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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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General Comments The paper analyzes a sequence of well defined experimental hillslope studies to better understand the controls of subsurface stormflow. To my knowledge, there have been only a few attempts in the hillslope hydrology community to compare different sites and to derive the precipitation intensity and antecedent mois-

ture controls. In this respect, the paper is unique and certainly worthwhile publishing in HESS. The paper is well written, methods and results are well structured. I personally would prefer to separate results and discussions, but I can see the benefit in this paper for combining the two sections. I recommend the paper to be accepted after the authors have addressed the points listed below:

1) In the abstract you mention type of SSF formation. You explain these types in the manuscript, but it is not clear in an abstract that should be self-explanatory what this is?

We followed this point and changed the abstract to "...SSF response depends on how SSF is formed at a particular hillslope."

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2) In the paper the authors use frequently the term base of the soil. I think a term like soil-bedrock interface may be more appropriate. Similar to the term saturation from below. I would suggest using the term saturation above bedrock or saturation above a low permeable layer.

We changed the terms as recommended.

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3) In the introduction, the authors focus on SSF in shallow lateral preferential flow pathways. I agree that this pathway is an important SSF runoff generation process, but I also recognize that they describe pathways in there study (e.g. im Sertel) that are different. Why did the authors focus only on these flow pathways?

We added a general definition of pipes and lateral preferential flow paths to the introduction and detailed the description of lateral flow paths that were found in this study.

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4) I would not say that we have a limited understanding of how SSF is formed (L22).

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We know quite a lot, but we have not be successful in developing some theory or some classification to predict what kind of SSF mechanism we can except at a particular hillslope.

We followed your recommendation and changed the "limited understanding" to a "lack of a conceptual framework to predict which intensity of SSF can be expected at a particular hillslope in terms of flow rates and timing of the flow."

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5) The authors claim that the antecedent soil moisture increased substaintally at the sites (p2148, L23). According to the values in table 1, the differences are quite small. Could you also explain how the antecedent soil moisture was calculated?

Only at Im Sertel the antecedent soil moisture content increased substantially +44 mm), not at the other sites (+15 - 20 mm). We changed the words to make the point clearer.

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6) According to Table 1 and Fig 3, I cannot see how nearly all precipitation was retained in the subsoil at the site im Sertel (p2149,L6). Could you please clarify?

We clarified the point in the text and added water balance estimates to tables 1 and 2 for each experiment. The fact that water was stored predominantly in the subsoil during the mentioned experiment is visible in Figure 6. We added a reference to Figure 6 in the text.

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7) I also think that it would be very informative to provide a simple water balance calculation for each experiment. I have the feeling that substantial quantities are not accounted for in your measurements and that these amount a losses into the bedrock. Since you know precipitation amount, total runoff, changes in soil moisture you can

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calculate the losses into the bedrock (including some error estimates if necessary). I believe that this would reveal some interesting patterns. Based on my experience, the assumption that the bedrock is impermeable is usually incorrect.

We added water balance estimates to tables 1 and 2 for each experiment that include an estimate of losses into the bedrock.

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8) You need to provide more details how Fig 5 was derived and what the reader should see in this Figure. Can you really use these calculations to explain changes of matrix and macropore infiltration?

We extended the explanation of Figure 5 and provided more details.

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9) It may be interesting to speculate about the effects of lower precipitation intensities, which are more common; on SSF formation based on you detailed knowledge about these sites. Any thoughts?

As it can only be speculated about the effects of lower precipitation intensity, we would like to omit this point. Most probably rainfall would just percolate into the bedrock thus preventing the initiation of lateral subsurface flow.

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10) You may point out in you discussion about OF and flow in the saturated topsoil (p2152, L 10-15) that these two flowpathways, as you could show with the hydrograph separation, are probably not separated, but they form an mixed pathway where water flows in and on top of the soil that is usually considered as OF flow, despite the water is not solely flowing on the surface of the soil.

We integrated this idea and pointed out the fact that overland flow and subsurface flow within saturated topsoil are not separated but form one well-mixed flow component.

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11) On page 2152 you mention deep subsurface flow. According to your descriptions, this should still be shallow subsurface flow. I would consider flow in a depth of several meters as deep SSF.

We changed the terms shallow and deep SSF to topsoil SSF and subsoil SSF to make more clear the specific origin of the flow.

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12) Please explain in detail how soil drainage in Fig 3 was calculated. What is considered to be 0%. How many TDRs, tensiometer and piezometers were included into the calculation? I would not call this soil drainage, it is more a relative change. Why is there a response at Schluessberg at 7 hours after end of the experiment?

We clarified and changed the caption and explanation of Figure 3. Figure 1 depicts the number of probes included for the calculations.

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13) In the schematic in Fig 7, you should still include SSF as a flow pathway. As you could clearly show, SSF is usually not changing when precipitation intensity is increased. So, there is still SSF even for high intensity and not only OF flow.

As subsoil SSF is usually not changing when precipitation is increased, there may still be SSF even for high intensity. However, as large volumes of water flow off as overland flow and topsoil SSF, the initiation of subsoil SSF is considerably delayed and therefore not accounted for in this scheme.

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Specific and Technical Comments 1) Change P2145,L21 to: In a similar way, increased precipitation intensity could lead to a subsequent faster onset of SSF because of a switch from matrix infiltration to macropore infiltration 2) Please specify the TDR and

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tensiometer set-up that was used (p2146,L26). 3) Please provide a reference for the Radon-222 methodology (p2147,L3) 4) Remove repetition were situated in the Swiss Plateau (p2147.L12) 5) Please provide details about the rainfall intensities used to calculate pre-event water fractions in Figure 2. 6) Please change P2152,L1 to Overland flow was not the result of complete saturation of the soil, as it started while still large parts of the pore volume were not saturated since tensiometers indicated unsaturated conditions. 7) Please explain what sizes of macropres were counted for the macropore density in Table 4. Only vertical macroproes?

We corrected and clarified all the points as recommended.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 2143, 2007.