

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

Anonymous Referee #2

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GENERAL COMMENTS

I applaud the author’s courage and his willingness to attempt generalisations. I think the idea of using classification to guide model structure selection is a very useful one.

The paper is written as though the proposed method has very generic applicability, when in fact it omits some very significant aspects of hydrology. For example, it does not address cold region hydrological processes, and it does not address the role of humans as water movers. To address this, there needs to be a brief acknowledgement of these types of limitations in the paper.

I would like to see the paper lay out a plan for scientific evaluation of the proposed

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modelling approach. How would the modelling approach be tested? Which fluxes and state variables should have a correspondence with observations? Against what reference would the results be judged? In which locations or environments should it be tested? What would qualify as a success?

SPECIFIC POINTS

4637 line 2 “This approach is a dead-end street” I consider this point is overstated. The reductionist approach leads to new knowledge when applied at appropriate scales, but is not useful when used at the wrong scales. The author did not state in his introduction which scales were of interest. Hydrological processes at the small scale are still hydrological processes, even if they are not of relevance to the author’s goals which are at landscape scales.

4637 Line 10 “Simple catchment-scale models apparently make use of emerging patterns of self-organisation implicit in naturally formed catchments and river basins.” How do we know that is the reason for their responses? One could equally say that catchments are open dissipative systems whose responses are constrained by mass conservation.

4638 Line 3-5 “The reductionist view of the ant, who observes physical processes at a microscopic scale, does not lead to predictive equations at the relevant scale of the catchment” I suggest that the REW concept is designed to produce precisely these equations, starting from a reductionist description. However, in my view the REW approach has not yet done so in a way that takes account of real heterogeneity.

4638 Line 9 I suggest that the paper cites and briefly describes Winter (2001), at about this point.

4638 lines 17 and 25 Generalised statements such as “the dominant land use of hill slopes is forest” “Floods are generated on hill slopes” are presented as fact, but without reference to published literature; this may be expected in an opinion piece, but still the

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reader needs to take care!

4639 line 4-5 “This groundwater partly ends up in the river as base flow but is also intercepted by the forests on the hill slope” Really? That must be very shallow groundwater, which is surprising to me, for a steep hillslope.

4639 line 8-9 “The floods, however, and most of the runoff dynamics are generated on the hill slopes, and these are mostly forested (sometimes planted with fruit trees or vineyards).” There’s an interesting competition of controls here, because the presence of forests tends to reduce flood magnitude, other things being equal, while steeper slopes tend to increase flood magnitude.

4639 line 14 “The riparian zones . . . are not the largest contributor to flood volumes” Again, this is a very definite statement, made without citing evidence, or limiting it to particular environments.

4639 line 16 “For a forest ecosystem . . .” This could be the start of a new section.

4639 line 19 “. . . it does not erode the foundation of the ecosystem” by foundation does the author mean the soil?

4639 line 21 “. . . the most efficient mechanism . . .” I would say “an efficient mechanism”

4640 line 5 “hillslopes tend to behave similarly all over the world” It would be useful to cite a reference or two for this.

4640 line 7 “subsurface drainage is the dominant feature” Please amplify. Dominant relative to what other features?

4641 “the same applies to all other intensively inhabited regions of the world” Seems a very strong generalisation. Perhaps replace “all” by “many”?

4642 Line 11 “probably Hortonian overland flow” perhaps cite Tom Dunne’s work at this point?

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4642 Line 13 Some aspects of geology play a significant role in determining flow regime, and this should be directly stated.

4643 Line 6 This paragraph does not mention soil hydraulic properties. These properties do vary somewhat in relation to topography, but there are also significant hydrological features of soils which are not topographically prescribed.

4644 Line 10 It would be useful to cite Winter (2001) here too.

4644 Line 25 “the link to conceptual modelling is a new approach” This statement surprises me, but I can’t immediately think of a counterexample.

4645 Line 12 “interception threshold D_w ” Is this a vegetation property or a combined climate/vegetation property? Are its units L or L/T?

4645 Line 13 “beta function” I do not see a beta function anywhere.

4645 Line 21 “the model is not very sensitive” Which component of which model is referred to here?

4645 Line 26 “short peaks” should perhaps be “small peaks”

4646 Line 7 “if runoff records are available” This seems to be a serious limitation; some more discussion of how to use the method without runoff records would be useful.

Line 12-13 “the parameters can’t be calibrated” Perhaps they could be calibrated from soil moisture data or groundwater levels?

4647 Line 18-21 Would the use of this model in a spatially distributed manner lead to an explosion in the number of parameters to be estimated?

4648 Line 2 “geology, geomorphology, . . .” I would add climate to this list

4651 Table 1: it is unclear to me what is meant by the term “nature”.

4651 Table 1: The values assigned to Dominant Function and Supporting Function would be easier to understand if there was a pre-existing list of the possible functions.

4652 Table 2: the abbreviations cc, fc, est and mc need to be defined.

4652 Table 2: The capillary rise parameter C is mentioned in the text, but not in Table 2

4655 Figure 3: Swp should be in Figure 4, not here. C is not in the list of parameters.

4656 Figure 4: The plateau parameter, p, mentioned in the text, is not shown here. C is not in the list of parameters.

4654 Figure 2: Why is there no travel time for the saturation overland flow in the wetland?

REFERENCES

Winter, T. C. (2001). "The concept of hydrologic landscapes." *Journal of the American Water Resources Association* 37(2): 335-349.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 4635, 2010.

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