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Hydrology and Earth System Sciences Discussions

Interactive comment on "Temporal dynamics of hydrological threshold events" *by* G. S. McGrath et al.

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We would like to thank the reviewers for the useful suggestions and comments to improve the manuscript. For easier reference reviewers comments are paraphrased (bold font) and the response follows below (nomal font).

Response to Reviewer 1

I felt that much of the paper could be better written...
I acknowledge that the motivation, and relevance, and implications of this study requires a better description in the introduction and conclusions. This has been changed

to improve readability. I agree there is too much jargon initially and have made changes accordingly.

..the equations and corresponding descriptions for variance, coefficient of variation etc are probably unnecessary given that these are relatively standard mathematical terms.
We agree that they are standard terms but they make the paper more readable, allowing non mathematical readers to understand the flow of what was done and how.

Response to Reviewer 2

Though beyond the scope of this paper, it would be of interest in the future to investigate the identifiability of these drivers of the variability using observations of physical process that can actually be measured. Questions to be investigated could include what measurements/data are required to determine the trigger for macropore /preferential flow? Such a study would improve the practical impact of this paper. An example of a recent work which looked at identifiability, albeit in a very different context, is given in Thyer et al. [2006].
I agree that identifying which aspects of the driver are significant controls on processes is an important question. In other work we have recently conducted we have attempted to identify which aspects of the rainfall signal were associated with triggering pesticide transport through lysimeters. The benefit of such work and that proposed by the reviewer is the ability to contextualise observed phenomena in a climate setting and therefore to hopefully make better predictions at ungauged sites for example. The identifiability of thresholds is also an important aspect which formed a significant point of discussion during the Thresholds and Pattern Dynamics workshop. We have fairly good small scale process understanding of many processes but what happens at larger scales is still poorly understood. Thresholds occur at these scales too, see for example

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Zehe and Blöschl, (2004) and Lehmann et al. (this issue) for hillslope scale thresholds. However can we predict a priori what these thresholds are and what determines their magnitude? From the example of pipeflow, similar threshold behaviour has been observed in a number of hillslopes around the world (Uchida et al., 2005) but the values of the thresholds are vastly different at each of these sites. With what tools can we make predictions of these types of thresholds?

...it is recommended that the authors verify the relationships derived ... by the process of numerical simulation...
All the analytical equations were verified by numerical simulation, and we feel confident enough to present them alone. Presenting verification of analytical solutions would add unnecessarily to the already lengthy paper. However we've added figures illustrating the comparison between numerical and analytical solutions to the supplementary material.

Rainfall is modelled as a series of independent and instantaneous (no duration) events which (as the authors state) is considered valid at the daily time scale. However, the physical process ... would occur at much shorter time scalesĚThis incongruity may invalidate the results ... it should be at least discussed...
We are primarily concerned with an event based description of processes as this allows a direct comparison to the driving rainfall signal. Also, because of the simplicity of the modelled triggers, sub-event scale rainfall may not be the appropriate "scale" of representation. Therefore we do not capture the within-event triggering dynamics but rather the intra-(rainfall)-event dynamics, i.e. this event did or did not trigger the threshold. The model of rainfall for infiltration excess describes a snap-shot of an arbitrarily short period of the highest rainfall that occurs during an event, as a surrogate for saying that based on this threshold preferential flow was triggered at least once during the event. Additionally as discussed above the idea was to investigate simple but general thresholds. We acknowledge that this incongruity should be discussed better in the text.

The abstract states that this paper derives new relationships for both the infiltration excess (IE) mechanisms and saturation excess (SE) mechanism. However, in the paper it is not clear how the IE results differ from that of Rodriguez-Iturbe et al (1999).

Rodriguez-Iturbe et al. (1999) considered storm volumes as opposed to intensities, and therefore strictly evaluated a type of SE mechanism with no carryover of storage between events. We used the same results considering an arbitrarily short period of rainfall intensity within that event exceeding an intensity threshold. The difference is purely semantics and we agree this should be recognised as such.

Response to Reviewer 3

On Conclusions, could the authors comment on the possibility of deriving similar expressions based on a rainfall model that accounts for clustering? What if storm depths are not modeled as exponentials as in the Appendix?

These are very interesting questions and ones I would like to investigate further. Inter-storm times have been previously well described by Levy-Stable distributions (Menabde and Sivapalan, 2000) which better captures the probability of extreme inter-storm durations. Such distributions have a similar probability density function (pdf) as displayed by the temporally clustered pdf of Figure 2b (AI=1).

It is currently unclear to what extent the degree of clustering of rainfall events would be reflected in the degree of clustering of flow events. We expect it will depend upon the memory inherent in the system i.e. the magnitude of the β term (apropriately modified). A system with a large β will be much more significantly affected by extreme inter-storm times than those with a small β for example.

