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Hydrology and Earth System Sciences Discussions

# *Interactive comment on* "Iterative approach to modeling subsurface stormflow based on nonlinear, hillslope-scale physics" *by* J. H. Spaaks et al.

**J. H. Spaaks et al.**

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We would like to thank Anonymous Referee #3 for taking the effort of writing a clear and concise Interactive Comment. We feel that the manuscript has benefited greatly from the constructive feedback we received. In the sections below, we cover specific issues in more detail. The Referee's comments are verbatim in bold face; our comments are in normal face.



## 1 Major points

**Selection of the single storm you used in simulations: Firstly, this storm does not seem like such a major event. Thus, given the long history of storm data collected at Panola, I am wondering why this was selected, and why only one storm was tested. Using several storms with different characteristics would have made an interesting comparison. Anyway, please justify your use of this single event.**

The Referee raises two issues: (1) why use an event of this size; (2) why use one event.

*Why use an event of this size*

We wanted to demonstrate how the observed nonlinear behavior of subsurface stormflow dominated hillslopes could be incorporated into a model. We focused specifically on the internal connectivity of wet patches. This implied that we needed a storm that could activate all types of behavior (unconnected dry state, as well as the connected wet state). This could only be achieved with a storm of a certain minimum size. On the other hand, we did not want the storm to generate much more lateral flow than was observed at Panola. We could have easily done this by either imposing a very large rain event, or by ignoring bedrock infiltration. We chose not to do this, because it would have generated a body of transient saturation, which was internally highly interconnected, which would have meant losing the hydrologic behavior specific for subsurface stormflow dominated catchments.

*Why use one event*

Generally speaking, calibration using multiple events essentially amounts to compensating for errors in the initial and boundary conditions (as well as allowing the parameters to compensate for errors in the model structure). This is why, for prediction purposes, it is important to use as many events as possible. However, when trying to 'finger down' into how a model result came about, it is not necessary to do this. In fact it may even be better to have a short period of time. For instance, none of our models would perform well when the summer season is included, simply because we have not yet incorporated the processes relevant during summer. Since it is impossible to manipulate the hillslope system as we would do in a laboratory experiment, limiting the simulation to a short time period is the next best thing. While this implies that one cannot extend the conclusions to the summer season, it is helpful in separating the effects of different processes.

Nevertheless, the 'one event'-issue was raised by all Referees—albeit in different contexts. It became obvious that some clarification was needed in the description of our experimental design's rationale. In the revised version of the paper, we therefore included a section similar to our explanation above.

**Section 6 of this paper: I found this section of the paper not so well developed and described compared to the other virtual experiments. In this experiment you imposed a very different type of storm (small and rather low intensity) to 'better assess differences in flow timing and magnitude between models 1 and 3'. You need to more clearly state why this virtual experiment can accomplish this goal; specifically, why a small storm is better to use. Also, maybe less details and a better overview would benefit this section as a whole. Could you include a schematic to help illustrate what you are doing here? The tabular information you present contains the details but not the 'big picture'. Overall, this section needs some work to justify its existence.**

In Virtual Experiment 4, we wanted to demonstrate how each model is associated with a certain type of behavior, and how other types of behavior may not be possible. In this experiment, we started out from spatially homogeneous initial conditions (in



terms of relative saturation). We ran both $Model_1$ and $Model_3$ in transect setup. For each model, the elements did not show very different behavior during the first event. (Although the flow curves for $Model_3$ were different from those of $Model_1$). At the start of the second event, relative saturation was no longer homogeneous. It became apparent that $Model_3$ can behave very differently *within* the hillslope, whereas this was not possible with $Model_1$. Of course, the artificial storm characteristics were chosen in such a way that the difference between the two models could be visually demonstrated (Fig. 6).

In the revised version of the manuscript, we added text to explain our rationale more clearly.

**Section 3.2.4: As I noted, I like the fact that you included leakage, but this process really should be described and justified in more detail; e.g., I have no idea what the Kl parameter is or how it would be measured/estimated. And I would have really liked you to have shown how this part of the model behaved during the storm (as well as other hydrological components).**

In the revised version of the manuscript, we have added some text to clarify how leakage is incorporated in the model. Unfortunately though, the process of leakage is not very well quantifiable. This is why many studies either do not include it at all (assuming impermeable bedrock), or assume homogeneity on the leakage parameter (that is, $K_l$). The lack of direct measurements also means that its value is estimated through calibration. For the specific case of Panola it is only possible to match the flow curve if there is a significant loss term in the model. Even so, it is unlikely that losses to the bedrock are spatially homogeneous, given the presence of blocks and joints in the bedrock. For Panola, there is some evidence to support this: some areas of the hillslope have runoff coefficients of 0%, even for unrealistically large events (see Tromp-van Meerveld and McDonnell, 2007; HP 21; page 754), whereas other parts obviously do generate some subsurface stormflow.

