

Comments on Manuscript #tc-2019-143:
“Revisiting the vapor diffusion coefficient in dry snow”
Andrew Hansen

Special thanks to the Reviewer for a detailed and thoughtful review of the paper. I appreciate the many comments aimed at improving clarity of the presentation. In addition, I have personally learned a great deal from the review and, in particular, I am grateful for the many fine references provided.

Please see my response to Reviewer comments embedded in green font throughout the review.

General Comments:

This manuscript proposes to revisit the concept of Apparent Diffusion Coefficient (ADC) in dry snow, which is known to have led to a quite extensive literature, exhibiting a high dispersion in the quantitative results. The author first presents the main studies in a concise, balanced and objective way. He then proposes a definition for the ADC with 4 specific mechanisms that need to be taken into account for its accurate estimation. The author then proposes 3 theoretical models of increasing difficulty to express the ADC in terms of pore and ice volume fractions. He then revisits most of the literature studies, and takes into account their peculiarities to provide ADC estimations that are consistent with those of the proposed definition, showing that ADC values are generally between 1 and 1.3, with a much lower dispersion than that given by the original literature studies.

This manuscript is a sound and interesting contribution to the vapor coefficient literature, and allows better deciphering the high dispersion of the quantitative results observed. In overall, it is clear, didactic and well written, but would benefit from (i) a clearer statement of the main model hypotheses and (ii) additional explanations (e.g. ice blockage exactly compensated by shortened diffusion paths). Here are some suggestions the author may find useful to improve the manuscript:

1. Objectives and definitions:

To my understanding, the diffusion coefficient the author is interested in is not really an effective diffusion coefficient, but an apparent one: it takes into account mechanisms that are not really based on diffusion processes (e.g. phase changes), but consider them as contributing to the overall diffusion. To my opinion, the estimation of the ADC is close to an ill-defined problem: I think it would be better not to hide specific processes in an apparent diffusion coefficient, but to address each physical process separately. It is important (i) for a better understanding of the different processes and (ii) for a better modeling of the vapor transfer through the snow cover (modular approach, with possible separate improvements). As the author knows, such a modeling has been e.g. proposed in Calonne et al 2014 and Calonne et al 2015, where specific source terms are used to describe phase changes. Microscale studies of TG metamorphism in controlled conditions (e.g. Pinzer et al, 2012, Calonne et al, 2015, etc.) and appropriate simulations (e.g. Kaempfer and Plapp, 2009) are also very promising means to better understand and identify the different processes, leading to a better quantification of macroscopic vapor transfers in snow.

Given the extensive (and extremely confusing) literature on the vapor diffusion coefficient of snow, I think this manuscript is a sound and interesting contribution to snow science, as it provides simple ways to estimate the order of magnitude of the apparent diffusion coefficient, and, last but not least, clarifies and reconciles most of literature results. However, I would suggest the following improvements:

-stating more clearly from the start of the manuscript that the diffusion coefficient addressed here is an apparent diffusion coefficient (e.g. in the title, abstract, introduction, definition part...).

A revised draft of the paper will clearly state that the diffusion coefficient of interest is really an “apparent diffusion coefficient” (ADC). The title of the article will be revised to include the phrase “apparent

diffusion coefficient.” The abstract will reflect the meaning of the “apparent diffusion coefficient” in that the ADC includes the influence of phase change as contributing to the overall motion of water vapor through snow.

The Introduction of the paper will also address the definition of the ADC compared with a more traditional definition of the diffusion coefficient. These differences in definition provide an explanation for some of the variability in computed numerical values of this term.

-recalling the objectives of ADC estimations, and if adequate, explaining the interest of ADC computations over other approaches (e.g. considering the effective diffusion coefficient and phase change effects separately).

The Introduction Section and an added Background Section will develop the objectives of ADC estimations and explain the interest of the ADC over other approaches. The 1-D mass transfer of water vapor through a layered ice humid air microstructure provides an excellent motivation for the ADC—see the introduction to the response to Review #1 for a good discussion. Specifically, the ADC is precisely the coefficient needed for heat and mass transfer calculations at the macroscale.

2. Exact compensation of ice blockage by shortened diffusion paths (see p. 17 and 22): I had some difficulties to understand this point. Adding some explanations would help the reader (see detailed comments).

The exact compensation of ice blockage by shortened diffusion paths is a critical aspect of the entire development of the theory in this paper. I failed to provide a clear explanation of this concept. The details of this argument are provided here in the General Comments, as this point is called out several times in the subsequent “Detailed and Minor Comments.”

