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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Effective rainfall: a significant parameter to improve understanding of deep-seated rainfall triggering landslide – a simple computation temperature based method applied to Séchilienne unstable slope (French Alps)" *by* A. Vallet et al.

Anonymous Referee #1

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First of all, thank you for this interesting paper. As the title says already, it connects hydrological and hydrogeological objectives with topics of the engineering geology. I'm not an expert in landslides therefore my comments mainly reflect the aspects of effective rainfall in the proposed paper. The title shows the dilemma already: The effective rainfall is the main topic of the paper and landslide aspects are only of minor interest. The improvement of the understanding, how deep seated landslides work, and what

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effective rainfall and landslides have to do with each other is nearly not regarded. The question is more, if the paper is a valuable contribution to the understanding of effective rainfall processes and process quantification. In this terms the paper proposes some kind of effective parameter calculation on a local scale which cannot easily be transferred to other sites and additionally it may not be helpful for the investigated site, too. In terms of relation to deep seated landslides many aspects are not regarded. For the process understanding of effective rainfall a clear process description is neither given nor cited. The summary of interception, direct runoff, infiltration, transpiration in one calculation based on radiation and potential or actual evapotranspiration calculation with an additional regarding of AWS alone is a bit too weak for the explanation of an influencing parameter of deep seated landslides. On the other hand, correlations are very robust and it is possible, that simpler calculations may lead to similar results - if the message of the paper should be the inclusion of evapotranspiration into the landslide calculations. Let's go into details, step by step. The abstract is correct but it shows already some weaknesses of the paper: There are already numerous proposed methods, only the lack of data makes it difficult to use these methods at the investigated site. Usually one of two ways is taken to overcome that common problem (we never have enough data): Measuring or substitution by modelling. The authors choose the second way but with a technical substitution of one parameter (radiation) by a function (dependent only on temperature). This seems to be clever, but the question is, if it is reasonable and helps scientifically and not only technically. The concepts even for the temperature calculation (which is not measured on site!) remain on a low statistical level and they are not connected to the local conditions (a height-dependency curve of the temperature with the weather stations around the site would be very helpful). The distribution of vegetation, the exposure to solar radiation (on a hillside!), the different depths to groundwater are not described clearly and the evident question remains, if a daily calculation of the actual evapotranspiration and effective rainfall is necessary or if a monthly (or weekly) calculation based on the simple and only temperature dependent method of Thonthwaite (1948) is not sufficient, perhaps with a daily calculation of

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daily rainfall minus average monthly/weekly evapotranspiration? This additionally focuses the question of the influence of the unsaturated zone: Normally the averaging of (effective) rainfall/recharge impact increases with depth to groundwater. At a site with depth to groundwater of several 10 m to several 100 m as described in the paper, the recharge should be averaged to a weekly or even monthly constant rate. Of course the described high conductivity of fissures and fractures leads to an overcompensation of the averaging effect, but this is not directly connected to evapotranspiration, as is also shown in the only minor effect on correlation coefficients (0.8 with effective rainfall instead of 0.66 with rainfall only). The statement of better performance of PMred ET0 compared to the other tested methods should be stated by numbers in the text, too, not only in Table 3. I think that the range of deviations is quite small, perhaps marginally. It also remains open, if a correlation of the data of one of the weather stations with full dataset (and therefore without any technical substitution, just based on the equations of FAO etc.) would give the same (or better?) result as all the local adaptation. The surface runoff also is regarded only on a statistical base and not based on measurements or process model assumptions. Even an explanation of the result (3% instead of 14% as explained before) is missing. Table 4 is unclear to me: What do the numbers in brackets say? Does the geology at the surface change with the percentage of PIS? Fig. 5 should be colorized, especially the graph with effective rainfall and rainfall. It would be good to have in fig 9 the detrended displacement in each of the other three scenarios as a colored curve and either R^2 or the correlation coefficient given for each scenario. My overall impression of this paper is that there are difficulties to structure the text thoroughly (some explanations are double, the reference to figures and tables can be improved), the calculations are not transparent enough (effective rainfall yes, but the other influencing factors not) and the statistics are only rudimentary done. Major changes are absolutely necessary in the outlined fields and additionally the data may be made available via electronic supplementary material, if possible in accordance with legal aspects.

Thornthwaite, C. W. (1948). "An approach toward a rational classification of climate".

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 8945, 2013.