Regarding the final part of the Referee's question, we refer the reader to our comment on the last of the Major Points in this document.

**Summary and Conclusions: Overall I felt this section was a bit long and could have focused more concisely on the important comparative findings, instead of the blow-by-blow summary description. This needs some work.**

At the suggestion of the Referee, we completely revised this section. We minimized the 'summary' part of this section, while the 'conclusions' part now has a higher level of abstraction. Also, we added a section on future steps to be taken for progress in the field.

**General Comment: While I am not really a big fan of virtual models, the approach taken here provided some useful insights into important hillslope hydrological processes. What would have made this a much better paper, would have been the assessment of the behavior of individual hydrological components that were part and parcel to these models.**

The Referee suggests that the manuscript could be improved by including all components of the water balance in our assessment. Initially we planned to include all components in the figure for this experiment. However, it turned out that some components had very similar patterns. Because we needed to strike a balance between including all relevant information explicitly, and keeping the figure fairly easy to interpret, we decided to omit some of these components. For example, in Fig. 6 we omitted leakage to the bedrock, and mobile water storage is also not included. However, the pattern of these variables is approximated by the lateral flow data which we do present. This is because lateral flow is approximately proportional to the volume in mobile storage, while leakage is exactly proportional. Presenting either storage, leakage, or flow in the figure therefore also gives insight into the remaining two variables. Since this is not obvious from the manuscript as it is now, we have added some text to the revised



version to point this out.

## 2 Minor points

**Pg. 5206, l. 17-18: your expression 'discharge being too steep' and 'not steep enough' seems a bit strange; the discharge is not steep, it is the rate of change of discharge that is steep.**

Changed.

**Pg. 5207, l. 8: I would substitute 'a relatively low permeability layer' for 'impermeable' because of what I discussed earlier about 'leaky bedrock'.**

Changed as suggested.

**Pg. 5207, l. 13: Including the Weiler et al., 2005 reference here along as an example of an early classic reference for subsurface stormflow is not appropriate; if you need another reference, I suggest Whipkey's 1965 paper.**

Changed as suggested.

**Pg. 5208, l. 7: Actually a better reference than Sidle et al., 2000 is Sidle et al., 2001 (also in HP, reference given later)**

Changed as suggested.

**Pg. 5208, l. 9: instead of 'channels' in the bedrock, I would say 'fractures and joints'**

Changed.

**Pg. 5208, l.12: in addition to Phillips, 2003, here is where the Sidle et al., 2000 reference would fit (as well as on pg. 5211, l. 8)**

Changed as suggested.

**Pg. 5209, l. 8: Could you please elucidate the 'downward approach'?**

Klemes 1983 (JoH 65) presents an interesting division of approaches to hydrology into an upward and a downward approach. The difference between the two is the direction of movement in terms of scale. For the upward movement, the goal is to start from behavior which is known at a small scale (for instance, the soil core scale), and integrate this to describe the behavior at a larger scale (for instance, the hillslope scale). In contrast, our approach is downward in the sense that we move in the other direction in terms of scale: we know the behavior that has been observed at the hillslope scale (and we also have a few clues as to what might have led to it). We subsequently constructed hypotheses with which we describe this hillslope scale behavior.

**Technical writing issue (example; pg. 5214, l. 16-17; pg. 5215, l. 3; pg. 5218, l. 9, etc.) It is better not to start a sentence off with telling readers what is in a figure —we can see this. Simply make a statement about the results of the figure that you wish to elaborate on and then put the figure number in parenthesis at the end of the sentence.**

Changed.

**In Figures 2, 4 & 5: There is a little confusing because you use A, B, & C to denote both different parts of the figure AND ALSO different portions of the x-axis. I recommend changed the notation on the x-axis (and in the text) to numbers.**

Changed as suggested.



**Pg. 5215, l. 6-7: Actually there is one rather steep section of simulated discharge rate change in sector B.**

The simulated hydrograph for $Model_1$ is indeed too steep for Sector B in comparison with the observations.

**Pg. 5215, l. 27-28: Why does this percentile contain only a few measurements? I may have missed something, but this is unclear and a bit confusing.**

The intervals presented here are parameter uncertainty intervals. We constructed these intervals based on the last 1000 parameter sets. A big interval means that, even though the objective scores are very similar for these sets, the discharge curves associated with them can be quite different ('being wrong in different ways'). When the shape of the curve is more like the observations, the last 1000 parameter sets are less likely to 'be wrong in different ways'.

**Pg. 5223, l. 13: change 'between' to 'among'; also same comment on pg. 5226. l. 8.**

Changed as suggested.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 5205, 2009.