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Hydrology and Earth System Sciences Discussions

Interactive comment on "Temporal variability of subsurface stormflow formation" *by* P. M. Kienzler and F. Naef

P. M. Kienzler and F. Naef

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Thank you very much for your constructive comments. We have revised our manuscript Temporal variability of subsurface stormflow formation" according to your recommendations. The specific and detailed comments were helpful for revising and improving the manuscript, and we made a strong effort to deal satisfactorily with all points raised.

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This is a manuscript reporting on detailed observations on subsurface and surface runoff generation at the hillslope scale. The content of the paper suits well into the scope of Hydrology and Earth System Sciences and is of high novelty. I suggest accepting the paper after some minor improvements:

I Please explain, why the antecedent moisture values for the second experiments are not much higher than before the first experiments. Is it possible to apply a closed water balance for the period from before the first experiment until the beginning of the second experiment, and ongoing until the end of the second experiment? This might be a rough calculation, but you could show a summarising table containing the rates and volumes of the different fluxes and stores of the hillslope scale water balances. This might also explain some losses to the underlying bedrock.

We added water balance estimates for each experiment to Tables 1 and 2. The reasons for the fact that antecedent moisture values for the second experiments were not much higher than before the first experiments at Lutertal and Schluessberg are explained in the "Antecedent precipitation and SSF formation" - section.

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II Fig 3, right pictures: what do you consider soil drainageWhat actual process is shown here, in what dimensions? Does it make sense to put the dynamics of soil moisture, soil suction, piezometric head etc. in the same figure? Their relation is highly non-linear

We clarified and changed axis title, caption and explanation of Figure 3. It is correct that the relation between soil suction, soil moisture content and soil water table is highly non-linear. Nevertheless, to our opinion it can make sense to put these three parameters in the same figure. The aim is not a (intra-) comparison of these three parameters at one site, but is an inter-comparison of the three sites.

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III the readability of the paper should be improved. In the present version the results are presented in sections 4 and 5 in a rather descriptive manner by explaining the different observations in detail and discussing possible occurrence and interactions of/between different processes for different antecedent moisture and irrigation intensities. It may already help to insert a sub-sectioning into section 5. In addition, I suggest

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including some scheme of the conceptual model underlying these processes and their interactions. Fig 7 is partly such a scheme. However, some processes are missing. - e.g. drainage below the hillslope lower boundary (e.g. is water infiltrating into the rock fissures ?) - why does the infiltrated water percolate through the macropores during low-intensity and not during high intensity ? What are the anticipated mechanisms for those observations. This many also help to explain the surprising results of the Schluessberg hillslope (the water almost entirely remains at / near the surface for high intensity sprinkling). Why? - What are the anticipated effects controlling surface infiltrability, resulting in less percolation (and/or bypass flow?) for high intensities? Did the high intensities cause soil siltation effects ?

We introduced a sub-sectioning into sections 4 and 5 as suggested and differentiated more clearly between results and discussion. We adapted Figure 7 and added missing processes. We extended also the caption of Figure 7 and the explanation in the text to clarify the surprising results of the Schluessberg experimental slope.

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IV The conclusions ref. possible future applications of the gained knowledge are very limited, e.g. (how) can this knowledge be introduced into catchment scale rainfall runoff analysis?

In the "conclusions" section, we added more details about what the study implies for catchment scale rainfall runoff analysis.

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V The text within the abstract is a controversially: Lines 11 - 13 say: Formation of subsurface stormflow was hardly influenced by the increase of precipitation intensity This sound rather opposing to the text in lines 15-17: This implies that timing and magnitude of flow response can change substantially at different precipitation intensities Please reword these statements in an unequivocal manner.

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We rephrased the abstract to avoid controversial statements.

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