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Hydrology and Earth System Sciences Discussions

Interactive comment on "Threshold behavior in hydrological systems and geo-ecosystems: manifestations, controls and implications for predictability" *by* E. Zehe and M. Sivapalan

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Review of Threshold behaviour in hydrological systems By Zehe and Sivapalan

On reflection I should perhaps not have accepted the invitation to review this paper since I am already in another discussion with the same authors resulting from an exchange of commentaries in Hydrological Processes concerned with how to do better hydrological science. However, since that other discussion has been somewhat inconclusive about how to do better I was hoping that the authors might, in this paper, point a way ahead.

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Interactive Discussion

Discussion Paper

In fact this paper does not do this. It is essentialy a review paper about thresholds in hydrology. More particularly it is primarily a summary of the two authors'; recent work in this area, all of which is published elsewhere. This rather narrow focus had led to a lot of earlier work on thresholds in hydrology and geomorphology not being mentioned (see below).

The paper comes to no great conclusions, apart from the fact that threshold phenomena are difficult to predict. We know that anyway. Robert Horton knew that in the 1930s, including his concentration on surface controls on infiltration rather than profile controls, his work on macropores and his treatment of flow in crack systems as 'concealed surface runoff' (see Beven, 2004). The Stanford Watershed Model had a distribution of infiltration rates to allow for variable thresholds in 1962. The SCS model can be interpreted as a threshold distribution model (see Steenhuis et al., 1995; Yu, 1998). Topmodel or PDM or VIC/ARNO/Xinanjiang give a dynamic distribution of thresholds for fast runoff production. We know that thresholds are important and lead to complex responses already – the comparisons of large field plot responses provided by Hawkings, 1982; or Hjelmfelt and Burwell, 1984 are even more impressive than the modelling results summarised here. The idea that the sensitivity of a system to change depends on 'closeness'; to a threshold (as expressed by the authors) was well explored in the concepts of catastrophe theory of Réné Thom in the 1960s and 70s, a precursor to some of the nonlinear dynamics concepts mentioned here.

One issue with threshold phenomena that the authors do not bring out in their review is the way in which threshold phenomena, by their very nature tend to destroy the initial conditions that lead to their occurrence. This is particularly the case in some hydrological and geomorphological processes (bank erosion, slope failure, hydrophobicity, vegetation response to drought...) when the 'closeness' to a threshold might be affected by the ordering of events (e.g. Newson, 1980; Beven, 1981). The implications of this are that not only will it be very difficult to predict threshold effects, but also that it will be very difficult to analyse, post-hoc, what led to the occurrence of a threshold

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event. In the environment, the events are not repeated or repeatable in detail and so are different in type to the authors' repeated 'tea-pot' analogy (see below). This destruction of history implies an equifinality of potential explanations (in the changing geomorphological sense of the word, Culling, 1957, 1987, Beven, 1996).

The classic 'tiger bush' example of self-organisation is an interesting one in this respect. It has a nice neat perceptual explanation but in Niger, at least, it does not actually make up a very large proportion of the landscape. It is not 'typical' (p.3273) as much as exceptional. If it is a self-organisational response, it seems to require rather exceptional conditions. Does that mean that a 'typical' banded soil-vegetation pattern has been widely disrupted by overgrazing, as suggested by the authors (but could there not have been grazing/over-grazing effects under 'natural' nonlinear population dynamics conditions)? What would be the threshold event that would disrupt this organisation (effects of rare droughts, or fires, or distributional characteristics of rainfall events?) How do we know that the tiger bush (or the non-tiger bush) landscape is not a transient response, in a period of recovery from a past threshold event until a negative feedback kicks in (as well as the apparent positive feedback that is interpreted here) - much more like the threshold/relaxation time approach of Anderson and Calver (1977)?

Systems at the edge of criticality often involve a balance of positive and negative feedback effects - a type of system ripe for responding to a big enough external forcing (threshold crossing) effect to switch to another mode of behaviour. It is not therefore the threshold that is intrinsically interesting, but the magnitude of the forcing event required to trigger a significant nonlinear (and recognisable) response (or different types of response - see Newson, 1980, again) and the consequent relaxation from that forcing that is interesting. Not self-organisation but transience (and perhaps in some cases Weinberg's trans-science where past threshold/relaxation sequences are difficult to discern and are the result of uniquely local conditions - see discussions of De Marsily, 1994, and Beven et al., 2002).

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So what is usefully added in this paper? There is some discussion about the relationships between thresholds, feedbacks, and structure in the system. There is some discussion of predictability when systems are within some 'unstable range' of system states, particularly at the 'functional level' involving longer time scales. But this does seem to me to be new. It is as if, when Siva spent some sabbatical time in Delft, the authors found they had concepts in common and wanted to structure that commonality in their own work, but if they wish to produce a really valuable review paper I would suggest that the scope has to be extended to include much more past work by others. I did not find the paper satisfying in this form.

Some points of detail.

p.3253 and elsewhere. The tea-pot analogy - to be pedantic for once, it should not be a tea-pot (a vessel for making tea) but a pan or a kettle (a vessel for boiling water). Good tea should be made with water just off the boil.

P3260 'Flury et al (1994,1995) were the first....'. This is a very biased view of history. What about Johan Bouma's tracing experiments in the Netherlands in the 1970s, or Tammo Steenhuis' very large cores in the 1980s, and Siva should remember seeing pictures of the very impressive Rhodamine tracing experiments in the deep lateritic soils of Western Australia from around 1980.

P3277 The soil moisture sampling problem should also not be a surprise - Hills and Reynolds in 1969 suggested that $>$100 samples were needed to estimate the mean near surface soil moisture to within 5% (and then there is the deeper profile and initiation of preferential flows of course that might have an effect!!)

P.3282 field capacity? Surely this is not a meaningful term in a Richards equation model?

P.3283 raster normally means gridded…..but here it is used in reference to slope width - how can form of hillslopes be adequately represented by a raster grid of 50 or

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100 m??

P.3289 Passing reference to principles of ecosystem function

P.3292 The possibility of increasing the accuracy of measurement techniques is important and we need to be optimistic for the future but the GPR and ERT techniques can improve spatial coverage but they are not particularly accurate because of the uncertainties of the geophysical inversions and have not proven to be particularly valuable in constraining model predictions (Binley and Beven, 2003; Looms et al., 2008).

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