Authors response "Modeling the snow surface temperature with a one-layer energy balance snowmelt model"

We thank the reviewers and editor for their comments on this paper. The revised paper has addressed reviewer comments and a detailed point by point response to each individual review has been given.

Summary of the more major elements of the responses to reviews.

One review comment was that "the time of simple snow models is over." We disagree with this. We certainly laud all the work that the reviewer making this comment and others are pursuing using layered snowmelt models. However modeling is an art involving the balance between representing details that are important to the purpose or question being addressed and avoiding complexity and inaccuracy for details that are less important. There is no one right solution. In this paper we examine and evaluate single layer solutions that avoid some of the complexity of multilayer models for our purposes which are the quantification of overall surface energy exchanges and meltwater produced by a snowmelt model for hydrological studies. The UEB model presented in this paper takes a physically based approach to the modeling of snow surface temperature. This is, in our evaluation, an advance that is useful for modeling watershed snowmelt inputs as it provides a parsimonious way to address important energy balance processes. The energy balance approach is an advance over temperature index models, but does not embody the detail and complexity of layered models that are more appropriate for more detailed studies. We note that there are recent papers in HESS that use even simpler temperature index models such as SNOW-17 in operational applications (e.g. DeChant and Moradkhani, 2011;He et al., 2012;Miller et al., 2011;Kim and Kaluarachchi, 2014). The UEB model presented in this paper may offer a parsimonious alternative that advances the physical basis for such operational applications, where using layered models may be prohibitive.

One review commented that the contribution was incremental and that it would be stronger if tested and statistically evaluated over a longer time period, in other words tested with more data. Incremental improvements are important. The contributions of this paper are:

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

Individually each of these is an incremental improvement. Collectively they solve important energy balance modeling problems and advance snowmelt modeling capability.

With respect to evaluation over a longer time period or testing with more data, we certainly agree that further comparisons against additional data are always valuable. However we note that the time and effort to do this may be considerable and in this case not feasible in the time frame of responding to these reviews. We also note that many journals (HESS included) encourage brevity in papers and additional testing would add to the length of the paper. We note that the model code is freely available and because of this it has already been used by others in model intercomparison studies (e.g. Rutter et al., 2009) and that further such evaluations are anticipated in the future. We feel that this paper makes a publishable contribution as it stands and that it is most appropriate to leave additional testing to future work.

Response to Anonymous Referee #1:

We thank reviewer 1 for his comments. Addressing them has helped improve the paper. We have addressed these comments in the revisions that we have submitted.

Minor revisions:

1. In the first lines of the abstract, I would stress the coupled dynamics of snow surface temperature and energy exchanges in a more explicit way;

We accept the suggestion and the abstract has been revised to express the coupled dynamics of snow surface temperature and energy changes in a more explicit way.

2. You could consider splitting Section 2 in subsections, since it could orient the reader in this great amount of relevant information;

We accept this suggestion and have reorganized the section 2.

3. In Section 4, it could be useful to provie some details about the resolutions and the accuracies of the instruments used;

We have added some details about the resolutions and the accuracies of the instruments used.

4. Lines 4-26 page 15092 seem out of context here, since they mainly deal with previous results;

We have removed the text referring to previous results.

5. Fig 4, 5 6, 7, 8, 10, 11, 12, 13 and 14: please consider using colors to differentiate lines, since at this stage it is very difficult to discern the different results;

We have revised the figures using colors.

Response to Anonymous Referee #2

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.

Response to M. Lehning (Referee)

The authors thank Dr. Lehning for his comments. We have addressed those that can be addressed and these have improved the paper. 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.

Dr. Lehning's major comment was that "the time of simple snow models is over". We fundamentally disagree with this. We certainly laud all the work that Dr. Lehning and others are pursuing using layered snowmelt models. However modeling is an art, involving the balance between representing details that are important to the purpose, or question being addressed and avoiding complexity and inaccuracy for details that are less important. There is no one right solution. In this paper we examine and evaluate single layer solutions that avoid some of the complexity of multilayer models for our purposes, which are the quantification of overall surface energy exchanges and meltwater produced by a snowmelt model for hydrological studies.

