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Hydrology and Earth System Sciences Discussions

Interactive comment on "The effect of spatial throughfall patterns on soil moisture patterns at the hillslope scale" *by* A. M. J. Coenders-Gerrits et al.

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This is an interesting paper on the effects of spatial patterns in throughfall on (simulated) soil moisture patterns. The authors use a throughfall pattern from one site and apply it in a modelling environment to another site. They study the effects of the selected throughfall pattern on the simulated subsurface flow response and soil moisture patterns. They also look at the effects of slope, storm size and soil depth in modulating the effects of the throughfall pattern on soil moisture. The proposed geo-statistical hydrograph is a novel and interesting concept. While this paper is

interesting, I have several concerns

The main difference between the spatial throughfall pattern used in the simulations of this study and those in Hopp and McDonnell (2011) is the higher standard deviation of the larger throughfall pattern. The authors explore the effects of storm size, slope and soil depth on the simulated soil moisture patterns, but not this important aspect. It is not clear why the authors did not explore the effects of the standard deviation of the throughfall pattern as well, especially because it can not be assumed that the standard deviation of the throughfall pattern remains constant when storm size or slope increase.
It is true that we did not study the effect of different standard deviations in the throughfall data on the soil moisture. In this study we focused on one single throughfall pattern with a fixed standard deviation (and range). Of course it would be interesting and important to study the effect of differences in throughfall standard deviation on soil moisture. However, the reason why we did not do this is twofold. First of all, we preferred to focus on the hillslope controls in relation to a throughfall pattern as an extension of the paper by Hopp et al (2009, 2011). Secondly, we did not have the required data set for such a study. As mentioned by the reviewer, the standard deviation will indeed change due to storm size and slope. The effect of storm size requires spatial throughfall data on event scale. For the Huewelerbach catchment the smallest time step for the spatial pattern is 2 weeks, so the effect of storm size is already averaged out. Also other spatial throughfall studies (e.g., Bouten et al, 1992; Keim et al, 1995; Staelens et al, 2006; Zimmermann et al, 2008) do not provide this data as far as we know. The change in standard deviation due to different slopes will, in the revised manuscript, be taken into account by correcting the data with the cosine of the angle. See for details comment 1 of the reply to Erwin Zehe.

We will further clarify our research scope in the revised manuscript.

The authors mainly look at the effects of throughfall on depth averaged soil moisture. It should be discussed more clearly how the effect of the throughfall pattern is different for shallow soil moisture and deep soil moisture. One of the conclusions of this work is that soil moisture patterns before and after an event resemble the bedrock topography. However, this result is for depth averaged soil moisture and is thus highly influenced by ponding/saturation at the soil bedrock interface. The authors do not discuss if these results for depth averaged soil moisture are also observed for shallow (near surface) soil moisture. Other studies have shown that ponding at the soil-bedrock interface does not last much longer than a week after an event at the Panola hillslope, so it can be assumed that the depth averaged soil moisture pattern will change again after this 'remnant' ponding in bedrock hollows has disappeared. I expect the soil moisture pattern at depth or the depth averaged soil moisture pattern to be influenced less by the bedrock topography after this period. The results for the intra-storm conditions in this study therefore seem to be still influenced by the event. The pre-event results may also still be influenced by drainage from the initial conditions and may thus also not represent intra-storm conditions well.

The shown semi-variogram analysis is indeed only on depth averaged soil moisture patterns in order to only show the most essential figures, however we also looked at the soil moisture patterns per layer. As can be seen in Figure 5 the soil moisture patterns indeed differ per layer. Firstly, the throughfall pattern will be first visible in the top layer and slowly drains to the deeper layers. Secondly, the figure also shows that on the bedrock interface soil water is accumulating and indeed is filling the water pockets, while in the top layer more 'free drainage conditions' occur. In the revised manuscript we will further discuss these issues in Section 4.2 and 5 and extend Figure 10 by also presenting the geostatistical hydrographs for shallow and deep soil layers.

