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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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I thank the Luxembourg group for reacting to my invitation to comment on the paper. They raised four issues that require more attention.

1. About the previous literature dealing with topography.

This opinion paper is not a review paper, so I don't plan to be exhaustive in mentioning all papers that have dealt with topography. However, I do need to mention some land mark papers, Beven and Kirkby (1979) certainly being one of them. What I want to avoid is going into depth about past approaches. What I find astonishing is that previous topography-based approaches used primarily elevation, aspect and slope, but

not the height above the nearest drain (HAND), which is the most obvious topographic parameter that we plot on the vertical axis when we make a sketch of a cross-section over a stream, as e.g. in the REW concept. The index that comes closest to HAND is the topographic index of Beven and Kirkby. However this index assumes that the groundwater saturation that triggers Saturation excess Overland Flow (SOF) is fed by the sub-surface flow from the hillslope. This is not everywhere a correct assumption. In the hillslopes of the Meuse catchment, the hillslope runoff process is Saturation excess Subsurface Flow (SSF) which does not feed the groundwater, but rather finds a shortcut route towards the stream. Although the underlying mechanism for the topographic index is probably appropriate for the catchments in which this concept was developed, it has certainly no general validity or validity beyond the specific conditions where sub-surface flow feeds the groundwater before fast runoff occurs.

2. About the definition of the terms.

The commenter argues that I confuse topographic and ecological terms to indicate the three dominant hydrological landscapes. This is of course not the intention. The three terms I use reflect hydro-ecological zones, or hydrological landscapes that correspond to a dominant runoff mechanism. Because the landscapes in question are the result of a joint evolution process involving: the geo-morphological processes, the ecological processes and the hydrological processes, there is nothing against using terms that have an ecological connotation, as long as they clearly indicate the dominant hydrological process. The term 'wetland' is, I think, very appropriate to indicate the SOF mechanism. A wetland is a landscape element where the groundwater (perched or not) is close to the surface and where the soil get's saturated during intensive rainfall events, leading to saturation overland flow. The term wetland is, as a result, very appropriate. Wetlands can be found on top of mountains, as long as open water is near, as well as in river valleys, where open water is near. As the commenter mentions, a wetland can both be located in a floodplain and on a hill (which we associate with the term plateau), as long as the groundwater is close to the surface. In that regard the term plateau is less clear than the term wetland. In my definition, the distinction between plateau and wetland is not the elevation itself, but the height above the nearest open water. It is the depth to the groundwater which makes the distinction between plateau and wetland, not the elevation. Maybe a better name for plateau would be 'deep groundwater landscape'. But since the HAND approach uses the term plateau, I think it is better to stick with it. In the paper I shall clarify what is meant by it.

3. About the scale question.

I am not sure if the main message intended in the paper came across sufficiently well. The proposed methodology is not a proposal for a new conceptual model. It is a modelling approach rather than a fixed model structure. The whole idea is to develop conceptual model structures in response to the model purpose and according to the dominant mechanisms that belong to the climatic, morphologic, ecologic and land-use setting of the basin under study. It is a flexible modelling framework where each model structure can be hypothesised, of which the appropriateness needs to be tested. This is nothing new, and is fully in line with the flexible modelling approach that Fabrizio Fenicia and I (Fenicia et al, 2008a, 2008b, 2010) have advocated for some time. What is new, is that we can use a new topographical indicator (HAND) to distinguish between different hydrological landscapes. For every climate, ecosystem, land-use system and morphological setting this may be different. Regarding the scale of the HAND approach itself, Rennó et al. (2008) already indicated that it works at different scales. A recent unpublished paper I saw from that group demonstrated that it functions well from the small catchment scale to the large river basin scale. Of course we also need to test this out in different catchments.

4. About connectivity.

The question whether the three hydrological landscape systems mentioned in the paper function in parallel or in series is an important issue to raise. For example, the topographic index mentioned above assumes that the hillslope feeds the wetland system,

after which the surface area of the wetland expands, so that the area of SOF becomes larger. This connectivity is probably correct in the catchments where this approach was developed, but not in the Meuse catchment. I know that there is also connectivity in the Zambezi, which consists mostly of Kalahari sands. There the recharge from the plateaus and the hillslopes feeds the groundwater, as a result of which the drainage system expands (Winsemius et al., 2006). The point is that depending on the local conditions a different conceptual model structure needs to be tested. In the Meuse catchment, the hillslopes discharge directly to the stream, without passing through the groundwater system. The plateaus feed the groundwater which discharges directly to the stream. There may be some interception in the intersection with the hillslope, but this is probably minor. One can also safely assume that the recharge on the wetland itself raises the groundwater level to saturation earlier than the groundwater flow from the plateau, where there is probably a longer delay between rainfall and recharge. So in my example the processes are not connected but act in parallel. Only the groundwater reservoir itself, I think, is more likely to be lumped, so that plateau, hillslope and wetland feed the same groundwater reservoir and jointly are responsible for the recession of the hydrograph. So in summary, the fact that in the proposed model structure the elements are not connected may be one of the key raisons why this model structure is conceptually better. Of course it still needs to be tested to be able to draw this conclusion, but for the time being my hypothesis is that the dominant processes act in parallel.

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