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HESSD

5, S2695–S2705, 2009

Interactive Comment

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

E. Zehe and M. Sivapalan

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Keith Beven’s review of HESS paper on threshold behaviour in hydrological systems by Zehe and Sivapalan and the author responses Keith Beven (K.B), Erwin Zehe and Murugesu Sivapalan (EZ&MS)

K. B: 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. In fact this paper does not do this. It is essentially 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).

EZ&MS.: We sincerely thank KB for his critical review, which challenged us to substantially improve and streamline this paper. We do not question his professionalism in performing review.

We do admit that the first manuscript and the revised paper use quite a few examples from our recent work. But this is to illustrate concepts and to refer to work that offers a novel perspective - not to ignore past specific work on thresholds. As can be seen we have now included many more instructive examples of threshold behaviour from rather different fields of environmental science - including relevant references recommended by KB - to underpin that we should distinguish different forms of threshold behaviour.

K. B: 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

HESSD

5, S2695–S2705, 2009

Interactive Comment

of Réné Thom in the 1960s and 70s, a precursor to some of the nonlinear dynamics concepts mentioned here.

EZ&MS.: We thank KB for encouraging us to come up with more visionary conclusions (which we did in section 6). However, we slightly disagree with his statement 'that we all know that thresholds are difficult to predict' (though we are sure that he knows). Why do all these models that KB cites downplay or minimize the role of thresholds through the use of 'probability distributions'?

We furthermore strongly disagree that the paper does not provide novel insights. In the revised manuscript we are very precise in distinguishing elementary hydrological processes, responses of hydrological systems - that refer to a specific problem context (flooding or erosion) - and hydrological functioning of an environmental system. We provide multiple evidences that threshold behavior occurs at each of these 'levels' and complexity of the underlying controls increases when moving from the process, over the response to the functional levels. Threshold behavior at the level of hydrological processes is controlled by the interplay of local soil characteristics and states, vegetation and the rainfall forcing. We selected overland flow formation, particle detachment and preferential flow as examples to discuss this idea in detail. Threshold behavior in the response of systems of intermediate complexity - for instance runoff response or sediment yields - is controlled by the redistribution of water and substances in space and time. This response is controlled by the topological architecture of the system that interacts with system states and the boundary conditions. Crossing the response thresholds means to establish connectedness of surface or subsurface flow paths to the systems boundary. We select subsurface stormflow in humid areas, overland flow and erosion in semi-arid and arid areas as illustrative examples, and explain that crossing local process thresholds is necessary but not sufficient to trigger a system response threshold. The third form refers to changes in the 'architecture' of human geo-ecosystems, which experience disturbances around the world. We suggest that a substantial change in hydrological functioning of a system is induced, when distur-

HESSD

5, S2695–S2705, 2009

Interactive Comment

bances exceed the resilience of the geo-ecosystem and discuss recent examples from savannah ecosystems, humid agricultural systems, mining activities affecting rainfall runoff in forested areas, badland formation in Spain and the restoration of the Upper Rhine river basin as a historical example. We furthermore suggest criteria for detecting threshold behavior in hydrological systems, namely intermittence and state dependent predictability and discuss common implications for predictability and identification of cause and effect relations in hydrology.

We think that this perspective on different forms of threshold behavior and their implications is very new, introduces ideas from neighbouring earth system sciences (prediction of earthquakes and ecology) and has important implications for future hydrological research (which is now in section 6 explained in much more detail as in the old manuscript). To bring it to the point: threshold behavior itself is not new, but the perspective we provide is brand new. Similarly, the issue of uncertainty is not novel in environmental science (e.g. meteorologists and physicists have dealt with this for a long time), but KB has introduced brand new perspectives on uncertainty, different sources and discussed the consequences for hydrological predictions.

As KB is precise in pointing out the difference between vessels and pots KB should also be precise about our conclusions. We do not simply conclude that threshold behavior is difficult to predict. We conclude that predictability depends on the state of the system. Threshold systems may be well predicted when we are sure that the threshold will be passed/or not: this depends on the combination of the state (our perspective, is the system in a critical state?) and the expected boundary conditions (KB stresses this point himself when talking about the forcing to push the system across the threshold). This is by no means trivial, since we have to accept the limits of predictability, even if we had perfect theories, models and highly resolved data. We further conclude that asking 'the why question' in the context of threshold behavior might offer a valuable perspective for understanding and predicting environmental system dynamics (in the tea pot/vessel the system switches to more/less efficient modes of dissipating energy, compare section 6).

K. B.: 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 event.

EZ&MS.: We added this very valuable point to section 2.2.

K. B: 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).

EZ&MS.: Absolutely true, we added this reference. Or in other words identical conditions are never truly identical but apparently identical (with respect to uncertainty of experimental conditions). Thus, close to a critical state we might have difficulties to establish 'cause and effect' relations even in a qualitative sense (e.g., does the system switch or not?) as pointed out in Zehe et al. (2007). We believe we stated this important point already in the old manuscript, but we put more emphasis on this in section 2.2.

K.B: 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

HESSD

5, S2695–S2705, 2009

Interactive Comment

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Interactive Discussion

Discussion Paper

’typical’ banded soil-vegetation pattern has been widely disrupted by overgrazing, as suggested by the authors (but could there not have been grazing/overgrazing 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)?

