**Hydrology and Earth System Sciences Discussions**

Interactive
Comment

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

**Anonymous Referee #4**

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## General Comments

After reading the abstract of this paper the first time, I though this should be a really interesting paper. However, I have to admit, that I am now a bit disappointed with the paper after better understanding the details and assumptions behind this paper. In general, the paper is well written, the methods and results well structured and developed. As the other reviewers have already pointed out, the paper is quite technically dense, but even without understanding the mathematical details, the reader can still follow most of the results and derivations. However, there are some points that I think need to be addressed before the paper is published in HESS:

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1) One focus of the paper is to compare the statistical properties of inter and intra-event times for different climatic settings. This may be a nice theoretical exercise, but is far from being applied practically. We know that infiltration excess runoff dominates in arid areas, whereas saturation excess mechanisms dominate in humid areas (see for example work by Mike Kirkby). So, the question arises whether it makes sense to compare these two mechanisms over the whole range of climates.

2) If few assume that this comparison makes sense, then the paper relies heavily on many assumptions that are not much discussed at all. I ill just mentioned some and hope that the authors can provide a more thoughtful discussion and clearly show the limitations of their assumption:

a. 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

b. 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.

c. ET is constant and does not depend on soil moisture. Many models and experiments show that ET depends on soil moisture.

d. The authors assume that only one process occurs at a time in a watershed. However, many watersheds show a mixture of processes.

3) The authors need to specify more clearly what is new about their approach and what has already been done by Milly and Rodriguez-Iturbe.

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4) 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.

5) 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 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)

6) The section about the role of initial storage (6.3.2) may be removed from this paper. It may be interested in relation to the model, but it has in my opinion no practical value. Watershed will never have an initial storage value, since they are continuously storing and releasing water. The initial value is just related to out model framework and hence should not be included in this analysis. There is never a first time saturation event.

7) 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 analyzes and measure the data?

Specific and Technical Comments

1) There is a difference between non-linearity and threshold process. Please, define specifically for this paper.

2) Kohler (2003) is a very recent reference for describing the threshold behaviour of

infiltration excess. Please cite a more original one.

3) Please provide reference to the definition of the aridity index. There are different definitions around and many do not make the distinction between humid and arid only.

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