Hydrol. Earth Syst. Sci. Discuss., 10, C3310–C3313, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C3310/2013/

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.

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Received and published: 21 July 2013

SHORT COMMENT

I have been very interested from the paper by Vallet et al. not only for the topic, i.e. the understanding of deep-seated landslide triggering, but mainly from the approach proposed by the authors. In fact, the use of effective rainfall, instead of total rainfall,

as important parameter for predicting deep-seated landslide movements is particularly interesting (at least for me). The use of effective rainfall allows to incorporate in a single parameter the effect of rainfall and antecedent wetness conditions that are the two main factors of landslides triggering. This approach can be employed not only for deep-seated landslides, but also for the prediction of shallow landslides triggering that is mostly addressed through the rainfall intensity-duration curves. Also in this case, the effective rainfall can be used in place of total rainfall with possible improved results. I suggest briefly mentioning and underlining these aspects that might further improve the significance of the paper.

Another important aspect of the paper is related to the quality of the displacement dataset. The availability of a long-term daily and continuous dataset makes the analysis very robust and reliable. A possible suggestion to the authors could be to make the dataset freely available (or at least in part) to allow other researchers to investigate different approaches for the prediction of the movement of the Séchilienne landslide. In fact, the availability of these datasets, also for other landslides, would permit to study the triggering of deep-seated landslides located in different regions worldwide, thus making results more and more useful for real applications.

However, I also found some general issues that should be addressed.

1) Among the references to studies investigating the deep-seated landslide movements, some recent and important contributions are missing. I have reported here a few of them that might be included in the paper (*Belle et al., 2013; Brocca et al., 2012; Corominas et al., 2005; Hürlimann et al., 2006; Ponziani et al., 2012; Prokešová et al., 2012*). Specifically, these studies employed measured groundwater level or soil moisture data as parameter for predicting the landslides movement. These approaches can be considered similar to the ones proposed in the current study and, hence, are relevant for the paper.

2) The proposed method is not as simple as advocated by the authors in some parts

of the paper (e.g. last sentence of the abstract). It is not clear which are the final equations used for obtaining the results in Figures 7-9. Also the parameter values for the computation of the soil water balance shown in Figure 5 are not specified. Moreover, it is not clear how the proportion of preferential infiltration structures (PIS), the AWS and the runoff coefficient are estimated from geological, geomorphological and topographic characteristics of the area. I suggest better specifying these aspects with also a final summary/scheme of the method.

3) Finally, as hydrologist, I believe that the method used for the estimation of runoff is quite crude. A lot of emphasis is given to the estimation of the evapotranspiration even though it reads: "Runoff processes play an important role in effective rainfall computation" (page 8951, first sentence). Likely, the method used for runoff estimation should be revised.

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