, and it is more important to let the storage capacity, and the distribution of storage capacity vary between cells.

2, We fully agree that our modelling concept is based on saturation excess, and did not take into account the Hortonian infiltration excess overland flow. The reason for this is saturation excess is the dominant runoff generation mechanism, and Hortonian flow only generated occasionally and usually is spatially and temporally localised. Previous

studies have shown that infiltration excess runoff can be integrated into TOPMODEL framework for macro-scale hydrological modelling. For example, Famiglietti and Wood (1991) in their paper has allowed infiltration excess flow to happen when local infiltration capacity is exceeded by the precipitation intensity.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 6613, 2010.