Response to comments by Referee #2 Dr. Florent Dominé

Dr. Florent Dominé raises a number of issues that question the validity of our paper. Before addressing them, we want to thank him for his thoroughness (it is clear that he has understood clearly our work), for the constructive tone of his comments, and for having introduced us to the literature on vapor diffusion as a mechanism for snow metamorphism. The latter, though not requested by Dr. Dominé, makes it clear that we need to address the snowpack water balance literature in the revised version of our paper. We will make a small summary of the topic in the introduction and we will compare our results to those of snow-pack researchers. It is important, however, to bear in mind that our contribution is oriented to water resources assessment (evaluation of surface and, especially, ground water runoff), as properly acknowledged by Dr. Dominé. The main novelty is that vapor diffusion is important not only for soil water and energy balances, but also for snow-pack diagenesis.

The three main issues raised by Dr. Dominé are (1) the apparent contradiction between our results and those of snow pack researchers; (2) the need for a more refined grid; and (3) the value of albedo. We respond below to these and other minor issues.

(1) Winter vapor flux

Based on our Figure 4 (reproduced here as Figure R.1), Dr. Dominé states that our winter vapor flux is zero, which contradicts results in the snow pack literature pointing that an upwards flux should be obtained. He is right in that the winter vapor flux should be upwards (since the deep soil is warmer than the surface, vapor pressure is larger at depth which leads to an upwards diffusive flux). However, upward vapor diffusion is highest during autumn and drops considerably during winter (mid December to April). This is also illustrated by, e.g., figure 12 of Dominé et al. (2016), (reproduced here as Figure R.2), which shows vapor fluxes in snowpack being highest in November. Actually, the magnitude and, especially, temporal evolution of vapor fluxes computed by Domine et al (2016) are comparable to ours. According to Domine et al (2016) vapor flux drops from 0.2 to 0.02 mg/m2/s (or from 0.017 to 0.0017 kg/m2/d, in our "hydrological" units) from Nov, 1st, 2014 to Jan, 1st, 2015. Ours starts at 0.2 kg/m2/d and drops more slowly at first, but faster towards the end of January. In both cases, the flux reverses by the end of April. Given the different settings (their study was performed at a much higher latitude than ours), we consider their work like an independent validation of ours.

This evolution of vapor fluxes can be explained easily by assuming the air in the soil (or snow) to be saturated and writing Fick's law as (compare eq. 9 of our article):

$$J_D = -\frac{MD}{RT}\frac{\partial p_{v,sat}}{\partial z} = -\frac{MD}{RT}\frac{\partial p_{v,sat}}{\partial T}\frac{\partial T}{\partial z} \qquad \text{(R1)}$$

The highest temperature gradients (*dT/dz*) are in autumn and drop in winter. Another effect is the fact that at low temperatures the saturated vapor pressure changes very little with temperature (that is, $dp_{v,sat}/dT$ is low). All these factors similarly acknowledged in the snowpack literature and in our work. Perhaps, the main difference stems from their emphasis on small scales (metamorphism within the snow cover), which is related to the spatial and temporal discretization, which is addressed below.

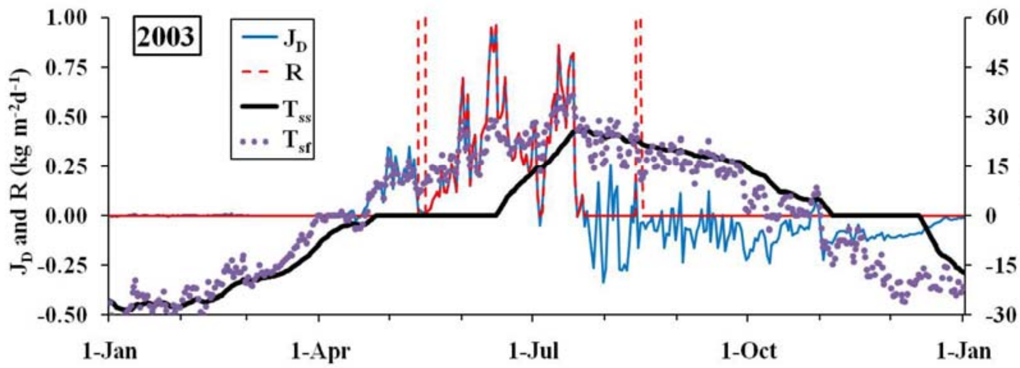

Figure R.1: Reproduction of Figure 4 of the Discussion paper. Note that the diffusive flux is negative.