To develop a *1-D vapor diffusion model* for snow, we assume the vapor velocity vector field is aligned with the temperature gradient. An excellent experimental visualization of this 1-D velocity field is seen in Figure 1(a) (Figure 3 of Pinzer, 2012), showing ice voxel displacement fields from particle image velocimetry. The water vapor mass flux is inferred in the opposite direction of the displacement field. The figure indicates that vapor diffusion velocity is largely *a one-dimensional vector field occurring within a three-dimensional ice network*. This experimental observation suggests a strong presence of the hand-to-hand diffusion mechanism described by Yosida (1955) and is perhaps the single most important aspect of the vapor diffusion analysis developed herein. The results further suggest that a 1-D model of vapor flow may be a reasonable approximation for water vapor diffusion in snow.

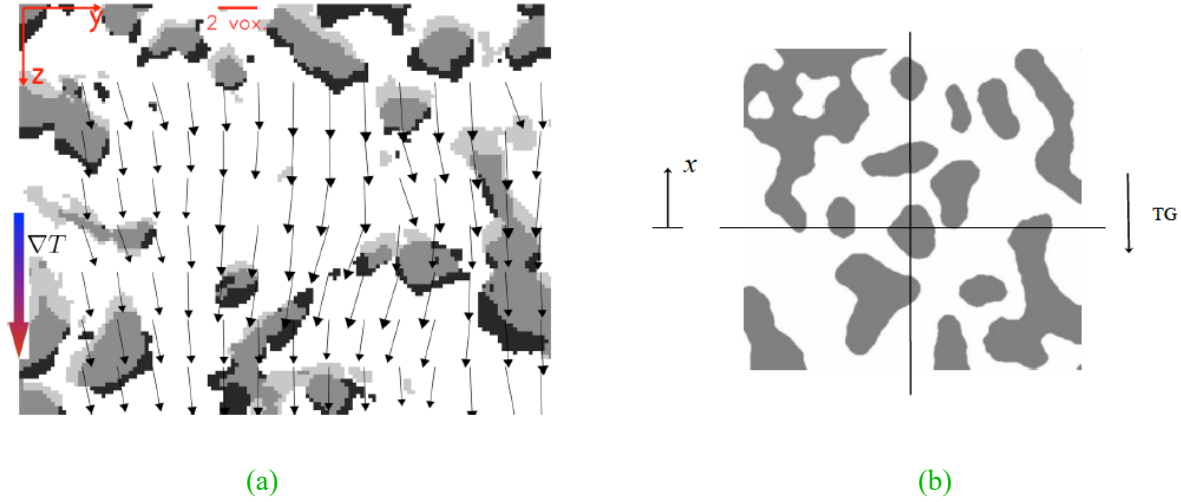


Figure 1. (a) Displacement vector field of Pinzer et al. (2012). The water vapor flux is inferred in the opposite direction. (b) 2-D surface section of a vertical slice of a snow microstructure.

Next, consider a typical surface section of snow from a vertical slice through a RVE, Figure 1(b). A strong negative temperature gradient is assumed in the x (vertical) direction. The macroscale mass flux may be determined by volume averaging the local microscale mass flux in the RVE. To compute this volume average via an integration, consider a differential length $d\xi_1$, aligned with the temperature gradient direction. In particular, let us follow the integration path along the vertical line of the surface section of Figure 1(b). As integration proceeds along this line, two immediate consequences of the ice microstructure are introduced. First, there is no vapor flux in the ice phase—the ice occupies a portion of the vertical path where diffusion cannot occur. Any reduction in the path where diffusion can occur clearly reduces the integrated average of water vapor diffusion along the path of integration. Moreover, the amount of this reduction in diffusion is equivalent to the ice volume fraction, ϕ_i , as the lineal fraction of the ice phase is equivalent to the volume fraction of ice.

The volume reducing ice blockage is readily visible as one follows the vertical line of Figure 1(b). However, there is an important counter effect in that the ice phase also acts to increase the apparent diffusion velocity, as the total straight-line path traversed by water molecules is shortened according to

$$L^* = \left(L_{\text{ha}} / L_T \right) L_T = \phi_{\text{ha}} L_T \quad , \quad (1)$$

where L_T is the total distance across the RVE and L^* is the apparent distance traveled by water vapor molecules. The apparent distance traveled has the effect of creating an apparent macroscale (average) diffusion velocity given by

$$v_v^* = v_v / \phi_{\text{ha}} \quad . \quad (2)$$

Hence, the ice phase along the vertical line of Figure 1(b) acts as a volume reducing blockage, slowing the averaged diffusion by ϕ_{ha} , while increasing the apparent diffusion velocity by $(1/\phi_{\text{ha}})$. The net effect is that mass transfer may be obtained by integrating in the ξ_1 direction of the humid air phase only, using the actual local diffusion velocity as

$$dj_s = \frac{1}{A} \left(\frac{1}{L_{ha}} \int \gamma_v(\xi) v_v(\xi) d\xi_1 \right) d\xi_2 d\xi_3 , \quad (3)$$

where A is the horizontal cross-section of the RVE.

