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Hydrology and Earth System Sciences Discussions

Interactive comment on "Rainfall threshold for hillslope outflow: an emergent property of flow pathway connectivity" *by* P. Lehmann et al.

Anonymous Referee #4

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General comments

This paper discusses the potential to model subsurface stormflow responses from hill-slopes in terms of connectivity of areas with saturated conditions at bedrock using percolation theory. In general the paper makes a good case that the outflow behaviour of hillslopes can be modelled using percolation theory. In particular the model is able to capture the threshold like behaviour of the hillslope outflow that is often observed in catchments dominated by subsurface stormflow processes.

The paper starts with a quite tightly define model but then relaxes the model in a number of ways (eg by using variable fractional losses) so that it envelopes the variability of

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the data without necessarily predicting individual events. One issue is that the changes taken to widen the range of predictions of the model are not well justified (see below) and it may be better to simply model the average relationship rather than the whole envelope or to argue that the idea is to show that the model is capable of simulating the range of observed behaviour. There are also some assumptions and resulting interpretations that need further examination.

Specific comments.

The authors appear to ignore (in section 3.1.2) the possibility that free water percolating from upslope can extend the zone of (base of soil profile) saturation downslope and eventually establish connection. An attractive modification to the model would be to allow any free water to move downslope and to add to the storage in currently unsaturated downslope cells, thus increasing the likelihood of connection to the downstream boundary developing. This assumption could change aspects of the behaviour and it would be interesting to know by how much.

In section 2.3 the role of topography in determination of connected clusters is briefly mentioned. Western et al. (2001) used the concept of connectivity in describing the spatial structure of soil moisture patterns and they found that incorporating topography and the idea that water flows down hill to be important for distinguishing hydrologically important connectivity and that the degree of connectivity was important in simulated catchment responses in a system dominated by saturation excess runoff.

In section 3.1.1 the variable c is introduced as being the available soil water storage capacity and it is stated that a small value of c means that the site is close to saturation. This is somewhat unclear. The context of the paper implies that the site (with small c) is close to having a water table form and not that it is close to a fully saturated profile. A practical interpretation of this situation is that a value of c=0 means that the entire profile is at field capacity and that any addition of water will cause gravity drainage to move water to the bottom of the profile. An implication of this is that c depends on soil

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moisture, soil moisture at field capacity and soil depth not soil moisture, porosity and soil depth as the authors state.

The authors have included what is essentially a continuing loss term that they attribute to seepage into bedrock as part of their model. They argue that this is required because the slope of the rainfall-runoff relationship is less than 1 once the threshold for runoff is surpassed. However, given the model it would be expected that the slope would be less than one. In fact with no losses the slope should be equal to the proportion of the hillslope connected to the bottom boundary. Nevertheless some sort of loss term appears to be necessary to reproduce the macroscale behaviour as the slope is less that what would be expected given the degree of connection.

In addition the authors argue that this loss to bedrock is dependent on the maximum intensity. Their process-related arguments for this seem tenuous. Loss to bedrock should be fairly constant while there is a saturated zone at the base of the soil profile. Intuitively I would expect that the duration of this saturation would be more strongly related to the storm duration or some effective duration that allows for initially filling the soil water deficit rather than peak intensity. While the authors don't specify what time step they calculate the intensity for (they should do this), it is presumably much shorter than the typical storm durations. It takes time for the water to be routed vertically through the soil profile and this will tend to smooth out fluctuations. It also takes time for water to drain either into the bedrock or laterally. As a consequence the saturation at the bedrock fluctuates over much longer timescales than the rainfall intensity. Presumably it is the timescales associated with the saturation that are important for flow into the bedrock.

It may be that there is a strong relationship between peak and average intensity and that loss to bedrock does explain the observations but there are also other possible mechanisms that might be able explain the dependence on intensity. For example, more efficient supply to macropores at the soil surface would be expected if the rainfall intensity exceeds the infiltration capacity of the matrix. This could lead to less water being retained in the matrix. Furthermore, the authors have allowed the loss parameter to vary so that the model covers the full range of the observed data. Given they set the initial moisture contents using measurements near the base of the soil profile to represent the whole profile, some of the observed variability could be related to variations in the vertical moisture profile between events.

Finally some of the discussion in the paper gives the impression that if the model captures the macroscopic behaviour of the catchment the small scale detail must be correct. This is particularly in section 5.2. We have learnt many times that this is not a valid line of reasoning. The model is a simplified representation of complex 3-dimensional phenomenon that with specific calibrated parameter values does capture the macroscale behaviour; however, the fact that a coordination number of 3.2 and that (the concept of) a site with a size of 1 to 2m works in the model doesn't mean that these value can be interpreted physically. I have little doubt that these are really effective parameter values that result in a reasonable simulation. They are likely to change if the model details were changed, for example by introducing some spatial correlation in either soil depth or soil moisture, or by changing the lattice structure (eg from orthogonal to hexagonal).

Technical issues.

The assumptions iv and v listed on page 2927 seem to me to be part of one assumption about the spatial fields. This could simply be stated as "The spatial fields are randomly distributed with no spatial correlation".

I found the first paragraph of section 2.1 to be lacking clarity. I think the discussion should start with the idea that the hillslope is represented with a grid of sites that form a lattice. Then get into the discussion of occupation and connection and further develop the concepts of percolation etc from there. There is also some confusion about which specific lattice is being used here.

P2925, L10-14. The threshold behaviour described here is specifically for rapid lateral hillslope drainage, not hillslope drainage in general.

P2926, L27 “become” should be “becomes”

P2926, L28 “downwards” should be “laterally”

P2927, L14 “randomly distributed” could be changed to “randomly distributed (with no spatial correlation)” for clarity.

P2927, L17 “neighboured” should be “neighbouring”

P2929, L21 “as percolation” should be “as the percolation”

P2929, L26-27 It would be worth noting that for finite systems, the percolation threshold also depends on the system size.

P2932, L14 The current wording of the definition of r is complicated and it could be clarified by saying simply that r is the rainfall depth.

P2933, L15 The comment here implies that once water has entered the bedrock it wont flow laterally rapidly. This may be true for Panola but is not true generally.

P2934, L5 “realisations” should be “realisation”

P2935, L27 (two occasions) “in case” should be “in the case”

P2938, L16-19 It would be better to give sum of squared errors and the mean rather than the total.

Figure 7 The caption should be modified to make it clear that this is the distribution of antecedent spatial average soil moisture.

Figure 9a The timebase for the intensity calculation needs to be given.

References

Western, A.W., Blöschl, G. and Grayson, R.B., 2001. Towards capturing hydrologically significant connectivity in spatial patterns. Water Resources Research, 37(1): 83-97.

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