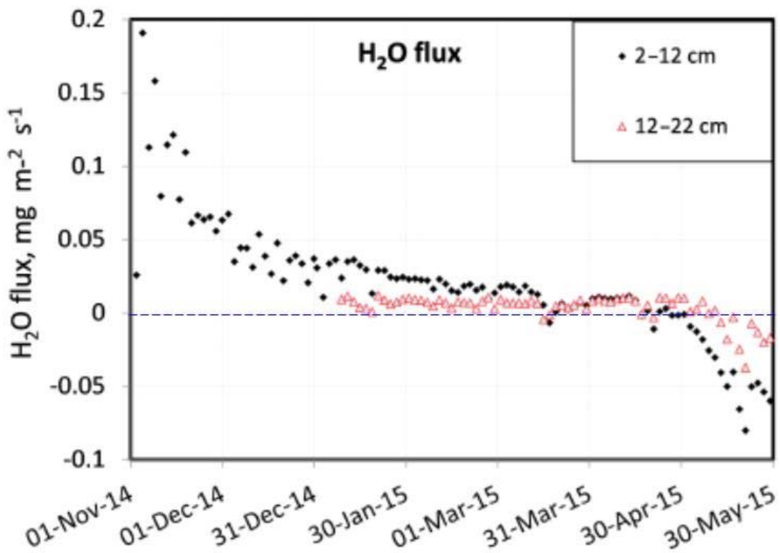

Figure R.2. Vapor fluxes computed by Domine et al. (2016) at the snow pack

(2) Spatial discretization

Dr. Dominé questions our choice of two layers. The issue of discretization has got three sides: (1) whether it is sufficient for simulating heat conduction, (2) whether it is sufficient to simulate vapor fluxes; and (3) whether it is necessary to acknowledge the insulation effect of snow. We have analyzed the first two issues, but not the last one. We discuss all of them below. We realize the referee is only questioning the last issue, but we feel it is necessary to discuss the other three to properly respond without "ex-cathedra" reasoning.

2.1 Are two layers (and 1-day time step) sufficient to simulate heat conduction?

The issue largely depends on the goal. In fact, the appropriate grid size depends on the time increment. The rule of thumb we adopt is that Δt should be of the order of

$C \cdot \Delta z^2/\lambda$, where C is thermal capacity and λ is thermal conductivity. You do not loose accuracy by working with a smaller than required time step, but you do not gain much either.

To address this issue, we have simulated heat conduction through a 1.6 m thick soil with thermal conductivity of 0.7 W/m/K and thermal capacity of 3 MJ/m^3/K. Temperature is prescribed at the surface a double sinusoidal, one with one day period and 20 ºC amplitude, to simulate daily temperature fluctuations, and one with one year period and 60 ºC amplitude, to simulate seasonal temperature fluctuations. This superposition is appropriate for mid-latitudes, though not above the polar circles (Domine's study was performed at a 70+ latitude, where the sun does not rise during winter).

We first simulate heat conduction with a 50 nodes grid ($\Delta z = 3.2$ cm) using time increments of 1d and 0.1 d. We then average computed temperatures for the top 5 elements (i.e., 0.16 m, our sf layer) and the bottom 45 elements (i.e., our ss layer). Results are shown in Figure R.3. It is obvious that the solution with 1 d intervals miss the impact of daily fluctuations (see zoom at the right of Fig. R.3) at the sf layer (results are identical for the ss layer). But results are also identical at the sf layer, when the comparison is made in terms of daily averages.