The above integral expression in brackets holds for any vertical line through a point in the $\xi_2 - \xi_3$ plane. Hence, integration over the entire volume leads to

$$j_s = \left(\frac{1}{V_{ha}} \right) \iiint_{V_{ha}} \gamma_v(\xi) v_v(\xi) dV . \quad (4)$$

The above result is nonintuitive at first blush. The effect of the ice phase having zero diffusion velocity is easily seen physically. However, the apparent diffusion velocity is a more abstract beast, but its effect is quite real.

I believe the cancelling effects of the volume reducing ice blockage slowing diffusion and the elevated apparent velocity increasing diffusion addresses the Reviewer's question. However, it is useful to proceed a bit further here in the development.

Utilizing Fick's law for the humid air mass flux in Eq. (4) leads to the 1-D mass flux

$$j_s = \left(\frac{1}{V_{ha}} \right) \iiint_{V_{ha}} -D_{v-a} \left(\frac{d\gamma_v}{d\theta} \right) \left(\frac{\partial\theta}{\partial\xi_1} \right) dV . \quad (5)$$

Finally, noting the one-dimensional form of Eq. (11) of the paper, there follows

$$D_s \left(\frac{d\theta}{dx} \right) = D_{v-a} \left(\frac{d\theta}{dx} \right)_{ha} . \quad (6)$$

The implication of Eq. (6) is profound. Because the thermal conductivity of ice is greater than that of humid air, the humid air temperature gradient is greater than or equal to the temperature gradient of the snow mixture. This implies $D_s \geq D_{v-a}$, indicating diffusion in snow is enhanced compared to diffusion of water vapor in air only.

The underlying assumptions leading to Eq. (6) are that water vapor transport in snow is diffusion limited and the diffusion path is one-dimensional. In this case, the influence of ice blockage is countered precisely by the apparent diffusion velocity resulting from a shortened diffusion path. We again refer to the results of Pinzer (2012), suggesting a 1-D diffusion path for water vapor in snow is a reasonable assumption.

The results of the one-dimensional vapor model for snow defined by Eq. (6) are also interesting in that this expression is identical to the upper bound model. The distinguishing feature separating these two models is the value of the humid air temperature gradient.

Finally, in the actual diffusion process in snow, water molecules may follow a curvilinear path. However, the displacement fields in Figure 1(a) above suggest that the deviations from a straight-line path are not significant—an admittedly qualitative observation. Curvilinear diffusion paths will introduce some error in the analysis developed above but, as a first approximation, it is believed to be a reasonable assumption.

3. Surface Kinetics:

From the manuscript, I understand the models would work especially well when diffusion through pores is very slow as compared to sublimation/deposition processes. Would the model still be valid if surface kinetics phenomena are of the same order or longer than diffusion times? At least a small paragraph would be welcome on this topic.

The issue of the time scale of surface kinetics to that of diffusion is an astute observation and an important question raised by the Reviewer. The Reviewer is correct in that the models will work well “when diffusion through pores is very slow compared to sublimation/deposition processes.” Support for this assumption may be found through estimating the time scale for a supersaturated vapor state in snow to reach saturated vapor equilibrium. Hobbs (1974) provides an expression for the condensation of water vapor to ice driven by the difference in the vapor pressure and the saturated vapor pressure of ice ($p - p^{\text{sat}}$). Foslien (1994) used the expression of Hobbs (1974) to show the time for this vapor pressure difference to dissipate to 0.1% of the original difference is on the order of $(10)^{-3}$ seconds in snow. In contrast, the time scale of interest for diffusion is on the order of minutes, hours, or days—a much slower process compared to the sublimation/deposition process.

If the surface kinetics time scale is on the same order or longer than diffusion times, errors in the model would certainly increase. Slower surface kinetics would increase the length of paths taken by water molecules as they would have a greater tendency to move around ice grains blocking their motion. In the extreme, the results would be driven toward a tortuosity model where the ice may be viewed as a solid that all water vapor must navigate around.

Slower surface kinetics may also lead to a build-up of the mass concentration at the ice/humid air interface, leading to a supersaturation of the vapor pressure. However, the numerical results of Foslien (1994) suggest this supersaturated state is dissipated several orders of magnitude faster than the diffusion process. The experimental observations of Pinzer (2012) further support that diffusion paths involve significant hand-to-hand vapor exchange, leading to largely straight-line diffusion paths. Pinzer (2012) observed mass turnover of as much as 60% of the total ice mass in one day.

A discussion similar to that provided here will be introduced in a revised draft of the paper.