For the rainfall intensity threshold and Poisson arrivals the type of pdf of inter-event times remains unchanged irrespective the intensity pdf as it depends only on the prob-

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ability of exceeding a threshold intensity not the nature of the intensity pdf so long as these two variables are independent. Therefore a temporally clustered input with a simple intensity threshold may produce a similar filtered pdf as the input timing pdf. When the timing and magnitude pdfs are not independent of one another more complex behaviour may be expected.

If storm depths were gamma distributed instead of exponentially distributed for the storage threshold one might expect more complex behaviour with the degree of temporal clustering depending nonlinearly upon both the mean and variance of the distribution. The results from the exponential distribution we provide here do provide a limiting case of gamma distributed storm depths.

Response to Reviewer 4

... the question arises whether it makes sense to compare these two mechanisms over the whole range of climates.
In fact we do not make the comparison between the two processes across a climate gradient, but rather only on the type of filtering expected of the different types of thresholds. Infiltration excess may be affected by the aridity through soil moisture's control on the infiltration capacity or other factors affected by the aridity such as biological activity and soil structure but this was not addressed in this paper.

...Poisson assumption for storm depths and inter-storm time. Is this assumption valid for all climates that are analyzed? Can you provide some precipitation data that shows the validity of this assumption for different climate settings? To my knowledge, many stochastic precipitation models move more and more away from independent Poisson processes. I am in particular curious about the assumption for storm depths. There should also be some natural limitations of parameters describing the precipitation input depending on climate

The reviwer is right to ask for discussion of the applicability of the Poisson assumption. The text has been modified accordingly. This work follows a significant history of the use of the assumption in hydrology. As mentioned previously rainfall is not exactly described by a Poisson arrival process, it is neither instantaneous nor unclustered. I refer this comment back to the general discussion at the beginning of this response, related to the generality of the approach. To me it makes sense to use a simple rainfall model with simple process conceptualisations as a basis for developing understanding before jumping into the deep end by increasing complexity. Additionally an initial hypothesis was that soil moisture storage will induce temporal clustering of event triggering. By using an unclustered rainfall model this hypothesis could be clearly tested.

Rigby and Porporato, (this issue) investigate the impact of the simplifying assumption of simple soil moisture and rainfall models in comparison to more complex process descriptions (including rainfall duration etc). This investigation aims to better understand the applicability of such simple models.

No seasonality, neither for rainfall nor for evapotranspiration. Since seasonality is very important in many climates, I would assume that this assumption may have a strong effect on the results.
Again this is beyond the scope of this paper. Relaxation of the assumptions is left to future research having now developed a framework for hypothesis generation. The stationary rainfall description is sometimes justified on the basis of its approximate applicability within a season and this justification may also apply to a degree here too. The reviewer is right to say that seasonality will have a significant impact, as not only the structure of storms but also the type of rainfall they deliver, changes throughout the year. In the context of pesticide transport the impact of seasonality of rainfall has been a focus of some research we have been conducting.

ET is constant and does not depend on soil moisture. Many models and experiments show that ET depends on soil moisture.
ET *is* modelled as being soil moisture dependent. It ceases when the storage reaches

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zero. As shown by Rodriguez-Iturbe et al. (1999) that with more complex accounting of ET dependence on soil moisture a bi-modal pdf of soil moisture emerges, with one peak near field capacity and the other near wilting point. While much less complex, the pdf of soil moisture derived by Milly (1993, 2001) displays the same bimodal form. Milly (2001) also demonstrated that estimates of actual evaporation by the simpler model were very similar to to the more complex version for a set of chosen parameters. So we feel the simplicity still captures such a soil moisture dependence, albeit with a little less detail.

The authors assume that only one process occurs at a time in a watershed. However, many watersheds show a mixture of processes. We agree with the reviewer that a number of runoff processes can occur in watersheds simultaneously. Our answer to the reviewers first comment above, applies in part to this issue too. In the Introduction we mentioned a slightly more complex numerical approach (Struthers et al., 2007a,b) with both infiltration excess and saturation excess. We also stated that the objective of this paper was to analysed each process individually to assess the type of filtering expected.

In this paper we consider short lived fast flow processes which tend to be more threshold like and as a result we deliberately neglect slower, more continuous contributors to catchment runoff. While feedbacks and interdependencies can occur between slow and fast flow processes, these are neglected in this paper but may introduce interesting effects on the temporal dynamics. Again this is left for future research.

