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Hydrology and Earth System Sciences
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 #2

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The paper by Follum and co-authors seeks to develop a new 'simple' temperature based method to estimate frozen ground in a hydrology model. First, the issue of simulating frozen ground is critical for watershed models, particularly in much of the world where frozen ground strongly influences the rate, timing and magnitude of hydrological fluxes. There is a long history of incorporating degree-day and other frozen ground methodologies into hydrological models as the authors state, but of course they are by their nature highly calibrated. The authors state that frozen ground models that are more physically based (such as SHAW) are highly calibrated and state there is a need

for temperature index models that incorporate more physical parameters (i.e. ground cover, radiation derived temperature indices, etc). I have no doubt that this is the case, but in this paper the authors present a highly parametrized and calibrated degree-day model. It works okay, but certainly not great. In fact, I think there is enough forcing data here to drive SHAW and/or other more physically based land-surface schemes with frozen ground. Cloud cover and other radiation parameters are rarely measured operationally, and the adjustments of the TI portion of the model rely heavily on empirical adjustment. While going down the road of complexity, the authors include some new process representation (interception/unloading), while neglecting sublimation and others. The frozen ground model is adjusted to better improve physical representation, but it does not represent an advancement of our understanding of frozen ground process as the method of simulation is largely empirical and parameters are not transferrable. The strength of simple models is their ease of use and simplicity - but here we have a simple model that gets more and more complex and requires parmaterization that truly limits its applicability and does not justify it use when compared to existing models in the literature. While I can see that developing a local or improved freezing model is important in forecasting and operations, I do not believe that this paper in advances our understanding of frozen ground processes at a fundamental level that would justify publication in HESS. Perhaps a more operational journal would be the appropriate venue for this work.

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