4. Discussion of the model parameters and domain of validity of the proposed results. From the 3 equations (55 to 57) used by the author to model the ADC and from the related figures (Fig. 8-9), it clearly appears that the diffusion enhancement only depends on the volume fraction of ice (1 parameter only). This description has the advantage to be very simple, but raise several questions, e.g.: -What about the impact of the ice microstructure (grain and pore size, connectivity, ice shapes, anisotropy...) on the real ADC? E.g., are the results the same for an horizontally (cf Fig. 6) or a vertically oriented layered microstructure of same volume fractions? Have large open pore structures the same ADC than small closed pore structures of the same volume fractions? Formulas have been obtained under strong hypotheses (isotropy, e.g.), but problems related to ADC are inherently linked to snow microstructures obtained under strong TG, i.e., which typically involve the formation of vertically elongated anisotropic structures. -What about temperature effects? When temperature decreases (e.g. from -0.5 to -5°C), metamorphism is known to be strongly inhibited while diffusion coefficient and conductivities stay nearly the same (cf e.g. Massmann, 1998 and Calonne et al, 2011). Is the real ADC impacted by temperature and how is it reflected by the proposed formulas? All these questions are probably difficult to precisely answer in this manuscript but some hints could be given in the discussion.

At least, the main hypotheses of the model should be recalled in the conclusion part.

The Reviewer poses multiple fascinating questions here and I will take a run at answers to each them below. I will also add similar commentary to a revised draft of the paper.

1. A good deal of the Reviewer's comments surrounds the question of the effects of anisotropy.

“What about the impact of the ice microstructure (grain and pore size, connectivity, ice shapes, anisotropy...) on the real ADC? E.g., are the results the same for an horizontally (cf Fig. 6) or a vertically oriented layered microstructure of same volume fractions? Formulas have been obtained under strong hypotheses (isotropy, e.g.), but problems related to ADC are inherently linked to snow microstructures obtained under strong TG, i.e., which typically involve the formation of vertically elongated anisotropic structures.”

The diffusion coefficient model developed herein relies on isotropic thermal conductivity as developed by Foslien (1994). However, anisotropy may enter the development through the influence of the humid air temperature gradient $(d\theta/dx)_{ha}$ in Eq. (40) of this original draft, specifically

$$D_s \left(\frac{d\theta}{dx} \right) = D_{v-a} \left(\frac{d\theta}{dx} \right)_{ha} .$$

Below are some of my thoughts on the topic of anisotropy although the subject warrants a thorough investigation.

During kinetic metamorphism, recrystallization causes the formation of thicker vertical-oriented ice structures aligned with the TG. In rough terms, the structure is trending toward a columnar pore microstructure of Figure 6(a) of the paper. As a result, the thermal conductivity is expected to rise, as the columnar pore microstructure represents an upper bound on thermal conductivity for a given density. The work of Calonne et al. (2011) and Riche and Schneebeli (2013) show the vertical thermal conductivity for kinetic growth microstructures is generally greater than the horizontal conductivity.

In the case of diffusion, I believe the opposite is true in that the columnar pore microstructure retards apparent diffusion compared to a general snow microstructure due to a lower TG in the humid air phase. Hence, the apparent diffusion is expected to be lower in the vertical TG direction than in the horizontal direction. I'll note this description is precisely the opposite of what is shown in Calonne et al. (2014). This contrast is directly attributable to the different definition of the diffusion coefficient used by Calonne et al. (2014) versus the Apparent Diffusion Coefficient studied here. Fundamentally different diffusion mechanisms are at play in the respective models.

Relative to diffusion in the horizontal direction, my experience with studying elastic properties of composite materials suggests that the structural changes of kinetic metamorphism in the vertical direction will have minimal effect on the horizontal diffusion coefficient.

In summary, I expect kinetic metamorphism caused by a vertical temperature gradient will lower the diffusion coefficient in the vertical direction and have minimal effect on the horizontal diffusion coefficient.

To quantify anisotropic diffusion effects, one needs to evaluate the humid air TG for snow exhibiting kinetic crystal growth. The most obvious means of obtaining this information is

through finite element simulations of the thermal conductivity problem. Pinzer (2012) achieved this for 3 snow samples imaged over time under a temperature gradient. One of those samples shows diffusion decreasing with time while the other two are rather steady in time. The decrease in diffusion (aligned with the TG direction) as kinetic growth proceeds would support the conjecture above. As I mentioned, a thorough understanding of anisotropy of the ADC warrants a serious study.

2. What about temperature effects?

“When temperature decreases (e.g. from -0.5 to -5°C), metamorphism is known to be strongly inhibited while diffusion coefficient and conductivities stay nearly the same (cf e.g. Massmann, 1998 and Calonne et al, 2011). Is the real ADC impacted by temperature and how is it reflected by the proposed formulas?”

