

Interactive comment on “Snow Accumulation-Melting Model (SAMM) for integrated use in regional scale landslide early warning systems” by G. Martelloni et al.

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All reviewers came to the conclusion that the paper presents a potentially interesting method but the paper needs important revisions to be considered for publication in HESS. First of all, the method needs a better description, with a clear presentation of the mathematical equations before discussing their numerical implementation (time stepping scheme). I agree with the reviewers that this is essentially an improved temperature-index approach with a high number of parameters, which seem difficult to estimate based only on point snow height observations. How can such a high number

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of parameters be related to a single driving variable (temperature) and a single output variable (snow height)? The performance gain with respect to a simple temperature-index approach should be discussed. As also pointed out by the reviewers, the results are presented in a too condensed way and the improvement of the landslide early warning system needs a better evaluation procedure.

Some additional comments to the points raised by the reviewers:

- I would like to ask the authors to not just discuss the sensitivity of the parameters but also their uncertainty: given all the available stations, it seems unjustified to simply calibrate the parameters on one station and test on another station. A more sophisticated method of cross-validation including all available stations and possibly not just the optimum parameter set (see the huge uncertainty literature in hydrology, e.g. the work of K. Beven or J. Vrugt) is required here to judge whether the method is really useful. Without a proper analysis of the performance of the method for snow simulation and its value for landslide early warning, the paper does not fulfill the standards of a HESS paper. This performance analysis should explicitly report if the identified parameter values do a good job for similar stations (e.g. similar altitude, exposition) or for all stations and, if not, whether you suggest that different parameter values for different stations should be used.
- The presented melt model should be properly referenced; which parts correspond to existing approaches (e.g. the temperature-index approach), which parts are completely new (e.g. has someone proposed a similar power relationship between melt and temperature?).

Detailed comments:

- p. 9397, line 13: should it not read "precipitation H_w " instead of "rain"?

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- p. 9398: references for the rain / snowfall separation approach? who proposed the exponential relationship? what is the value of comparing the proposed relationship to an old study without giving details on how their values were obtained (region, method, model or observations etc.)?
- eq. 7: ρ_0 should read ρ_{so} ?, who proposed this equation before?
- p. 9399 and elsewhere: I suggest not using synonyms for snowpack (i.e. do not use words such as "mantle"), I would also stick to snowpack depth instead of height

I would like to ask the authors to give a separate, detailed response to each of the reviewers' comments and to the present editor review before preparing a revised version.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 9391, 2012.