Furthermore, our results show that after one week (t=190h) the soil moisture pattern

reflects the bedrock topography. It is possible that in reality this is not the case. This can be due to several processes that are not incorporated in our study. As discussed on P8640L27-P8640L18 transpiration, macropores, and antecedent soil moisture are neglected. These processes are important for soil moisture behaviour, but are not included in this study because no data was available for these processes, and to unravel the complex hillslope bit by bit. We totally agree, that if one attempts to simulate observed soil moisture, these processes cannot be neglected. However, since hillslopes are complex, we decided to only focus on the controls as shown in Figure 12 by the solid blocks, and to study how these controls interact. Transpiration, macropores, and antecedent soil moisture are important processes that needs to be investigated in the future. On P8642L24-P8643L5 we hypothesize how we think that the neglected processes will influence soil moisture behaviour. We will elaborate on this in the discussion and emphasize that we do not claim our findings are completely in agreement with reality.

When the authors do not look at depth averaged soil moisture, they compare soil moisture in the different slices. Since the slices have different thicknesses, they represent different depths below the soil surface. Thus soil moisture at for example 5-10 cm depth in one area is compared to soil moisture at 60-80 cm depth in another area. This is not well explained in the text and it is also not clear how this comparison of soil moisture at different depths influences the results.

The reviewer is right that we compare soil moisture in different slices or layers. The layers are linearly scaled between the top surface layer (10) and the bedrock layer (5). Thus layer 9 is always 20% of the total soil depth deep, layer 8 40%, layer 7 60%, and layer 6 80%.

This indeed means that e.g. layer 9 has different absolute soil depths. We agree, that therefore Figure 5 and 7 should be interpreted with (more) care. In Figure 6, 9, 10, and 11 we used the average soil moisture over the entire depth. We will add this information and its consequences to the revised manuscript.

Finally, the Panola hillslope has been used in other hillslope modelling studies. By more carefully comparing the results and assumptions of this study to those studies it will be easier for the reader to compare the different hillslope models and to understand the different assumptions.
Ok, we will more carefully compare our results with the other Panola studies (see also reply to specific comments).

Specific comments:
1.) P8628L10: Note that Keim et al (2006) also showed that throughfall has a small influence on the modelled subsurface flow responses. It is not just the amount of precipitation that is important but also the intensity. Perhaps explicitly mention this intensity effect somewhere in the text. Now it appears that the differences are attributed to the amount effect.
We will refer to Keim et al (2006) and will discuss the influence of intensity smoothing for our findings.

2.) P8628L20, P8629L1-2: If the variation in throughfall or the scale of the pattern is so important that it warrants a new study and adds something significant to the results of Hopp and McDonnell (which I agree it does), it is odd to study the effects of the throughfall pattern on the effective range (Fig 11) but not study the effects of the range of the throughfall pattern or the variability of throughfall on the responses and in fact assume that this range and variability remain constant when slope or rainfall amount change. This is a big assumption that needs to be addressed (and discussed) more.
In Gerrits et al (2010) it was found that the effective range of the throughfall pattern did not differ much between the seasons. Apparently, tree structure (stem + branches) is

more important than canopy structure (phenology). Therefore, we did not investigate differences in effective range for the base case scenario. For the case where we adjusted the slope, we will correct the effective range of the throughfall to consider that trees do not grow perpendicular to the slope, but in the opposite direction of gravity (see also comment 1 reply to Erwin Zehe).

3.) L8630L1-9: There is no need to describe this soil moisture data if it is not used anywhere in this study. If you used it, then you have to better describe how and when you used it.
Ok, we will remove this paragraph.

4.) P8631L11-15: Better explain why these five classes are used if continuous data is available. The advantage of using classes is not clear and leads to extra smoothing of the data and the pattern.
We used five classes because HYDRUS-3D only has 4 ‘Variable Fluxes’ plus 1 ‘Atmospheric Flux’, which can be used to simulate spatial throughfall data. We will add this enforced limitation by HYDRUS in the revised manuscript.