The three models presented in the paper assume the same functional form regardless of temperature. Temperature effects do enter the numerical values resulting from the formulas through the temperature dependence of the diffusion coefficient for water vapor in air. Massman (1998) recommends the value for D_{v-a} as

$$D_{v-a} = 0.2178 \left(\frac{\theta}{\theta_0} \right)^{1.81} \text{ cm}^2 \text{ s}^{-1} ,$$

where θ is the absolute temperature and $\theta_0 = 273.15 \text{ K}$ ($=0 \text{ }^\circ\text{C}$). Hence, for a temperature drop from $-0.5 \text{ }^\circ\text{C}$ to $-5 \text{ }^\circ\text{C}$, the diffusion coefficient of water vapor through air drops by approximately 3%. However, note that the *normalized* diffusion coefficient remains unchanged, as D_{v-a} divides out of the normalized expression.

There is an additional temperature effect in the diffusion of water vapor in snow in that the thermal conductivities of ice and humid air are functions of temperature. These changes in thermal conductivities produce changes in the constituent temperature gradients. Specifically, as the temperature drops, the humid air thermal conductivity drops while the thermal conductivity of ice rises. Both of these effects contribute to a rise in the humid air temperature gradient. This result would suggest a rise in the ADC, thereby mitigating the effects of changes in the diffusion coefficient of water vapor in air described above. The models developed here cannot account for the effects caused by changes in thermal conductivity with temperature.

In summary, any expected changes in the ADC due to changes in temperature are expected to be quite small.

3. What about the effects of grain, pore size, connectivity, and ice shapes?

“What about the impact of the ice microstructure (grain and pore size, connectivity, ice shapes, anisotropy...) ... Have large open pore structures the same ADC than small closed pore structures of the same volume fractions?”

The question of the impact of ice microstructure is difficult to answer with complete confidence. However, some insight can be achieved by examining results from Christon (1994) and Pinzer (2012). The microstructures are wildly different, yet, the ADC predictions in each case are quite similar. I am not surprised by the similarities in predicted values of the ADC, as the ADC is computed based on a volume averaging of diffusion in a RVE which, in turn, is a function of the volume averaged local (microscale) temperature gradient. The volume averaging process in a linear problem is very forgiving with regard to the influence of microstructure. Brockenbrough

(1991) demonstrates this for the computation of the elastic modulus of composite materials using both periodic and random microstructures. Elastic properties, governed by volume averaged local stress/strain fields, are largely insensitive to microstructure.

Additional insight into the variability of the ADC may be obtained by examining the directional variability in the thermal conductivity calculations of Calonne et al. (2011) and Riche and Schneebeli (2013). In this case, the snow microstructure is, indeed, directionally different and pore size and connectivity may vary substantially in the vertical and horizontal directions. The finite element predictions do show a difference in predicted directional thermal conductivity, suggesting that ice microstructure does play a role in the value of the ADC, albeit a small one compared to changes in density.

While the impact of ice microstructure on the ADC may be small, the impact of microstructure on surface kinetics is believed to be significant. I agree with a conclusion of Christon (1994):

“Geometrical effects alone explain most of the transport phenomenon of heat and mass transport in dry—snow, effects heretofore attributed to surface kinetics or nonequilibrium thermodynamics. However, such effects are real and affect the details of specific growth habit (shape) of ice grains, but they do not appear to explain the heat and mass transport process.”

Support for the role of microstructure in surface kinetics is found in Yokoyama (1990) who show that the pattern of growing crystals depends on the temperature, supersaturation, and dimensionless crystal size which is, in turn, a function of the mean free path of a water molecule.

The 3 topics mentioned in the Reviewer’s comment will be addressed in a subsequent draft of the paper.

Detailed and minor comments:

p. 2, lines 3-4: *“The net effect is that ice grains act as an instantaneous source and sink of water vapor, thereby shortening the diffusion path of a water molecule”*. I would replace “instantaneous” by “quasi-instantaneous” (here, and everywhere in the paper) : depending on the conditions (temperature, facet orientation, etc.), the kinetics at the interface may strongly impede the phase change and impact the resulting geometry. See e.g. Yokoyama, 1990; Libbrecht et al, 2005; Flin and Brzoska, 2008; Libbrecht and Rickerby, 2013, etc.

The Reviewer’s point is well taken here. I agree with the suggestion of replacing “instantaneous” by “quasi-instantaneous” throughout the text.

p. 3, lines 1-2: *“For instance, the numerical studies of Christon (1990), Pinzer et al. (2012), and Calonne et al. (2014) all use different methods to evaluate the mass flux and/or the diffusion coefficient.”* Actually, as truly explained by the author just a line before, the main problem is probably not the method, but the definition. E.g., in Calonne et al, 2014, we did not consider the sublimation condensation effects (mechanism #2 of the author’s definition), as it is not a real diffusion process, but a phase change that can be viewed as contributing to the overall apparent diffusion process. Replacing “methods” by “definitions” might probably be more accurate.