Finally the rainfall model we adopt is essentially a point based model and so these results are likely not operational at watershed scales. At best these results could be applied at hillslope and plot scales. None the less it remains an interesting question of how spatio-temporal rainfall impacts the spatio-temporal structure of the triggering of threshold processes in watersheds and the accumulated impact of this on the runoff signal at catchment scale.

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The authors need to specify more clearly what is new about their approach and what has already been done by Milly and Rodriguez-Iturbe.
One extra acknowledgement to Milly (2001) and one to Rodriguez Iturbe (1999), with regard to the semantics of the IE mechanism, have been added.

I am not very convinced if we benefit from analytical solutions as derived in this paper. The authors only show the solutions for specific aridity indices (0,1, inf). Did they also derive the solutions independent of AI. If not, I am wondering how the results in Figure 3 and 4 where derived. Please clarify. Since the numerical simulation of these processes are so simple, I am wondering if a model that is more flexible (seasonality, ET, etc), but has to be solved numerically, may be even better.
We agree with the reviewer that investigation of the impact of more realistic descriptions of climate is an attractive goal for research. The issue of model simplicity, the analytical approach, and generality addressed above apply to this point.

A stated in the text the complete analytical solutions for the first four moments are provided in the supplementary material. The later moments in particular are far too complicated to be of any direct value to presentation. The value of these higher moments is to demonstrate similarity to the higher moments of the exponential distribution in the arid and humid extremes and deviation from an exponential distribution in intermediate climates. We argued in the text that these higher moments displayed the characteristics we expect of temporal clustering and hence further discussion was restricted to the mean, variance and coefficient of variation. Equations 16 and 17 were used to generate Figure 3 and Equations 11 and 14 were used to generate Figure 4. This will be made explicit in the figure captions.

The authors focus on preferential flow as a very important threshold process. This may be true, but I think it is a bit over-stated. Infiltration excess and saturation excess are just the two most important overland flow generation mechanisms. They may be also important for preferential flow, but we often do not

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know how preferential flow is connected to stream flow. The authors should also cite some literature that experimentally studied the initiation process of preferential flow and not cite other modelling studies (e.g. Simunek)
Preferential flow is all important if you study pesticide transport for example. This is my bias. If you were an ecologist you may be more interested in the storage dynamics of leaf litter intercepted rainfall. Or a hillslope hydrologist perhaps more interested in pipe flow. All these processes display episodic, threshold dynamics to varying degrees. The aim of the paper was to develop a framework for unifying these processes into a more general hydrological framework.

Two references to experimentalists are also added on this point.

The section about the role of initial storage (6.3.2) may be removed from this paper. ...in my opinion (it has) no practical value. Watershed will never have an initial storage value, ... initial value is just related to out model framework ... There is never a first time saturation event.
We respectfully disagree with the reviewer on this point. Nearly all measurements we can make are done in the context of a "when". Additionally we often need to know the timing of an event since some other occurrence. For example it would be useful to understand the timing of the first significant erosion event after land use/management change, natural disturbance, or the reconstruction of mined landforms in order to better manage environmental impact. Perhaps we can convince the reviewer otherwise with the following analogy:

Shortly after a pesticide application a soil moisture measurement is made in the near surface soil. This is time zero. From previous experiments we know that this soil mosture determines the occurrence of preferential flow in a highly nonlinear way such that it determines whether it is occurring or not. We also know from experiments that there is a rapid movement of rainfall to groundwater via this mechanism, and it is effectively a discrete, on/off process making it occur episodically. If we take a measurement of the near surface soil moisture, that measurement is an initial condition relative to the time

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of measurement. As it turns out our analysis reveals that this state of the system determines our level of certainty about the time till the next preferential flow event. Therefore we now have a measure of risk, the mean and variance of the time till the next pesticide leaching event. This risk measure includes our knowledge of the structure of rainfall as well as our uncertainty of rainfall events to come in the future, based upon a single measurement of soil moisture.

As mentioned by one of the other reviewers. It would be really nice to have some data to verify some of the hypothetical results. Or at least, the authors may comment on how we can verify this model, what data do we need and how do we need to analyses and measure the data?

A future outlook statement has been added to the conclusions to reflect this point. An extensive validation process has not been undertaken and is beyond the scope of this paper. It is an interesting question of what to measure and how to analyse. It will of course depend on the process and its degree of observability. For example preferential flow might have to be quantified in the field by the temporal structure of intermittent pesticide breakthrough and related to near surface soil moisture as the preferential water flux is largely not measurable yet. In combination with isotopic analyses of deuterium to infer the flux and the measurement of storage in a weighing lysimeter all three of the aspects of the process investigated here might be quantifiable. Developing comprehensive inter-relationships between flux, timing and soil moisture variability will likely require laboratory scale or weighing lysimeter studies initially.

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