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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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The main purpose of this work is to test using mostly a modeling approach whether vapor diffusion in the soil of a cold and semi-arid region is a significant process worthy of being incorporated into integrated hydrological models. The authors model the water and energy budget of a soil in Mongolia and conclude that it is indeed an important process. They find that the temperature gradient between the cold ground and warm air in spring leads to water vapor condensation in the upper soil layer, which greatly modifies both the water and energy budgets. In particular, it is important for the thawing of the active layer.

The question addressed is truly worthwhile and has often been neglected not just for the soil but also for the snowpack water budget. (Sturm and Benson, 1997) studied water vapor exchanges between snow layers and between the soil and snow layers in a cold and fairly dry region of interior Alaska which may be fairly similar to that studies by the authors and indeed found that in winter soil moisture migrated upward and condensed in the snow, so that there was an overall water loss from the soil because of vapor diffusion in soil. (Domine et al., 2016) studied the evolution of the temperature and water content of the soil at 10 cm depth at a high Arctic site and also observed water loss over the course of winter, which they also ascribed to water vapor diffusion from the warm soil into the colder snow.

Now, in this study, the authors find the opposite. Their Figure 4 shows that the flux of water vapor between the soil and the surface is zero during the winter months (mid-December to April) but that from mid-April to mid-August the water flux is from the warmer surface (or atmosphere) to the colder soil. On a yearly basis, there is a net flux of water vapor into the soil, which is found to be critical for recharge and for active layer thawing.

It is clear that there is a discrepancy between both experimental studies cited and the model results of this study, and this is worrisome. In particular, the zero water vapor flux in winter in the model in troubling, because both experimental studies conclude to a very significant soil water vapor loss over that period, and this issue must be resolved before publication. The purpose of this review is to suggest reasons for this discrepancy, which may perhaps lead to useful model modifications by the authors. The model divides the soil into just 2 layers: the surface layer, which is 16 cm thick, and the subsoil which extends down to 150 cm. I did not find a description of how the snow layer was treated (besides its albedo) and I wonder whether the thermal impact of the snow was even treated in the model. In winter, the snow protects the ground from the winter cold air, with the result that the ground surface is much warmer then the snow surface. This is illustrated e.g. in (Domine et al., 2016) but also in countless other papers, showing that a thin snow cover leads to a soil surface temperature warmer by at least 10°C than the air. If this effect is not taken into account, then the simulated soil temperature will be too cold, and therefore the water vapor flux will be greatly reduced. In any case, simulating a negligible soil drying over winter seems contrary to published work, and also to all my observations in cold regions, where I have always observed very dry soils at the end of winter.

The thickness of the layers may also cause errors. If my understanding is correct, the temperature of each layer is the average temperature of the layer, while in fact exchanges with the atmosphere will be dictated by the difference in temperature between the air near the surface and the soil very surface. I believe this may cause very large errors. After snowmelt, the surface can warm up very rapidly in the presence of solar radiation. Radiation absorption by the surface is even the reason why the air warms up, as heat is transferred from the hot surface to the colder air in the daytime, as the authors doubtless know. If the average temperature of a 16 cm layer is considered, then clearly this average temperature will be significantly colder than the surface temperature, because the soil is warming up in spring and summer. Calculating water vapor exchanges using that temperature can only lead to inadequate conclusions, and most likely to the wrong sign of the flux.

Other aspects of the model are surprising or arguably approximate. Using 0.6 for snow albedo (line 239) is extremely low. Perhaps it does apply to the actual site studied, but this would need qualification as snow albedo is almost always much greater (Gardner and Sharp, 2010), except when large amounts of vegetation protrude above the snow (Sturm et al., 2005; Loranty et al., 2011). An earlier statement (line 162) that albedo was determined from snow depth makes this all very confusing. For downwelling irradiance, why not use SBDART (Ricchiazzi et al., 1998)?

In summary, while the objective of this paper is interesting and laudable, I am very concerned that the model structure is not adequate (probably much too simple) to allows testing the objectives stated. I would recommend treating the thermal effects

of snow adequately (this may have been done, I just did not find the expected details) and using more soil layers with several thin layers near the surface. If water vapor exchanges are to be calculated reliably, the temperature of the top soil and of the air very near the surface must be calculated accurately. This probably requires 1 cm-thick soil layers near the surface. Finally, a convincing validation of the model would require measurements of the soil temperature and water content, preferably at several depths. Without such data, confidence in the model will remain very limited. Very major changes therefore seem required before publication.

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