I agree with this comment and I will replace the word “methods” by “definitions.” The distinction is important as the fundamental definitions lead to different results. The word “methods” may call into question the correctness of the work and that was not the intent.

p. 4, lines 15-19: “*Convection is neglected. Convection only occurs in extreme weather conditions such as near the top of a snowpack in the presence of a **strong wind** or extremely large temperature gradients. Foslien (1994) provides support for this assumption through the calculation of a Rayleigh number for porous media. His results show the Rayleigh for snow is an order of magnitude below the number required for the onset of convection...*”

What kind of convection the author actually wants to address here? -1) Forced convection as in Calonne et al, 2015 (i.e., wind pumping -with e.g. Reynolds number, etc.) or-2) Natural convection as in Kaempfer et al (2005) (i.e., natural convection, due to the fact that cold air is heavier than warm one -with Rayleigh number)? As the author knows, the physical mechanisms (and associated characteristic numbers) are different for these two distinct processes. It would be better to address these two processes in distinct paragraphs.

I agree with the Reviewer in that forced convection and natural convection should be discussed separately.

Forced convection in an alpine snow cover occurs in extreme weather conditions such as near the top of a snowpack in the presence of a strong wind. Foslien (1994) provides support for neglecting forced convection through the calculation of a Rayleigh number for porous media. His results show the Rayleigh number for snow is an order of magnitude below the number required for the onset of forced convection.

Natural convection at the scale of an RVE is also assumed negligible. Hammonds et al. (2015) provides support for this assumption through calculations of the Grashof number. Microscale convection is also a possibility. Using the Knudsen number, Hammonds et al. (2015) suggest that microscale convection effects do not become relevant until pore diameters fall below 50 μm .

While natural convection effects are negligible for the scale of the RVE and below, large scale free convection has been observed in natural snowpacks (Sturm and Johnson, 1991). Clearly, the geometric scale of the problem must be accounted for in the consideration of natural convection.

p. 8, line 15-16: “*Vapor diffusion in snow is driven by temperature gradients and, therefore, it is useful to examine relations between macroscale and microscale temperature gradients.*” This is a small detail, but strictly speaking, the first part of the sentence is not perfectly true, as vapor diffusion in snow can be also caused by curvature effects (see e.g. Brzoska et al, 2008), at least at microscale. Replacing “Vapor diffusion in snow” by “Macroscale vapor diffusion in snow” or “Large scale vapor diffusion in snow” would be more accurate.

I agree with the Reviewer’s comments here in that vapor diffusion in snow can be driven by curvature effects in addition to local temperature gradients. On page 8, lines 15-16, I will replace “Vapor diffusion in snow” by “Macroscale vapor diffusion in snow” as suggested by the Reviewer.

In addition to making the noted change, I will add a brief paragraph to address the significance of curvature effects—similar to the following.

Kelvin’s law shows that the equilibrium vapor pressure over curved surfaces exceeds that of flat surfaces. Christon (1994) and Flanner and Zender (2006) show that the correction for a curved surface only becomes significant for radii of curvature below $(10)^{-7} \text{m}^{-1}$. Hence, the ice surface in a seasonal snow cover may be considered flat relative to the Kelvin effect. Curvature effects are of importance for short periods in diffusion and metamorphism of freshly fallen snow (Colbeck, 1980).

p. 6, line 10: “To account for this altered time scale for water vapor diffusion, the notion of intrinsic time,...” For me, the terminology “intrinsic” is unclear in this context. I would suggest to replace “intrinsic” by “apparent”, here and everywhere in the text.

I agree with the Reviewer’s comment. The word “intrinsic” will be replaced by the word “apparent” throughout the paper.

p. 16, Fig. 7: “x-axis: kg m -3”
=> blank spaces to be suppressed.

Blank spaces will be removed—thank you

p. 16, Fig. 7: “Figure 7. Thermal conductivity analytical prediction of Foslien (1994) versus finite element predictions of Riche and Schneebeli (2013).” To my understanding, the comparison made here is a comparison with a very specific fit of the dataset of Riche and Schneebeli (2013), namely the vertical component of the thermal effective conductivity, and for FC and DH only (see Fig 7, p 224: “k_z FC and DH”). In addition, I am not sure the red curve of Riche and Schneebeli (2013) can be really considered as “a finite element prediction”: for me, it is rather a fit computed from a subset of k_z values (FC and DH only), obtained from their whole dataset.

I agree with the comments of the Reviewer and I will revise the text to reflect that the curve of Riche and Schneebeli is a curve fit of specific k_z values. I will also add a curve fit from Calonne et al. (2011) along with the prediction of Foslien (1994) to this curve fit. Additional discussion regarding the figure will also be provided in a revised draft of the paper.

p. 16, line 10: missing brackets or parentheses.