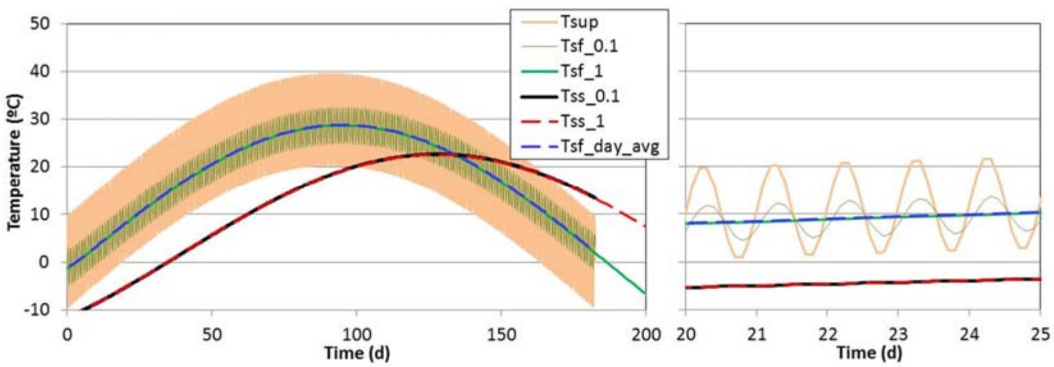

Figure R.3: Temperatures computed with a 50 nodes grid and time increments of 0.1 and 1 day for a half year and (right) zoom over 5 days. Tsf represents average over the top 16 cm, and Tss represents average over the remaining 144 cm. In terms of averages, results are identical for 0.1 and 1 d time increments, but the latter misses the impact of daily fluctuations.

We now compare these solutions to those obtained with a 2 layers (cell centered) solution, similar to the one we used in the HESSD paper (except that here, we are adopting the boundary temperature at the edge of the top cell, to isolate the impact of spatial and time discretization). Results are shown in Figure R.4, which makes apparent that (1) in terms of daily averages, surface layer temperatures computed with 50 nodes are virtually identical to those computed with 2 cells; and (2) daily fluctuations with the two layers' model are not accurate (both in terms of amplitude and time lag).

In summary, the adopted discretization is adequate for simulating daily averages of temperature in the shallow layer (top 16 cm of the soil), although it would not have been appropriate to simulate daily fluctuations, which was not a goal of our model.

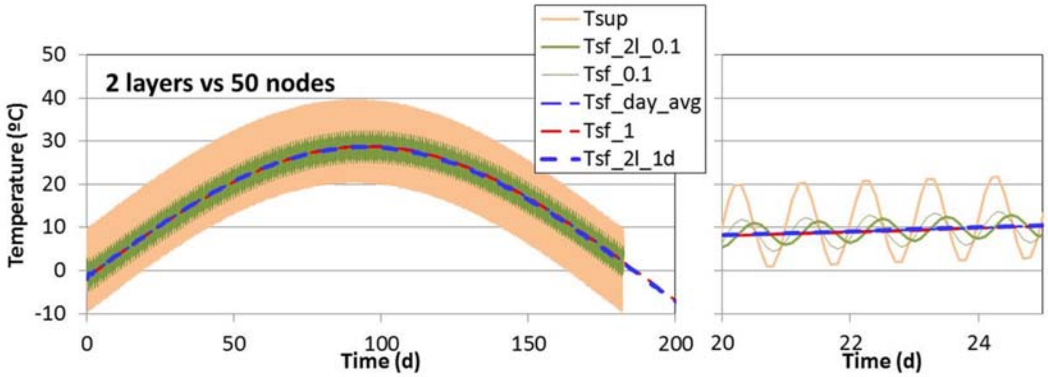

Figure R.4: Temperatures computed with a 50 nodes grid averaged over the top 16 cm (with a 0.1 d time step Tsf_0.1, averaged over 1d, Tsf_day_avg , which is virtually identical to the one with a 1 d time step, Tsf_1d) and the two layers' model of our HESSD paper (with a 0.1 d, Tsf_2l_0.1, and 1 d, Tsf_2l_1, time steps) for a half year and (right) zoom over 5 days.