5.) P8632L11: If the pattern with the largest influence on SSF is chosen, is it likely that this is also the pattern that leads to the largest effect on soil moisture? Does this thus suggest that the modelling results represent a potential maximum effect since this specific pattern was chosen? The impact of choosing this pattern on the results should be discussed somewhere.
Ok, we will elaborate on our choice and its effect on the results. We agree, that in the current manuscript we give the impression that the pattern with the largest effect on SSF will also have the maximum effect on soil moisture. Although this may be the case (we did not investigated this), one should be aware that the differences on SSF are small.

6.) P8633L10: Change the ordering of the methods paragraphs (3.1 and 3.2). You first need to describe the model as otherwise it is not clear what these layers are. Also explicitly state here that this is the depth averaged soil moisture.
We will change the order of Section 3.1 and 3.2

7.) P8633L19: It is not that clear to me why it would save computation time to assume a nugget of zero. Perhaps explain better.
See minor comment 2 of Reply to Erwin Zehe:
"We used an optimization algorithm to fit the 'best' exponential model (Equation 4) through the data points of the semi-variogram. Equation 4 has three freedom parameters: the sill c, the range r, and the nugget n. By forcing the nugget equal to zero, the search space reduces, resulting in a shorter computation time. We verified this assumption by a visual check on the semi-variograms, where indeed the nugget was close to zero. We added this information to the manuscript."

8.) P8633L24: Again why not study the effect of the CV of throughfall as well, as according to the introduction this is the main difference with the Hopp and McDonnell paper. Furthermore, why keep the CV of throughfall constant when slope and storm size change (which in reality isn't true)? Explain this better - or preferably include additional simulation results.
Please see comment 1 of the general comments.

9.) P8634L21 and P8643L4: Weiler and Tromp van Meerveld 2008 showed that it was crucial to run their hillslope model for longer time periods and that including preferential flow changed the saturation pattern at the soil-bedrock interface (and will thus change depth-average soil moisture patterns). You should acknowledge these

results somewhere, even if preferential flow pathways aren't included in this model and you simulate a single storm event.
We will add this reference and discuss their findings.

10.) P8634L28: The data is given in the WRR datanote and available online. James et al 2010 used this data in their hillslope model but showed that using this small core scale data did not lead to reasonable model fits. There are good reasons not to use small core scale data in a hillslope model but these should be given here rather than stating that the data is not available.
Thank you for this suggestion. We will change the revised manuscript accordingly and add reference to James et al. (2010) and Tromp-van Meerveld et al. (2008).

11.) P8635L26: Is Qbar the average of Q in time, or in space? Please explain.
$\bar{Q}$ is the average Q in time. We clarified this in the revised manuscript.

12.) P8636L14-15: Which soil moisture pattern is described here? Depth averaged? Shallow? For what time? Rewrite this sentence/section as it is not very clear.
In this section we refer to Figure 5 and we describe in L14-15 the soil moisture pattern of layer 5 (bedrock interface). Especially, at t=56h and t=190h this is visible. We will rewrite this section for clarification.

13.) P8636L17-21; figure 5: Soil moisture in model layers that all have different thicknesses and are thus located at different depths below the soil surface are compared. A layer may be located at 20 cm or at 120 cm below the soil surface depending on the local soil depth. How can you then still compare soil moisture at different locations, and make sure that you are not comparing different things? How much of the pattern is due to the fact that these model grid cells are located at different depths below *the soil surface vs how much is a true difference in for example shallow (0-10 cm or 0-30cm) soil moisture? This is not clear.*

It is indeed true that the soil moisture layers presented in Figure 5 are not at the same absolute depth. This is why we studied depth average soil moisture. We agree, that this has an influence on the results. In the revised manuscript we will discuss this issue.

