REF: Interactive comment on "Modeling the snow surface temperature with a one-layer energy balance snowmelt model" by J. You et al.

Response to Anonymous Referee #2
J You, D Tarboton, C Luce
jyou2@unl.edu

We thank this reviewer for his comments. Addressing them has helped improve the paper.

The reviewer commented on the scientific contribution being incremental and that it would be stronger if it was tested and statistically evaluated over a longer time period.

The contributions of this paper are threefold:

1. Evaluation of the modified temperature parameterizations in a complete model
2. Introduction and evaluation of the refreezing parameterization in a complete model
3. Addition of the refinement to adjust thermal conductivity parameters for shallow snowpacks.

Our earlier paper (Luce and Tarboton, 2010) evaluated the equilibrium gradient, force restore and modified force-restore approaches driving the calculations directly with measured temperatures. It did not consider these parameterizations in a free running model driven only by atmospheric forcing. This current paper is the examination of these parameterizations in the context of a complete model. We believe that this is a non-incremental contribution. By themselves, 2 and 3 are incremental contributions, but they are necessary to improve the model as a whole. Collectively these contributions have solved the issue of overestimating the energy loss of snowpack and underestimating the average snow temperature in the original model. After the introduced modifications, the model represents important variables as surface temperature, average snow temperature (represented as internal energy), snow water equivalent, melt water, and albedo quite well. We have revised the paper to more directly present these contributions in the conclusions. We have also added material to the results and discussion to support these conclusions.

The model has been widely tested such as in the Reynolds snow experiment site, Idaho, Toolik site at North Slope, Alaska. The model has also been compared in the intercomparison project as in Rutter et al., 2009. These results were not included in this paper.

With respect to the comment about additional testing on longer data, we have not performed additional simulations with additional data because we are unable to do this in the time frame of this response. We certainly agree that further comparisons against additional data will be of value. It is always good to do more and compare models against more data. However we feel that this paper makes a publishable contribution as it stands and will leave additional simulations to future work.

Specific comments

1) What is the numerical stability of the new approach?

The new approach for surface temperature does not alter the numerical stability. The model has a number of checks for numerical stability. The solution for surface temperature first uses a Newton Rhapson scheme. However it tests for convergence and in time steps (a small percentage depending on the data) when this does not converge, it resorts to a more robust bisection approach that is guaranteed to converge because the equation giving temperature flux into the snow based on surface temperature is monotonic. We have added sentences to the paper to explain this.

2) Please consider to unify the figures (i.e. by using the same layout - axes limits, labels, etc). It will be easier to compare the results of different plots for the same station.

We have revised the figures to enable better comparisons.