Parentheses will be added—thank you

p. 17, lines 14-15: “Preserving the ratio of (k_i/k_{ha}) is a necessity to preserve the local temperature gradient field obtained from the heat transfer analysis.” At first glance, I found this idea quite difficult to understand. The author should add some explanations (or an example on a specific configuration, with a figure?).

In what follows, I develop the results of interest without using the ratio (k_i/k_{ha}). When the development is complete, I return to this ratio to bring out its physical significance.

To determine an expression for the apparent diffusion coefficient, begin by solving the heat conduction problem and volume averaging to determine the macroscale thermal conductivity. Heat conduction at the microscale is defined by the boundary value problem (BVP) over a RVE as (Calonne et al., 2011):

$$\begin{aligned}\nabla \cdot (k_i \nabla \theta_i) &= 0 \quad \text{in } \Omega_i \quad , \\ \nabla \cdot (k_{ha} \nabla \theta_{ha}) &= 0 \quad \text{in } \Omega_{ha} \quad , \\ \theta_i - \theta_{ha} &= 0 \quad \text{on } \Gamma \quad , \\ (k_i \nabla \theta_i - k_{ha} \nabla \theta_{ha}) \cdot \mathbf{n} &= 0 \quad \text{on } \Gamma \quad .\end{aligned}\tag{7}$$

For a 1-D macroscale temperature gradient of $\frac{d\theta}{dx} = 1$, the thermal conductivity for snow is given by

$$k_s = \frac{1}{V} \left(\int_{V_{ha}} k_{ha} \frac{d\theta}{d\xi} dV + \int_{V_i} k_i \frac{d\theta}{d\xi} dV \right) . \quad (8)$$

The thermal conductivity model of Hansen and Foslien (2015) provides a good approximation to Eq. (8) as demonstrated in Figure 7 of the paper. This model is given by

$$k_s = k_{ha}(1 + \phi_i \phi_{ha}) + k_i \phi_i^2 . \quad (9)$$

An important point in the development of the ADC is recognition that diffusion at the microscale is driven by the local temperature gradient field whose solution is driven by Eq. (7)—neglecting latent heat effects. A solution for diffusion is then approximated from Eq. (9) using the substitutions

$$k_{ha} \rightarrow D_{v-a} \quad \text{and} \quad k_i \rightarrow 0 \quad ,$$

leading to

$$D_s = D_{v-a}(1 + \phi_i \phi_{ha}) . \quad (10)$$

Note that the above substitutions are made *after* solution of the boundary value problem. Eq. (10) is the expression for the “tortuosity model” given as Eq. (54) in the paper. While one could stop here, there are subtleties that deserve further explanation.

Let us look at the work of Calonne et al. (2014). They begin by solving the diffusion problem of Eq. (7), appropriately non-dimensionalized, using the same substitutions noted for the development of the ADC, namely

$$k_{ha} \rightarrow D_{v-a} \quad \text{and} \quad k_i \rightarrow 0 .$$

Importantly, if I understand correctly, these substitutions are made prior to solution of the boundary value problem. Hence, the local temperature fluctuations are no longer the same as they are for the thermal conduction BVP. The resulting expression for diffusion assumes the form

$$D_s = \phi_{ha} D_{v-a} \tau_{ha} \quad , \quad (11)$$

where τ_{ha} is the tortuosity of the humid air phase (Calonne, et al., 2014). The above leads to a diffusion coefficient less than one while the normalized ADC of Eq. (10) is greater than one. The differences in the results of Calonne (2014) and the approach taken here are subtle, and the results are a consequence of fundamentally different definitions of the diffusion coefficient.

Now, let us revisit the governing equations given by Eq. (7) for the diffusion problem where the ice phase is treated as a quasi-instantaneous diffusing medium—the spirit of the ADC. To achieve this for the diffusion problem, we recast Eq. (7) with the substitutions

$$k_{ha} \rightarrow D_{v-a} \quad \text{and} \quad k_i \rightarrow \left(\frac{k_i}{k_{ha}} \right) D_{v-a} .$$

The purpose of the latter substitution is to produce the identical local TG field as the thermal conduction problem, as diffusion is driven by the local temperature gradients. Because the problem described in Eq. (7) involves homogeneous linear pde’s, only the ratio of k_i to k_{ha} matters, and this ratio is preserved with the above substitutions for the diffusion problem.