2.2 Are two layers sufficient to simulate vapor fluxes?

Addressing this question is very tricky because it would require performing a non-isothermal, multiphase flow model. Therefore, we verified the simulation of vapor diffusion by comparison with the laboratory experiments of Gran et al (2011). Results, which are shown in Appendix B of our paper, are excellent. This exercise is quite demanding, because the experiments of Gran et al. (2011) were performed under stiff conditions (very dry soil surface). The fact that our model performed well suggests that, indeed, the simulation of vapor fluxes is appropriate.

However, those experiments were performed in the laboratory and did not include daily fluctuations. Note that both Figures R.3 and R.4, display significant differences between extreme daily temperatures and average Tsf. Therefore, a valid question would be if daily fluctuations have a long term impact on vapor fluxes. Gran et al (2018) have addressed this question and conclude that that they do (while the average of daily temperature fluctuations is zero, the average of daily vapor flux fluctuations is not), but the effect is very small for water resources assessment, which is the goal of our work.

2.3 Is it necessary to acknowledge the insulation effect of snow?

The thermal conductivity of snow is low compared with that of typical soils. Therefore, snow can insulate more effectively than soil, depending on snow depth (Romeroy and Brun, 2001). The thermal conductivity of snow varies from 0.04 to 0.25 W m^{-1} K^{-1}. The thermal conductivity of snow measured by Domine et al., 2016 in Canadian high Arctic that 0.04 W m^{-1} K^{-1} for thin snow depth and 0.28 W m^{-1} K^{-1} during winter for thick snow depth. In fact, vapor diffusion changes the thermal conductivity of snow (Domine et al., 2016). We have used an average 0.15 W m^{-1} K^{-1} of thermal conductivity of snow for our study.

To address the issue raised by the referee, we need to recall how resistances to flow are simulated. The fact that the surface temperature is different from the mean layer temperature is acknowledged by adding a soil surface resistance to the boundary condition (eq. 25). That is, heat exchange is assumed to consist of two resistances is series. The first represents the resistance to (sensible) heat exchange between the surface and the atmosphere

$$H = \frac{\rho_a c_a}{r_a}\left(T_{sup} - T_{air}\right) \tag{R2a}$$

The second resistance represents the resistance to heat conduction between the surface and soil (mean layer) temperature

$$H = \frac{\rho_a c_a}{r_{sf}}(T_{sf} - T_{sup}) \tag{R2b}$$

Combining these two equations to eliminate T_{sup} yields:

$$H = \frac{\rho_a c_a}{r_a + r_{sf}}(T_{sf} - T_{air}) \tag{R3}$$

This is identical to our equation (25). The issue is that in the discussion version of the paper r_{sf} was calculated (our Eq. 26) from the thermal conductivity of the soil, as

$$r_{sf} = \frac{0.5 L_{sf} \rho_a c_a}{\lambda} \tag{R4}$$

where λ is the thermal conductivity of the soil. To acknowledge the insulating effect of snow, we would need to add:

$$r_{sf} = \rho_a c_a \left(\frac{0.5 L_{sf}}{\lambda} + \frac{L_{snow}}{\lambda_{snow}}\right) = r_{soil} + r_{snow} \tag{R5}$$

Once T_{sf} has been computed, temperature at the snow surface can be obtained from Eqs. (R2a) and (R3) as:

$$T_{sup} = T_{air} + \frac{r_a}{r_a + r_{sf}}(T_{sf} - T_{air}) \tag{R5}$$

The temperature at the interface between soil and snow can be computed analogously. Therefore, the comparison is sensitive to the relative values of r_a, r_{soil}, and r_{snow}. In our case, r_a is very sensitive to wind velocity (sensible heat exchanged is enhanced by turbulence and high wind, so that r_a is reduced proportionally to wind speed, Eq. 5, with a 0.1 m/s threshold). But it is typically some 10 times greater than r_{soil}. As for r_{snow}, it depends on the type and thickness of snow. While the thermal conductivity of snow can be much smaller than that of the soil, its thermal capacity is also much smaller. The effect of variations in these two parameters on fluctuations within the medium was analyzed by Slooten et al., (2010) (they analyzed on the impact of variations of hydraulic conductivity and storativity on the hydraulic response to sea tides, but the problem is mathematically identical to the one we are discussing here). For $\lambda_{snow} = 0.15$ W m^{-1} K^{-1}, and thicknesses of the order of 5-10 cm, r_{snow} is much greater (some 3-6 times greater) than r_{soil}, but smaller than typical values of r_a. Therefore, one can expect a moderate effect snow isolation.

