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Hydrology and Earth System Sciences Discussions

Interactive comment on "The influence of soil moisture on threshold runoff generation processes in an alpine headwater catchment" *by* D. Penna et al.

Anonymous Referee #1

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In this paper, the authors examine the influence of soil moisture content on runoff triggering processes. Overall, the paper is interesting, and reads fluently. I have only a couple of comments that I recommend the authors to address, because I believe it would help to place the paper more in context with other, related research, and would help to clarify some items in the paper.

- I am missing a discussion on the variable source area concept. I am sure the authors are aware of this concept, and it seems to be related to this research. I would recommend a discussion of this concept in the introduction, and a discussion of how

the results in this paper relate to this concept.

- Also, there has already been performed quite a bit of work on model reinitialization using remote sensing soil moisture data ("data assimilation"), for the improvement of modeled discharge peaks. This goes back for about a decade. Lately, more research seems to be done on this subject. Perhaps it would be a good idea to also discuss this research in the introduction, because it seems to be related to the results in this paper.

- Section 3.2. It is stated that the average of the four probes was in good agreement with the average of the measurements at the 26 points. We really need proof of this, because it is a central result for the remainder of the paper. I would recommend a table or graph (or both), where this statement is substantiated.

- Section 3.3. It is stated "baseflow was substracted from total flow to obtain the value of event stormflow. Event runoff coefficients were defined as total stormflow (in mm) divided by the rainfall. A couple of questions come up here. First, how was the baseflow estimated or measured? Please explain this, because the results and the conclusions are probably going to be very dependent on this. Further, this implies the assumption that there is no contribution from the ground water table to the runoff, because the rainfall leading to base flow is not substracted from the total rainfall. This does not seem very realistic, and is going to have a strong influence on the results further in the paper. The authors either need to justify this methodology, or to reprocess the rainfall data as stated above.

These are the remarks I would like to see addressed in the paper. Overall, I think it is a very good paper, that should be published.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 8091, 2010.