An interpretation of the latter substitution is that $\left(\frac{k_i}{k_{ha}}\right) D_{v-a}$ is a description of a quasi-instantaneous diffusion coefficient in the ice phase. In this context, the diffusion time in the ice phase is quasi-instantaneous compared to the humid air phase. Now return to the arguments found in Item 2 of the General Comments where the path of a water vapor molecule is largely one-dimensional as shown in Figure 1(a). An apparent diffusion velocity in the humid air phase is then given by

$$v^* = \frac{v_v}{\phi_{ha}} .$$

Following arguments from Item 2 of the General Comments, we are led to the expression for the mass flux given by

$$\begin{aligned} j_s &= \gamma_v v_v = \frac{1}{V_{ha}} \int_{V_{ha}} \gamma_v(\xi) v_v(\xi) d\xi \\ &= \frac{1}{V_{ha}} \int_{V_{ha}} D_{v-a} \left(\frac{d\theta}{d\xi}\right) dV \end{aligned} \quad (12)$$

Eq. (12) may be found in the paper as Eq. (59).

p. 17, lines 26-27: “Note that the averaging process is done over the humid air volume as opposed to the total volume as the effects of ice blockage and the shortened diffusion paths from hand-to-hand vapor transport cancel each other out.” Same as just before: some additional explanations would be welcome.

I have provided a detailed explanation of this statement in Item 2 of the “General Comments.”

p. 21, lines 20-25: “Perhaps the most extensive numerical study of mass transport is found in Calonne et al. (2014). They performed finite element analyses for 35 RVE’s of snow spanning a density range from 100–500 kgm⁻³. Unfortunately, it is not possible to reconcile their results with the analytical models presented, as they solved a fundamentally different boundary value problem for mass transfer driven by the relation substitutions shown in (51) where the thermal conductivity of ice was set to zero. In contrast, in the analytical models, the influence of k_i is retained to obtain the local temperature gradient field. The influence of k_i is then removed in the calculation of the diffusion coefficient.”

I agree with the author. However, it should be mentioned that, in the model of Calonne et al, phase change can be described using specific sources terms (see Calonne et al, 2014 and Calonne et al, 2015).

If I understand the Reviewer correctly, the source terms being referenced here to describe phase change are the same as the “mass supply” terms in the work of Foslien (1994) and Hansen and Foslien (2015). Following Hansen and Foslien (2015), the governing equations of energy balance and mass balance are, respectively,

Energy Balance

$$\left(\phi_{ha} \gamma_{ha} C_{ha}^V + \phi_i \gamma_i C_i^V\right) \frac{\partial \theta}{\partial t} = \frac{\partial}{\partial x} \left(k \frac{\partial \theta}{\partial x}\right) + \mu_{sg} \hat{c} \quad (13)$$

Mass Balance

$$\hat{c} = \frac{\partial}{\partial x} \left(D_s \frac{dy_v}{d\theta} \frac{\partial \theta}{\partial x}\right) - \phi_{ha} \frac{dy_v}{d\theta} \frac{\partial \theta}{\partial t} \quad (14)$$

The above equations are identical in form to those developed in Calonne et al. (2014, 2015).

The mass supply, \hat{c} , represents the rate at which the ice phase is gaining mass due to phase change, with units of $(\text{kg m}^{-3} \text{ s}^{-1})$. A positive value indicates deposition is occurring while a negative value implies sublimation is occurring.

In the mechanics of a solution to Eqs. (13) and (14), assuming a saturated vapor pressure, the mass supply does not drive the diffusion process. Rather, diffusion is a major factor driving the mass supply (phase change) i.e., the value of the mass supply is a computationally derived result. The importance of using a proper definition of the diffusion coefficient becomes clear in this context.

The mass supply may be viewed as an independent source/sink term for mass exchange if one assumes the water vapor pressure is not saturated. Saturated vapor pressure is generally assumed for the time scale of vapor diffusion.

p. 22, lines 15-16: *“Arguments from stereology, combined with experimental observation, suggest that the influence of ice blocking diffusion paths is canceled out by the shortened diffusion paths from hand to hand vapor transport.”* Same as p. 17, lines 26-27: some additional explanations or justifications would be welcome.

Please see Item 2 of the “General Comments” for a detailed development of this concept. Greater clarity of this subject will be provided throughout the paper in any subsequent draft.

p. 22, lines 26-27: *“Importantly, Eq. (59) represents the mass flux per unit area of snow and not just the flux across the humid air phase as the ice blockage impeding diffusion is exactly countered by the shortened diffusion paths of water molecules”* Same as just before: some additional explanations or justifications would be welcome. Minor typo: a “t” is missing in “importantly”.

The typo in “importantly” will be corrected—thank you

Eq. (59) shows the mass flux for snow is achieved by volume averaging over the humid air constituent only.

$$j_s = \gamma_v v_v = \frac{1}{V_{ha}} \int_{V_{ha}} \gamma_v(\xi) v_v(\xi) dV_{ha} \quad (59)$$

The discussion of Item 2 in the “General Comments” shows the details of the development of Eq. (59)—contrasted with the rather vague explanation I provided in the paper as noted by the Reviewer.

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