This is illustrated in Figure R.5, where r_a, r_{soil}, and r_{snow} are (1.14, 0.11 and 0.33 $J/m^2/K/d$, respectively. It is clear that the effect of resistances is severe (so severe that daily temperature fluctuations are eliminated, not shown). As one might expect, if no air resistance is acknowledged, computed Tsf is significantly different when snow resistance is acknowledged (Tsf_snow). In fact, the winter difference is some 10 ºC, which coincides with Dr. Domine's perception of the temperature difference across snow and he insulating effect of snow (the fact that the number is identical must be considered coincidental, given the differences between his work and ours). However, the role of snow insulation is much smaller when atmospheric resistance is acknowledged. The maximum difference due to snow insulation is some 2ºC (difference between blue and brown line in Figure R.5), but the difference is negligible at mid and late fall, when vapor diffusion fluxes are maxima.

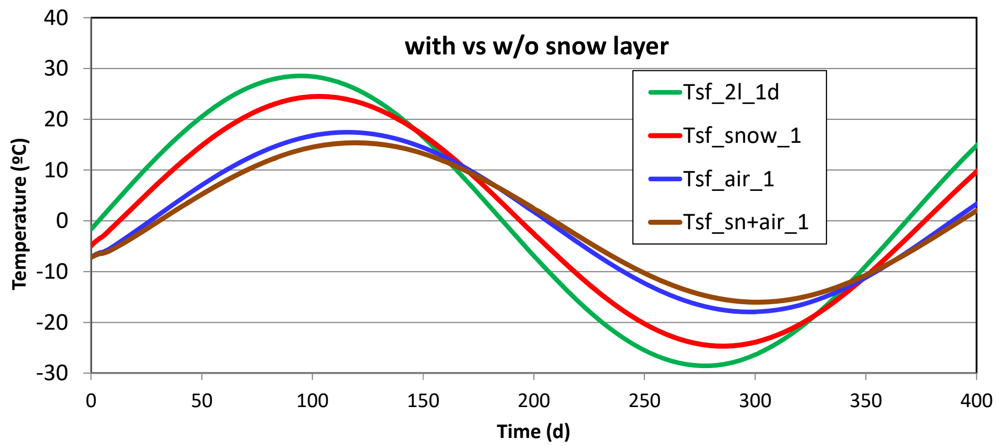

Figure R.5: Temperatures computed with the two layers model without atmospheric or snow resistance (Tsf_2l), with only snow resistance (Tsf_snow), with only atmospheric resistance (Tsf_air), and with both snow and air resistances (Tsf_sn+air).

As a result, the impact of acknowledging snow resistance to heat flow is small (Figure R.6)

