The stated objective of this paper is to better estimate the spatial pattern of frozen ground in a gridded watershed model by modifying a simple cumulative freezing or thawing degree-day approach. This is basically accomplished by (1) using an air temperature index modified by a spatially variable solar radiation index (slope, aspect, elevation) and spatially variable canopy cover to model the snowpack, (2) using shortwave radiation and canopy cover in the calculation of a frost index, (3) using the insulating effects of ground cover when calculating a frost index, and (4), computing frost depth from the frost index by using the "a modified" Berggren Equation. The calculated snow depths and frost depths are then compared to measured snow and frost depths for locations within the Sleepers River Watershed for 5 winters. Snow depths and frost depths were also calculated using the unmodified temperature and frost indices. The results indicate that the modifications implemented generally improved simulations of snow depth, frost depth, and frost occurrence over the 8 spatially diverse sites.

I found that the Introduction to this paper was unnecessarily complicated. Basically, the authors used an air-temperature index modified by a potential solar radiation index (as affected by slope, aspect, and elevation), canopy cover and ground cover. This modified temperature index was then used to drive a snowpack accounting scheme and a frost depth calculation based on cumulative freezing degree-days.

I found the "frozen ground index" and its relationship to the "modified Berggren Equation" unclear. Is the modified Berggren Equation used only after the frozen ground index exceeds a "threshold" value?

Also, equation (16) on page 6 is puzzling to me. There seems to be an Rnet term (net all-wave radiation) missing. It should read: Rnet = Rsw,net + Rlw$\downarrow$, - Rlw$\uparrow$. Equation (16) makes sense only if Rnet is assumed to be zero but that is not stated.

It is unclear from the text if sublimation from the snowpack is accounted for when there is no canopy cover. It appears that sublimation is only calculated from intercepted snow.

Page 13, table 3: The term "residual saturation" is ambiguous, particularly at the low value of 0.038. Is this referring to some "degree of saturation", ie, some volumetric soil moisture expressed as a fraction of the saturated volumetric moisture content?

Page 23, line 4. Should read "requires more energy **loss** to cool and freeze the soil"

Overall, the authors are to be commended for their sound modeling procedures that included several field test sites, distinct calibration versus validation time periods, the sophisticated parameter estimation techniques, and sensitivity analysis. By using a 30-meter grid for capturing spatial variability they were able to generate informative maps of snow depth, frost depth, and snow water equivalent. The use of RMSE and NSE values for each case, as well as plots of absolute simulated and observed values for each time period, were appreciated. The absolute errors were hard to perceive for some data sets simply because of the small size of the

graphs. It does appear from the plots that there were cases with high absolute differences between simulated and observed.

I agree with the authors assessment of the model's strengths and weaknesses, and the need for improved representation of the effects of wind and soil moisture in achieving better accuracy.