14.) P8637L15: It is unclear how these results will be different for shallow or deep soil moisture. Do you expect shallow (surface) soil moisture to be dependent on bedrock topography? And deep soil moisture (near the soil bedrock interface) to only be dependent on bedrock topography when there is significant (lateral) drainage and ponding on top of the bedrock interface? Make this distinction between shallow, deep and depth average soil moisture patterns and responses.

We will add a figure where we show the geo-statistical hydrograph of the shallow, mean and deep soil moisture patterns and discuss the results.

15.) P8638L1-2: Does it asymptotically reach this range or does it start to deflect again if you wait longer/when lateral flow and ponding at the soil-bedrock interface have disappeared? Does it reach it more quickly for the shallow layers than for the deeper layers? What if the simulations lasted much longer and the soil dried out and ponding at the soil-bedrock interface disappeared? Does it start to decrease again? I do not expect soil moisture to be so dependent on the bedrock topography if there is no longer any saturation at the soil-bedrock interface. You should run your simulations for a longer period to test this. (see also the next comment).

In our model we did not included evaporation or transpiration processes. Hence water can only leave the domain by drainage. As a result, a part of the soil water will be stored in the bedrock depressions. In reality, this won't be the case due to the fact that trees will use this water for transpiration. We will emphasis this in the revised manuscript.

16.) P8640L1-2: But what if the model was initialized for a much longer time and there was no more lateral flow and ponding at the soil-bedrock interface? Would the results have been different? Discuss if this is indeed a result of the wet initial conditions and if it would be different if the hillslope was much drier.

See reply to comment 15).

17.) P8640L3-8: Did these studies look at shallow or deep soil moisture or profile average soil moisture? Include this information as it will make it clearer how these results compare to your results.

Liang et al. (2007) and Sansoulet et al (2008) looked at soil moisture at several depths until ca. 50 cm deep. At all depths they noticed the influence of the hotspots. Raat et al. (2002), Jost et al. (2004), and Shachnovich et al. (2008) looked at profile average soil moisture up to a depth of 8 cm, 60 cm, and 150 cm respectively. We added this information to the manuscript.

18.) P8640L12-14: Weren't these results for summer when the soil was much drier than in these simulations and soil moisture was mainly influenced by ET? Also, it seems that that was a result of differences in soil depth, not bedrock topography per se. It is important to make the distinction between the effect of soil depth and bedrock topography. While they are related, they represent and influence different processes. More careful wording is thus needed (here and throughout the text).

We agree, and will discuss the effect of soil depth throughout the text.

19.) P8641L5: This requires more careful wording as this result depends on the time scale and season. Throughfall may be dominant when we look at the time scale of a few hours (in winter) while ET may be more dominant if we look at the time scale of a *few weeks (in summer).*

We will elaborate on the effect of time scales. Indeed our study only shows that throughfall is important for soil moisture patterns at the short time scale (and by neglecting the influence of transpiration). During summer time the effect might be less clear due to transpiration, but at the time scale of a few hours the effect of hotspots remains observable (Liang et al, 2007; Sansoulet et al, 2008).

20.) P8643L20: I don't agree that this is a large control. You acknowledge this on P8642L19.

We agree that interception does not have a large influence on SSF and will remove this from the manuscript and more carefully describe our findings.

21.) Figure 10: How different are these results for the different soil layers? Is the deepest layer most affected by bedrock topography? Do the shallower layers respond earlier? Also perhaps flip the y-axis of this figure so that what you call a peak in the text is a peak (rather than the dip) in this figure. Plus add the scale for the precipitation.

Figure 10 will be replaced by a new figure where the geostatistical hydrograph of shallow, deep, and mean soil moisture will be presented (see comment 14). We will also change the direction of the y-axis and add the scale of the precipitation.

22.) This paper is generally well written but needs some editing. Some of the sentences aren't very clear and there are several 1-2 sentence paragraphs that should be combined with other paragraphs.

We will combine paragraphs that only consist of 1-2 sentences into larger paragraphs.

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