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Hydrology and Earth System Sciences Discussions

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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We would like to express our gratitude to the Editor. Her comments were very useful to prepare an improved version of the manuscript.

Answers to the general comments of the Editor:

We improved the description of the model, providing a clearer presentation of the mathematical equations and of the physical meaning of the 13 parameters (we added this information in Table 1). We also added some more references, as suggested in the detailed comments. Similarly, the "results and discussion" section was extended with

a more complete and detailed validation of the results, both for SAMM (see specific comment hereafter) and for the landslide warning system (as suggested also by the referees). We also agree that our model represents an improved temperature-index approach. The main improvement is that we added the conservation of mass equation to better constrain the computing of the Snow Water Equivalent with respect to rainfall effect, temperature and compression of the snowpack. This has been explained in the revised text, following also the suggestions of the 3 referees. As for the comparison with temperature index models, some authors report that improved temperature index models have markedly better performances than the basic implementations (Jost et al., 2012). In addition, it has been observed that the accuracy of simple temperature index models decreases with increasing temporal resolution (Hock, 2003). These are the two main reasons that led us to develop an improved temperature index model.

Hereafter, we address some additional comments:

COMMENT:

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.

ANSWER:

We performed additional numerical simulations to answer this important issue. We retrieved all the data required for the model calibration (snowpack thickness, rainfall, air temperature) for all possible measurement stations (namely, six). We performed a distinct calibration for each of them, obtaining a specific set of values for the 13 parameters. We compared those values and put them in relation with elevation and aspect. The values of some parameters show a very limited variability from a measurement station to another. Some others have a more marked variability which can in part be explained with the different aspect of the measurement station. In the discussion, we explain and discuss these outcomes. Moreover, we performed a cross-validation: each set of parameters obtained with the calibration of a specific station was applied to all the remaining stations and errors were calculated. Results show that the best performances are obtained using the optimum configuration previously identified in the paper (Doccia di Fiumalbo station). The other configuration sets provided similar results, excepted for the two stations with a southerly aspect (in which mean absolute error is increased by about +50%). We prepared a table listing, for each station, all the parameters values, their range, the percentage of variability, the aspect and elevation and the mean absolute error observed in the validation. Moreover, we added to Table 1 a column in which we shortly explain the meaning of each of the 13 parameters.

COMMENT:

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?).

ANSWER:

We improved the description of the model. In particular, we properly referenced all parts corresponding to existing approaches (e.g. reference for the relationship between new snow density and air temperature; definition of a temperature threshold; relation-

ship between melt and temperature). We better highlighted a novelty of the proposed snow model: the processes linked to the accumulation/depletion of the snowpack (e.g. compression of the snowpack due to newly fallen snow and effects of rainfall) were modelled identifying limiting and inhibitory factors according to a kinetic approach.

DETAILED COMMENTS:

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

OK

-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.)?

Instead of making reference to Fig. 3 (which was deleted), we provided additional explanations and references. Various techniques and different approaches have been employed in attempt to explain meteorological controls with new snow density. Bartlett et al. (2006) use an exponential relationship between new snow density and air temperature developed by Hedstrom and Pomeroy (1998) from the data of Schmidt and Gluns (1991) and the US Army Corps of Engineers (1956), where: S0= 67.92 + 51.25eTa/2.59 Gustafsson et al.(2004) underline the presence of a threshold temperature below which precipitation turns from pure rain to snow, and they include it in the equation above, considering also the fraction of liquid water in mixed precipitation defined by Jansson and Moon (2001).

-eq. 7: _0 should read _so?, who proposed this equation before?

Yes. Eq. 7 expresses the average density of the snowpack as a weighted average of the density of snow in the previous time interval and density of the newly fallen snow. As for the compression term, we identify snowpack height (Hs) as a limiter and snow density as an inhibitor, following the above mentioned kinetic approach.

-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

OK

REFERENCES:

Hock, R.: Temperature index melt modelling in mountain areas, J. Hydrol., 282, 104–115, 2003. Jost, G., Moore, R. D., Smith, R., and Gluns, D. R.: Distributed temperature-index snowmelt modelling for forested catchments, Journal of Hydrology, 420–421, 87–101, 2012. Bartlett, P. A., MacKay, M.D., and Verseghy, D.L.: Modified snow algorithms in the Canadian land surface scheme: Model runs and sensitivity analysis at three boreal forest stands, Atmosphere-Ocean, 44:3, 207-222, 2006.

Hedstrom, N.R. and Pomeroy, J.W.: Measurments and modeling of snow interception in the boreal forest, Hydrol. Proc., 12, 1611-1625, 1998.

Gustafsson, D., Waldner, P. A. and Stähli, M.: Factors governing the formation and persistence of layers in a subalpine snowpack, Hydrol. Process., 18: 1165–1183. doi: 10.1002/hyp.1398, 2004.

Jansson, P-E., Moon , D.S.: A coupled model of water, heat and mass transfer using object orientation to improve flexibility and functionality, Environmental Modelling & Software, 16 (1) 37-46, 2001.

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