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Discussions

Interactive comment on "A Simple Temperature-Based Method to Estimate Heterogeneous Frozen Ground within a Distributed Watershed Model" *by* Michael L. Follum et al.

Anonymous Referee #1

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The aim of this study was to come up with a simple index-based soil frost distribution model for hydrological purpose that can capture spatial variabilities in snow depth and vegetation cover. To this end, the authors extended an old, well-established frost index model (CFGI) and combined it with a modified temperature-index snow model. The snow and soil frost models were tested against data of five winters from an experimental watershed in Vermont. The results show some improvements of the simulated snow and soil frost depths, but they also highlight that simple, index-based models have their limitations in representing the temporal and spatial distribution of snow and soil frost.

The distribution of snow and soil frost depth is indeed a complex problem for northern-latitude and high-altitude regions of the world – in particular where a vegetation cover is present. Research studies with the objective to improve the simulation of snow and soil frost depth for hydrological purposes have been numerous in the past twenty years . . . not only in north America, but also in Europe. For example, a physically based model (http://www.coupmodel.com/) was developed in Sweden some twenty years ago, has been made available (open access) and has been used for snow and soil frost simulations in Nordic and alpine countries, as well as for sites in Greenland. This model calculates the combined heat and water balance of a soil profile and accounts for interception processes and the presence of a variable snow cover (quite similar to the snow model in this manuscript). So what's the advantage of using a simpler index-based model?

In former times, the main argument was that such index-based models only needed one or few input variables (typically air temperature), whereas physically-based models need five or more meteorological input variables (cf. page 2, lines 11 and 12) and maybe a considerable amount of parameter settings. That's certainly a valid point for many applications. But here in this work, it seems that the new snow and soil frost model is not really simple and index-based, but is going in the direction of a more physically-based approach. Also it apparently needs not only air temperature, but also precipitation, wind speed, relative humidity and cloud coverage. Thus, the argument for developing a new index-based model (instead of using an established physically based model) is not obvious.

The snow and soil frost model modifications proposed in this work are reasonable. Improving a pure air temperature index snow model with a radiation term and accounting for snow interception is certainly of added value for an area such as the Sleepers River experimental watershed where the topography and the vegetation cover is distinctive. And adding a variable soil moisture content and a litter cover to the soil frost model is relevant as these are two important controls of soil frost formation. The core of the new models is a radiation-derived proxy temperature T(rad) instead of simple air temperature. It would be interesting to see somewhere in this manuscript how much T(rad) differs from T(air) in order to clarify whether or not this makes a big difference for the snow model compared to the other modifications. Speaking of T(rad) I can recommend the following very recent publication (EOS spotlight article https://eos.org/research-spotlights/how-the-micrometeorology-of-alpine-forests-affects-snowmelt) for reference: Webster, C., et al. 2017. J. Geophys. Res. Atmos.,122, doi:10.1002/2017JD026581.

The results presented in this manuscript confirm that the modifications made to the original index models are significant and reasonable. Nevertheless, Figures 6 to 8 also clearly show that – even with these more sophisticated models – it is still a long way to reproduce correctly year-to-year variabilities and spatial variabilities in soil frost depth. This is only partially due to the limitations of the model. On the other hand it also reflects the tremendous sensitivity and variability of soil frost in a real landscape. A nice example of such a complex variability in soil frost occurrence was presented in Journal of Hydrology, 546: 90-102, 2017.

The conclusions from this work are plausible, though not really exciting and surprising. And the proposed avenues for further research related to this topic are reasonable. Overall the manuscript is well written and easy to read.

Specific comments:

Page 2, line 17: "When the frost index exceeds a threshold, the soil is considered frozen and impermeable to infiltration." And Page 20, line 7: "…any frozen ground has the potential to impede infiltration and produce flooding" This is not really true. There is a bunch of literature showing that frozen soils are not always impermeable (depending on the ice content), and it needs a certain frost depth for the soil to become impermeable. This could be mentioned somewhere.

Page 8, line 30: "Frost depth was measured using gages described by Ricard et al.

(1976) and Shanley and Chalmers (1999)." Please provide some information what principle these measurements are based on. Are these frost tubes containing a blue liquid? Just referring to Ricard and Shanley is not good enough.

Pages 18 and 19: The results in Figures 5 and 6 show very nicely that frost depth is primarily controlled by the snow depth, which in turn is controlled by the forest canopy (or soil management). The air temperature decreasing with altitude is obviously of minor (or no) importance for this elevation range. This is an important message and agrees very well with observations from the lower alps in Switzerland (Journal of Hydrology, 546: 90-102, 2017).

Pages 25 to 29 (References): the cited literature is almost exclusively from North America, but there is a wealth of publications on the observation and spatial modelling of snow and frozen soil from Europe (and other parts of the world) that the authors seem to be unaware of.

The following reference is very relevant to this one and could be mentioned somewhere in the discussion: Campbell, J.L., Ollinger, S.V., Flerchinger, G.N., Wicklein, H., Hayhoe, K., Bailey, A.S., 2010. Past and projected future changes in snowpack and soil frost at the Hubbard Brook Experimental Forest, New Hampshire, USA. Hydrol. Process. 24, 2465–2480, doi: 10.1002/hyp.7666.

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