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Hydrology and Earth System Sciences Discussions

Interactive comment on "HESS Opinions "Topography driven conceptual modelling (FLEX-Topo)"" *by* H. H. G. Savenije

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Reply to Referee # 2

About the general comments

First of all I would like to thank the referee for the detailed reading and valuable suggestions for improvement of the paper. In general I agree with most of the observations made and these will be taken into account in the final paper. More in general, the referee raises two issues: The general applicability of the method and the scientific evaluation.

On the scientific evaluation we have already given a detailed response to referee # 1.

I shall not repeat the details here. In short, we shall apply a crash test approach, as suggested by Klemes (1986) and reiterated by Andreassian et al. (2009), and we shall use a number of benchmark models for comparison. In general our aim is not to look for a perfect model, but for a model structure that performs better than other hypotheses in terms of: performance (multi-criteria), realism and predictive uncertainty. We are first going to test the method in Luxembourg where we have detailed DEM's and a relatively dense network of hydrological observations, and subsequently we are going to expand the method to the Moselle and the Meuse. Of course we are also thinking of applying it to other climatic zones, such as the Blue Nile Basin and the Zambezi. The research is primarily meant to enhance our understanding of how landscapes work and much less to test if the concept of topography-based conceptual modelling is a correct one.

Regarding the general applicability, the referee is right to say that the description made is not meant for cold regions and does not yet include the human factor. But interpreting the concept more widely (in the way it was intended) the concept can be used to cold regions as well. Of course it would require other model structures, but it may still be quite relevant to make use of topographical properties to select certain model structures in a flexible modelling framework. In the final paper an observation will be made to address this issue.

About the specific points

Many of the specific points relate to overstatements, corrections or the need to clarify the points made. All of the points mentioned make sense and will be addressed in the final paper. The question whether simple models work because of "emerging patterns of self-organisation", or that it could be because "catchments are open dissipative systems constrained by mass balance" is not yet possible to answer. Fact is that the solutions of the basic balance equations allow a much wider solution space than the range of "behavioural" outcomes. The joint evolution of landscape (evolved as a result of energy dissipation), ecosystem (evolved in tune with climatic, hydrological, and geological and topographical forcing) and subsurface drainage system (evolved as

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a result of energy dissipation in tune with ecosystem development) leads to a sub-set of possible realisations, which one could call "behavioural" and which are the result of "self-organisation". If this is what the referee means by "open dissipative systems constrained by the mass balance", then we mean the same thing. The difference is maybe the time element (the evolution). A new constructed catchment, for instance a bull-dozed artificial hillslope, is probably also an "open dissipative systems constrained by the mass balance", but it is probably not a "behavioural" catchment since it has not yet developed a subsurface drainage and retention structure resulting from the interaction between ecosystem development and erosion processes. This thinking also agrees with your observation about the REW. The REW concept is very much like a "bull-dozed" catchment where a local property is upscaled to account for the entire watershed, without explicitly accounting for heterogeneity. Of course, when an REW model is calibrated to data, then the parameters obtained will reflect representative values that implicitly account for heterogeneity, but this reduces the predictive capacity of the approach.

Finally, I am grateful for the suggested reference of Winter (2001). The diagrams are good illustrations of the plateau-hillslope-wetland interaction and also show how the toe of the hillslope can possibly intercept groundwater.

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