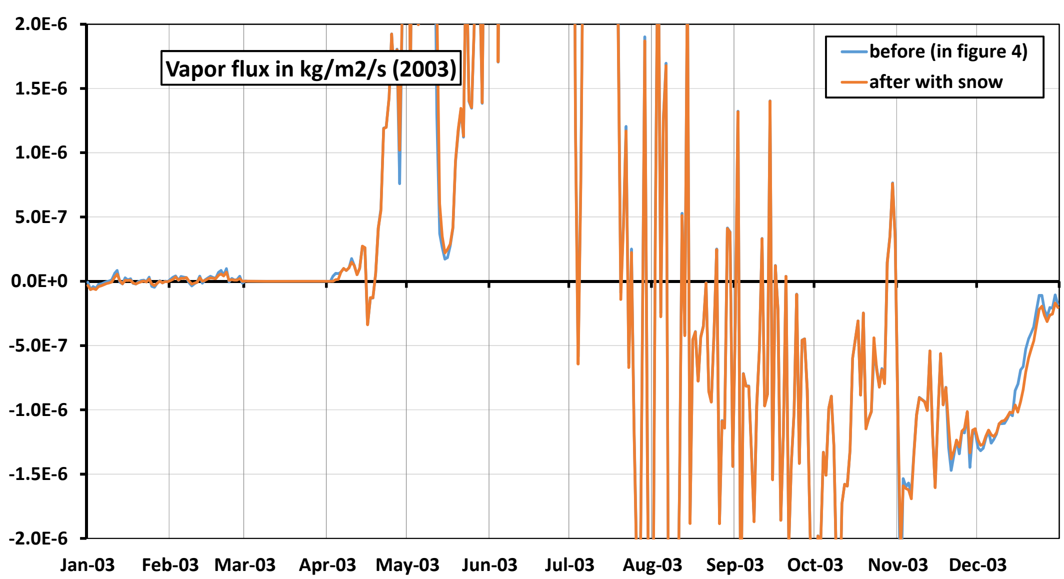

Figure R.6: Vapor fluxes computed with our model between the shallow (sf) and the deep soil (ss) layers with the original model (blue) and after including snow resistance to heat conduction. It can be seen that the difference is small.

The summary of this long discussion is that rather large thermal gradients are to be expected in response to temperature fluctuations (along the day when the sun rises!) or in windy days, when atmospheric resistance is small. These gradients can produce significant fluctuations of vapor fluxes, which is consistent with the results cited by the referee (Sturm and Benson, 1997; Domine et al., 2016) and those of Gran et al. (2018). These fluctuations may cause irreversible changes in snow morphology through sublimation, melting and freezing (vapor flux is a very powerful energy transport mechanism), but will tend to average out, so that their impact of water resources assessment should be moderate.

(3) Value of Albedo and downwelling irradiance

Dr. Domine argues that our value of albedo (0.6) is too low. Indeed, the albedo is an important parameter for energy balances, but it is highly variable for snow. Values range from 0.7-0.9 for fresh snow to 0.2-0.4 for old snow or dirty snow (Hall and Martinec, 1985). Oke (1987, table 1.1) gives values of 0.4 for old snow and 0.9 for fresh snow. In general, snow albedos in northern Mongolia are low. Dashtseren et al., 2014 calculated an albedo of 0.5. The reason is, that in our study area snowfall is relatively low and wind speed high during winter and spring. This causes high sublimation in relation to winter snowfall. Wimmer et al. (2009) estimated a sublimation/snowfall ratio of 80% for northern Mongolia. This leads to thin snow packs (thicknesses of 3 to 6 cm were measured), which affects the albedo, because it is affected by the underlying surface and by vegetation protruding through the snowpack (Dang et al., 2015). In this work we avoided the above complexity and used a constant albedo of 0.6, which is low but may represent a realistic average value for northern Mongolia. The albedo depends on snow depth in the sense that this value is applied if the snow depth is larger than 0.

SBDART model was suggested for calculating downwelling irradiance, which is a software tool that computes plane-parallel radiative transfer in clear and cloudy conditions within the Earth's atmosphere and at the surface (Ricchiazzi et al., 1998). However, it is already a quite complicated model. For our purpose, we think the simple approach for calculating irradiance suffices. Actually in hydrology similar simple methods are common (see, e.g., Allen et al., 1998, which is a standard method).

(4) A final note

Dr. Dominé concludes “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 allow testing the objectives stated”. It is true that the model is very simplified, but still a lot more complex than the existing land surface schemes used. Models always have to simplify the reality. The required complexity of the model depends on the goal of the model, available data, and model scale (spatial and temporal).

The result from our dwelling into the fascinating snow processes literature is that (1) vapor diffusion is important also for snow metamorphism, and (2) at the short term, small scale, diffusion fluxes are strongly controlled by surface temperature fluctuations, and the thermal insulation provided by snow is critical, but (3) we conclude that for daily averages over greater thicknesses, the effect is much smaller.

In the revised version of the paper, we will address these issues both in the introduction and in the discussion of results and we will include the thermal resistance (insulating effect) of snow. We will also expand the discussion of snow albedo and irradiance.

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