**Hydrology and
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Discussions

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Interactive comment on "A surface model for water and energy balance in cold regions accounting for vapor diffusion" *by* Enkhbayar Dandar et al.
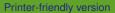
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Response to comments Anonymous Referee #1

This paper studies the role of vapor diffusion in the land energy and water balance cold and semi-arid regions. A new water and energy balance model has been developed that accounts for freezing and melting. In general, the topic is interesting for the land surface modelling community. However, there are several problems need to be addressed.

We respond to the comments in normal text, whereas the comments are numbers in

parentheses.

We thank the referee for his/her kind assessment of our work

(1) Line 359: 'In summary, results are qualitatively consistent with observations'. However, I did not find the evaluation of the model simulations with observations. It is important to comprehensively evaluate the new model in representing water and energy cycles (e.g., river discharge, soil/surface temperature, sensible/latent/ground heat fluxes) over more than one site in different river basins.

The referee is right in that we have not tested the model in a quantitative way for the Tuul River basin. There are two reasons for that. First, data are limited. Long records of output variables are available only for river discharge and snow depth. A proper modeling of the basin requires spatial discretization to acknowledge variability, which is relevant for both rainfall and snow depth. Therefore, a description of the model would be far too long. We are preparing another work comparing model results with observation data. Second, and most important, while our work was motivated by the Tuul River basin, the primary goal of this paper was to show the importance of vapor diffusion fluxes within the soil, which are ignored by most models. The validity of such concept can only be tested qualitatively. Thus we tested model validity by comparison to other studies in the region (Ma et al., 2003; Zhou et al., 2014). We also tested our model with laboratory experiment's data (Gran et al., 2011a) which is presented in appendix B.

(2) The equations are not precise enough. All of them should be re-checked. For example, in equation 16, R_{nl} should equal to $R_{L,down} - R_{L,up}$, but not $R_{L,up} + R_{L,down}$. Line 208, the description of λ is puzzled. It is better to express by an equation.

We have revised the equations independently a few times and will do again prior to submitting the revised version of the paper. Yet, typos are a pest hard to eliminate. In fact, a typo in line 208 has made the description of thermal conductivity "puzzling", the reviewer is right. It should read:

"where λ is the soil thermal conductivity ($\lambda = \lambda w \hat{\ } phi * \lambda s \hat{\ } (1\text{-}phi)$), where phi is the porosity, λs is the thermal conductivity of the solid particles, and λw is the thermal conductivity water, equal to that of ice, λi, when water is frozen or liquid water, λl, otherwise (Côté and Konrad, 2005)."

Regarding the sign of RL,up, our sign convention is that upward fluxes are negative, which facilitate handling terms where the flux can be positive or negative. We agree that it may be confusing to treat RL,up, which is always upward, as negative (see eq. 21), but it should be written this way for consistency. Still, acknowledging the potential for confusion, we will clarify it in the revised version.

(3) Lines 74-76, the inputs and outputs are mixed here. I suggest to describe the inputs/outputs in a more clearly way, not only for water, but also for energy balance.

For water balance, main input is the precipitation as rain or snow. For surface layer, outputs include the evapotranspiration (both ice deposition and sublimation), surface runoff and the infiltration into the subsoil while the recharge into aquifer for subsoil soil. Vapor diffusion occurs between the two layers.

For energy balance, main input is the net radiation. For surface layer, outputs consider latent heat, sensible heat and heat fluxes whereas conduction between the two layer and energy released due to phase changes.

(4) I do not see the advantages of the new model. The authors may want to emphasize it more clearly.

As mentioned above (see response to comment 1), but also in the paper, the main goal of our work is to show the importance vapor diffusion, which is ignored by most land surface models (e.g., SWAT). As such, our primary goal is not so much to develop a new model (although we consider it pretty complete, also in regards to radiation) as to argue that widely used existing models should incorporate diffusion. None of these integrated models simulate vapor diffusion in the soil. The goal of our work is to gain

insight into the hydrological processes in cold regions. We analyze the sensitivity of the model to the values of the parameters, which implicitly allows us to assess the relative importance of the various processes in the soil. It is from this analysis that we conclude that vapor diffusion is a relevant mechanism that should not be ignored. We will add a sentence at the end of the abstract and conclusions to emphasize this point.

(5) In Equation 1 and Lines 96-99, how the interception loss (by canopy) is treated in the model?

The canopy is important both for interception and for evapotranspiration. We do not model interception loss explicitly because, our goal is not so much flood prediction (for that we would have to integrate many other processes) as to assess the relevance of vapor diffusion, which is a slow process. Interception is usually evaporated within a few hours after rainfall and it is incorporated in the soil layer, which is usually sufficient for water resources assessment models. As for the role of the canopy in evapotranspiration, the surface resistance, rs, describes the resistance of vapor flow through stomata openings, total leaf area and evaporating soil surface (Allen et al., 1998). It shows in equation 6.

(6) Instead of using 'length' to explain Lss and Lsf, the 'depth' might be easier to understand. (e.g., Line 130 and other places)

We thank the reviewer for the suggestion, because length might suggest horizontal extent, but we are not sure. Depth would be the distance from the surface, which is not our case. Our model is simulated in the midpoint of the surface and the subsoil layers. Therefore, we propose denoting Lss and Lsf as thicknesses.

(7) Equations 13-15, how the ml, mi and ms are determined in model?

We thank the referee for this comment, as the explanation was confusing: ms is the mass of solid, and ms=(1-phi)*rho_s*L, where rho_s is solid phase density (2650 kg/m3), phi is porosity, and L is the layer thickness. The total mass of water m, which

is known from the mass balance, is equal to the sum of the liquid (l) and ice (i) masses ($m = ml + mi$). The total internal energy is the sum of that of solid, liquid water and ice ($U = ms*cs*T + ml*cl*T + mi(ci*T - \Lambda melt)$). At temperatures above 0°C all water is liquid ($m = ml$) and at lower temperature all water is ice ($m = mi$). Melting and freezing occur at 0°C, and the fraction of water frozen can be obtained from the internal energy, U (Eq. 14). The concept is illustrated in Figure 2. We will clarify the explanation in the revised version of the paper.

(8) How is the model initialized? At what time step is the model run? The model was initialized by running it twice, to ensure negligible storage variations that would blur the balances. The conditions at the end of the first run were adopted as initial conditions for the second and final run, which is the one reported in the paper. We solve the water and energy balance equations using a semi-implicit finite differences scheme with a time step of one day. For modeling the evaporation experiments (Appendix B), we adopted a time step of 1 hour.

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