

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

**G. Martelloni et al.**

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We would like to thank the Referee #3 for the constructive comments. All the criticalities raised by the referee are addressed below point by point.

Comment: The main issue is that the paper is extremely short, particularly in the results and discussion section.

Answer: As suggested by all Referees and Editor, to fully highlight that the performance of the SAMM+SIGMA system is better than the performance of SIGMA alone, we presented the results of the comparison in deeper detail. A table is provided to show the

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confusion matrix (true positives, false positives, true negatives, false negatives) and a series of indexes commonly used to evaluate the performance of similar models (e.g. sensitivity, specificity, likelihood ratio and so on). The table is accompanied by some additional text that describes and discusses these outcomes.

Comment: I agree with reviewer number one that this is not an “intermediate” model. I think it’s a fully physical-based model; thus I would remove that part that does not add any importance to the paper.

Answer: We removed from the abstract and from the introduction the sentence “SAMM follows an intermediate approach between physically based models and empirical temperature index models”, which evidently leads to misunderstandings. In the conclusion, we better explained our point of view, trying to avoid overstatements: “In this regard, SAMM could be considered an intermediate approach between temperature index- and physically based models: from an operational point of view it simply uses air temperature as an index to take into account snow melting and accumulation, but the value of the threshold temperature (as all other parameters involved in the equations) are defined by means of a semi-physical approach, which starts from a conservation of mass equation but uses empirical approximations and calibrations to overcome the unavailability of dynamic data for an operational employ of the model at regional scale”. If this sentence is still found inappropriate, we are ready to remove it.

Comment: In the abstract I suggest to make the conclusions much less specific to the main results of this paper. The abstract should capture the attention of the reader and starting from the beginning to the end I feel I loose my curiosity despite the interesting model presented.

Answer: The abstract was slightly changed according to the suggestions of the reviewer.

Comment: As for the methods I wonder if the author can address also in the discussion how easy is to get the 13 empirical parameters. That is not clear and it is related to the

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immediate applicability of the model. A detailed presentation of the parameters would be also very appreciated.

Answer: In section 2.3 we added the following text: “The presented methodology could be easily applied elsewhere, provided the availability of the data needed for real time implementation (temperature and rainfall) and for the calibration process (historical snow depth measurements)”. We made minor edits to the text, to make clearer that, according to the methodology used in this paper, getting the optimum value of the 13 empirical parameters is very easy since it is sufficient to perform the calibration process described in section 2.3. Of course, to accomplish this task, some experimental data are needed (historical snow depth measurements). Moreover, we improved the description of the model and of the parameters.

Comment: The conclusions section should be shorter and the discussion should be more extended. My same considerations for the conclusions of the abstract apply to the conclusion section. Results are really too short and the reader cannot appreciate the findings of the model. I am surprised by the number of figures and such short results and discussion section.

Answer: We shortened the conclusion and extended the discussion (according to this comment and others comments received from the Editor and from the other Referees).

Comment: Overall, the paper is reasonably well written, but it might gain if revised by a native English speaker that is not expert in this field. This can give that broader perspective and communication ability that the paper lacks.

Answer: The manuscript was revised by a professional English speaker (and we added her to the acknowledgments).

Specific Comments My only advice is to present Fig. 11 much earlier in the text. It does not make sense to have the figure of the model at the end of the paper. As for the other figures, I suggest the authors to increase the size of the fonts of labels. They are

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a bit small to read.

Answer: We considered the advice to present FIG 11 earlier in the text. However, we believe that this figure does not contribute to explain the snow accumulation-melting model. Rather, it helps understanding how SAMM is integrated in the Emilia Romagna landslide forecasting system. As a consequence, we believe that Fig. 11 could be conveniently presented in section “3.2 Integration between SAMM and SIGMA”. As for the other figures, we increased all the fonts to ease a full comprehension.

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

**HESD**

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