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Hydrology and Earth System Sciences Discussions

Interactive comment on "Evaluation of alternative formulae for calculation of surface temperature in snowmelt models using frequency analysis of temperature observations" *by* C. H. Luce and D. G. Tarboton

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The authors thank the anonymous reviewer for thoroughly reading the paper, providing thoughtful comments, and recognizing the contribution of the paper. We are also thankful for the opportunity to add to the discussion of some important ideas raised by the reviewer. We have made the requested changes in the manuscript for the technical comments. While we agree that more data and more model comparisons would be beneficial, they are outside the scope of this paper, and could actually detract from the contribution of theoretical considerations by adding substantial complexity.

We agree, in principle, that more sites and more seasons of analysis would be helpful for discussing generalization of parameters. Philosophically, we believe that independent replication of the experiment is the most appropriate means to test and describe the generality of the model and its parameters. Unfortunately, the financial reality is that these are expensive data to collect, and we are not presently supported for further data collection. Although the data here represent only one season at a site, they are sufficient to demonstrate the merits of the analysis and modeling methods proposed. It may be worth noting that Figure 4 actually shows performance during the entire two week period without snowmelt, not just 8 days. We have provided a thorough description of our analytical techniques to support replication.

It is probably worth restating that the one of the improvements seen with the modified force-restore is that physically reasonable values of the thermal conductivity can be used. Thus estimates of thermal conductivity for another site or time could be estimated using formulae such as described in Sturm et al. (1997). The remaining calibration parameter is what to use for the lower frequency component. It is not clear that it should change based on snowpack properties, although it may have some dependence on local climate characteristics. Unfortunately we could not test for periods outside of the 14 day period shown in Figure 4 because of liquid water in the snowpack, which would require modeling state transitions in the snowpack. You (2004), shows validation of a complete model incorporating this conduction scheme on these data and shows reasonable matches for other periods of mixed conduction and refreezing of water, although with slightly different parameters.

Comment 1 suggests a comprehensive review and comparison to finite difference models models such as SNTHRM (Jordan, 1991). There have been several detailed comparisons of single layer and finite difference models (e.g. Koivasulo and Heikinheimo, 1999). Because these models use climatic drivers, the comparisons always become a bit complex because of differences in flux parameterizations. Performing a parallel analysis to ours on SNTHRM (e.g. using only surface temperature as a boundary condition for conduction calculations) would require some reprogramming of SNTHRM. Furthermore, SNTHRM has time and depth-varying density and thermal conductivity, so it would be difficult to directly compare the lumped parameterization (where we used an observed value) to one where the model actually sets the values. Again, reprogramming would be necessary to set the values to allow for comparison of the two approaches. To be fair, performance for the uncalibrated values provided by SNTHRM would also need to be evaluated. Although, this set of analyses could form a useful and interesting set of comparisons, we think it would detract from the current presentation, which the reviewer indicates is “a solid step forward”.

We decided not to add a table showing the range of methods because after developing a draft, it was mostly an uninteresting list of models that use finite difference schemes. The list of citations in the text where finite difference models are introduced mentions the scientific papers where several models are discussed.

Comment 2 asks how this approach using the surface temperature data directly compares to using estimates of net radiant and turbulent fluxes. The measurement of snowpack temperatures with depth is a direct measurement of the snowpack energy balance during periods where there is no liquid water phase present. The surface temperature forms the time-varying boundary condition for conductive fluxes into and out-of the snowpack. Net turbulent, radiant, and conductive fluxes depend on surface temperature and must balance at the surface of the snowpack. Thus the weather measurements are ultimately used to estimate the surface temperature of the snowpack, and that estimate depends in part on the conductive model used. By applying the observations of the surface temperature to drive conductive models, we can directly test the conduction model rather than mixing uncertainties from radiation and turbulent flux measurements and models of conductive flux. While this simplification limits the periods of testing to those when conduction is the dominant process, it is a much cleaner, directed test of the conduction formulae.

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