

Ref MS No.: hess-2013-472 Modeling the snow surface temperature with a one-layer energy balance snowmelt model

Response to reviewer #1:

*We greatly appreciate Reviewer 1 in helping improve this paper.*

Authors welcomed all the minor issues highlighted by this Reviewer. I would suggest some (very minor) revisions for this second version of the manuscript:

- Lines 78/79: what do you mean with "unmeasurable sub-element scale variability"?

*We have added text to define the term to the remote isolated catchments where it is extreme difficulty to conduct large scale field work.*

- Lines 79-83: the statement is unclear to me. Moreover, "to seem central" is a subjective statement. I would suggest to the Authors to provide references of works which report examples of simple models reproducing snow dynamics with good performances;

*Thank you. We have revised the statement.*

- Line 84: I understand Authors' point of view here, but I would prefer more "neutral" statements. In particular, I think that saying that "Modeling needs a balance between ..." is enough;

*We accept and have revised the sentence as suggested.*

-References: All references are missing.

*Thank you for pointing this out. The references were messed up when we print out the manuscript.*

- Figure 1: maybe some additional indications (e.g., specifying that the upper boundary is atmosphere etc.) could help to enhance this Figure;

- Figure 2: as for Figure 1;

*Thank you for the great suggestion. We have made changes to Figure 1 and 2 as suggested.*

Ref MS No.: hess-2013-472 Modeling the snow surface temperature with a one-layer energy balance snowmelt model

Response to reviewer #2:

I would like to thank authors for their revision, however some important revision is still needed. Some important comments from the first review were not considered but are needed mainly to increase the significance of the contribution. I still feel that using additional data for showing the value of the new approach is essential. The split sample validation of the approach is an important part of hypothesis testing (in this case new parametrization of the model) and for such validation some new (independent) data are needed. The model (new approach) is calibrated for one season (which might be considered even as a short period), but the validation in an independent period is not presented. I would strongly suggest to revise/add such validation to the manuscript.

*We greatly appreciate comments of reviewer#2 in improving this paper. In the revision we have more clearly stated the contribution of this work. This paper has been very long. The model was calibrated against the USU Drainage Farm dataset, tested on the CSSL dataset, and validated on the Niwot dataset. In the paper we also indicate the needs for more complete tests on other datasets, study sites, and more complex environments and settings. In fact, the model has been applied to other sites such as the Reynolds Experimental Farm, Alaska North Slope dataset. The model has also been included in the snowmelt model intercomparison project. Those results have not been included in this paper and will be presented in future work.*

I'm also did not find some response and improvements to the comments concerning the Results and Discussion sections. In the Results, there are presented Figures without a closer explanations what the readers should see and what does it mean with respect to the paper's objectives. For example, on p.32, l.696-698, there is a sentence that the comparisons are shown in Figure 9,10,11 and 12, without any further descriptions.

*Thank you. We have added some descriptions on these figures. More discussion of the results in these figures has been placed in the discussion section.*

In the Discussion section should also be more closely linked with the results of other studies (not only discussing the own results), in order to clearly indicate the added value and implications of the findings. In some paragraphs, it is not clear, how are these related to the manuscript. For example, there is a paragraph showing different values of thermal conductivity, found in other studies, but it there is no discussion on how it is related to the study?

*Thank you. We have modified the text for better linkages.*

*Since this model is the only one layer snowmelt model, it is difficult to find reference model to compare in this paper. We agree with Reviewer #2 that it is necessary to compare the performance of this model with other models. Future work will compare the results from this model with those from other snowmelt models e.g. the snobal. However, such comparisons will not be included in this study since this paper is already very lengthy; instead, this paper only shows the algorithm and improvements over the original UEB to successfully resolve the discrepancies of the original UEB in modeling the internal energy content and meanwhile*

*adequately modeling other variables such as snow water equivalent, snowmelt water, and surface snow temperature.*

Specific comments

There is still some space for making Figures more consistent. Fig. 3 to 8 shows the same time period, but the x-axis is not the same (i.e. tick labels, axis size, line type/color for the same variable, etc.). Also some Figure captions are not self-explanatory, please consider revising them.

*Thank you. We have tried our best to make these figures consistent.*