We have added several sentences to the introduction to explain the utility of simple models in the broad context of research on snowmelt models. Philosophically, we argue that there is utility in parallel development of lumped and discretized approaches. Ultimately any model has to make choices about the basic model element used for its computation, so whether an approach is layered, or single layer, or lumped or distributed amounts to a selection of scale with which variability can be explicitly represented. Not all variability can ever be fully explicitly represented. We do not think that publication of single-layer models should be opposed simply because they represent the bulk conduction processes differently than multi-layer models.

While there is clear value in discretized models for being able to demonstrate the effects of heterogeneity, it can be as difficult to identify values for the parameters they require as it is to develop a lumped parameterization of the net effect of the heterogeneity. This paper shows validation of implementation of previously discussed solution to the 1-d heat flow equation under sinusoidal forcing. In that earlier paper, we demonstrated the utility of the general equations and principles involved in analyzing data to estimate snowpack properties as well. While a "simple" model can be inverted in a way that is useful for potentially tracking model parameters over time. Interpreting highly discretized models with depth varying parameters would be substantially more difficult and potentially subjective. In addition better development of lumped approaches could lead to more accurate modeling for layered models, which, after all, rely on layers with finite thickness, not infinitesimally thin ones.

We use a parameterization for conduction into the snowpack that is nominally as valid as a finite difference scheme (without having to worry about numerical dispersion) using parameters that can be independently identified outside of a calibration process. Although the layered models are able to represent vertical heterogeneity explicitly, it is unclear whether all of the modeled heterogeneity in snowpack properties is consistently correct. The lumped parameter conduction model replaces many parameters on grain and layer development with a single parameter for thermal conductivity. While we may not present results demonstrating that the algorithms presented here are better than the layered models, there is not a parallel publication unequivocally demonstrating that the layered models are fundamentally better at representing conduction and melt-refreeze than this model either.

There are also other complexities that are not addressed in the layered models that Lehning refers to. There is an increasing realization that lateral inhomogeneities in snowpacks are important (e.g. Wankiewicz, 1979;Higuchi and Tanaka, 1982;Kattelmann and Dozier, 1999;Williams et al., 2010;Eiriksson et al., 2013). These inhomogeneities result in lateral variability across a range of scales and fingering in the way that meltwater enters and flows through snow that is different from the matrix flow represented in one-dimensional finite difference solutions. This suggests that even our most complex snowpack models must seek a way to parameterize unmeasurable sub-element scale variability. In the single layer approach we have taken we strive to model the factor that we think is of most importance, the surface temperature that provides the connection between the snow and the atmosphere above, while avoiding the complexity of processes that do not seem central to getting the big picture of the energy exchange right.

Dr. Lehning also commented that this paper was not a general evaluation of the force-restore method which has been done earlier, but was limited to the freezing front parameterization and model validation. We have revised the abstract and conclusions to make the contributions of this paper more clear. In fact, while our earlier paper (Luce and Tarboton, 2010) evaluated the equilibrium gradient, force restore and modified force-restore approaches it did this driving the model with measured temperatures and 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. The contributions of this paper are thus threefold:

1. Evaluation of the modified surface temperature parameterization 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.

By themselves, 2 and 3 are incremental contributions, but they are necessary to improve the model as a whole, which is why they were introduced in this paper. 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.

We appreciate the detailed comments added directly to the pdf of the paper and have addressed these as follows (page and line numbers refer to the supplement to Lehning's review):

15072, line 2. First sentence modified.

15073, lines 18-20. We have expanded the discussion here to clarify our logic.

15074, lines 6-7. We have revised this sentence. We have added citations to Wever et al and CROCUS in our revised discussion (that appears above this point) of model complexity and the basis for our approach.

15080, line 1. We do not have measurements that are sufficiently discerning to allow us to comment on whether stability corrections dampen the fluxes too much.

15080, line 10. We have added a sentence to note that strongly unstable conditions are rare over snow.

15082, line 1. We have retained the units used in the model (hours, not seconds which would be strict SI) because we want the paper to be consistent with and describe the model the way it is.

15082, line 15. We have changed temperature to energy.

15092, lines 11-12. We appreciate the suggestion to compare to additional stations. This was addressed above.

15092, line 19-20. We have explained how longwave radiation was estimated.

References

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