EZ&MS.: Again we ask KB to be precise. We stated that the Tiger Bush is a typical example for structured vegetation in semi-arid areas (see, Hearmann, 2008, Tietjen and Jeltsch, 2007). We never stated that this is typical vegetation for Niger. The rest of the KB arguments refer to this misunderstanding.

Yes, there is a large amount of literature that suggests the Tiger Bush to be a result of a positive feedback (see Pitmann and Stoufer, 2007, Tietjen and Jeltsch, 2007) and that exceptional disturbance due to overgrazing, for instance, or as KB suggests large droughts, might exceed the resilience of this ecosystem. We employed this example to explain the idea of a functional threshold, because it is a) stabilized by obvious positive feedbacks between water redistribution and vegetation structure b) 'causes and effects' for crossing a functional threshold can easily explained. The example is indeed 'nice and neat' as pointed out by KB. Tietjen and Jeltsch (2007) compared 41 different models to simulate coupled vegetation and water dynamics of savannah ecosystems and found a series of deficiencies concerning the representation of water dynamics and feedbacks of vegetation on key hydrological processes such as the lateral redistribution due to Hortonian overland flow and infiltration. Tietjen et al. (2009a; 2009b) developed their own coupled eco-hydrological. This underpins that finding the right level of simplification (or complexity) to model bi-directional feedback is difficult even when we deal with systems that appear 'nice and neat' at first sight.

HESSD

5, S2695–S2705, 2009

Interactive Comment

K. B: 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).

EZ&MS.: How can we know what is a big enough forcing to cross the threshold and whether state is ’critical’ if we don’t know the underlying controls of threshold behavior? This is in fact not independent, it is a combination of how critical the state is in the sense ’how far the system’ is from the threshold (with uncertainty) and whether the expected forcing is big enough to push the system to cross the threshold i.e. to work against the balance of positive and negative feedbacks (with uncertainty). Understanding whether the state is critical requires understanding the controls of threshold behaviour, which are qualitatively different at the process, response and functional level. This is what the paper aims at and this is now much clearer expressed in section 2.2. and the following sections.

K. B: 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

HESSD

5, S2695–S2705, 2009

Interactive Comment

include much more past work by others. I did not find the paper satisfying in this form.

EZ&MS.: We are astonished by the suggestion that somehow we came up with these ideas on a whim. We added many references recommended by KB and in fact many more from the fields of geomorphology and ecology. However, it seems that KB missed to survey the HESS special issue on ’thresholds’ that MS co-edited, which was based on the successful Sir Mark Oliphant Conference on Thresholds and Pattern Dynamics - A New Paradigm for Predicting Climate Driven Processes that MS co-convened in July 2005. Our ideas have been informed by fresh ideas gained at this very successful inter-disciplinary conference. We have exhaustively explained above what we think is new.

We think that the conclusions section of the revised manuscript discusses very precisely a key dilemma in hydrology. Namely that our models are either capable to predict threshold behaviour at the process levels (controlled by local state variables, boundary conditions and system properties) or to predict/reproduce threshold behaviour at the response level by implicitly conceptualizing the (hidden) multivariate statistical or topological controls based on effective states and parameters. However, we lack models that can do both i.e. capture how threshold behaviour at the process level and the redistribution of local dynamics translate ’forward’ to the response of a hydrological system and allow a ’backward’ estimate on the pattern of local dynamics and structures that control redistribution after they have been shown to reproduce system behaviour. The rest of the conclusion section suggests the way ahead, as encouraged by KB.

Some points of detail. K. B: 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.

EZ&MS.: We prefer coffee in the morning :), so reformulated the analogy.

K. B: 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

HESSD

5, S2695–S2705, 2009

Interactive Comment

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Interactive Discussion

Discussion Paper

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.

EZ&MS. We will reword the sentence accordingly. Still, Markus Flury was the one that pushed this technique in the soil physics community (as KB acknowledges in his own book)

K. B: 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!!)

EZ&MS We will added this reference.

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

EZ&MS. We defined 'field capacity' as soil water content at -0.63 m matric (according to the German Soil Science Society).However, we skipped this detailed part on our model setup as recommended by Stefan Uhlenbrook,

K. B: 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

EZ&MS. The hillslope model is 2D (vertically and downslope), thus it integrates across the slope with and cannot account for changes in soil type and crops, which is of course an approximation. Soil types do not change in perpendicular to the downslope direction there. Areas of uniform cropping are typically 50 to 100 m. wide. We think, and the results in several papers show, that this approximation allows good reproduction of runoff and also internal state dynamics.

K. B: P.3289 Passing reference to principles of ecosystem function

HESSD

5, S2695–S2705, 2009

EZ&MS.: We are not sure what is meant by this comment.

K. B: 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).

EZ&MS.: True, inverse problems are under-determined and solutions are therefore non-unique. We will add references accordingly. One way ahead is joined inversion of several independent observations as suggested by Paasche et al. (2006), Paasche & Tronicke (2007), and Tronicke et al. (2004), see below for these references.

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HESSD

5, S2695–S2705, 2009

Interactive Comment

Full Screen / Esc

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Interactive Discussion

Discussion Paper

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