

## **Review of “A macroscale mixture theory analysis of deposition and sublimation rates during heat and mass transfer in snow” by A. C. Hansen and W. E. Foslien.**

The paper presents the macroscopic modeling of the heat and water vapor transport in dry snow derived from a mixture theory. The conduction and diffusion processes coupled to phase change are considered. Analytical expressions of the effective thermal conductivity and effective diffusion coefficient of diffusion are also proposed.

The publication fits well with the scope of The Cryosphere. The paper is well structured but some mathematical developments are difficult to follow and the definition of some quantities must be specified in order to clearly evaluate the proposed results and to avoid any misunderstanding. Moreover, since the main goal of this paper is to find a macroscopic description of the heat and water vapor transfer in snow, the paper should be more positioned with respect to the current literature on the same topic, notably the papers of Albert et al. 1993 and Calonne et al. 2014a. The novelty compared to this work, as well as the differences and similarities in results (description of the terms arising in the macroscopic model) should be pointed out.

In our detailed response below, we provide multiple comparisons with the work of Calonne (2014a). Indeed, many of the comparisons are fascinating, including similarities and differences. We have learned a great deal from their work and we hope our comparisons are informative for the reader. We thank both the current reviewer as well as Henning Lowe for bringing Calonne’s work to our attention.

In contrast, the work of Albert et al. (1993) has no relevance to the current study. Notably, Albert’s work centered on natural convection in snow and the phenomenon known as *wind pumping*. The equations developed involve a velocity for the humid air and conditions where the vapor density is not saturated. These conditions only occur in snow under extreme circumstances.

Foslien (1994) performed a dimensional analysis of the conditions needed for natural convection and showed the Rayleigh number for typical snow conditions was 1-2 orders of magnitude below what is needed for the onset of natural convection (See pp. 30-31 of the thesis in the **Supplemental Material**). As a consequence, natural convection is not considered and the present paper develops a theory with no air velocity and, further, a saturated vapor density.

### **1. Major comments**

The mixture theory allows finding the macroscopic modeling of heterogeneous materials from the physics at the microscopic scale – written in a particular form in the sense that the physical phenomena occurring at the interface between the different phases are not explicit. Such phenomena can be complex and play an important role on the macroscopic modeling. This seems to constitute a limit of the mixture theory in comparison with rigorous upscaling methods (volume averaging method, homogenization based on asymptotic developments...) that capture the interface processes.

In the present review, as well as the first review of this work, we sense a reticence toward the use of mixture theory in that it is somehow limited in capturing microstructural effects. We believe the theory should not be viewed as a competing approach but rather a complement to microscale research and upscaling.

By way of analogy, we have spent a good deal of our career studying the nonlinear response of high performance composite materials. We study deformation mechanisms at the microscale and

use the information to develop macroscale mixture models for failure analysis. Results from macroscale analyses may then be used as inputs to study microscale behavior, in high stress regions of a structure for example.

In precisely the same manner, the mixture theory work presented here-in relies on microscale modeling for macroscale material properties. The mixture theory for snow can then simulate a host of interesting macroscale problems, featuring terrain changes, ice crusts, surface effects, snow storm deposition, extreme temperatures, etc. Heat and mass transfer results from a mixture theory analysis could then be used as *inputs* for a host of interesting microstructural studies.

In our response to the first review of this paper, we provided what we believe is a genuinely interesting study case where we showed an uncoupling of mass transfer from heat transfer. Specifically, under a strong negative temperature gradient, we showed conditions where either sublimation or condensation could be occurring. Current microstructural models could not predict this as the lone input to a typical microscale analysis is the temperature gradient whereas mass transport is an outcome predicated on the temperature gradient.

In the present paper, the macroscopic description is given by two coupled equations, for the water vapor transfer (41) and for the heat transfer (57). These equations are

coupled through a source term  $\hat{c}$ , which seems not to be given by the mixture theory but must be postulated (section 4.2) These equations involved two effective parameters, the effective diffusion coefficient  $D_s$  and the effective thermal conductivity  $k_s$ . At this stage, equations (41) and (57) are similar as the one proposed by Albert et al 1993 using a phenomenological approach, or by Calonne et al. 2014a using an upscaling approach. Let us remark that in the latter work, the method allows the authors to rigorously define, from the physics involved at the microscale, the effective properties and the source terms induced by the phase change at the ice/air interface. Moreover, they have shown theoretically that both effective coefficients  $D_s$  and  $k_s$  do not depend on the phase change occurring at the pore scale, but depend only on the intrinsic properties of the constituents (coefficient of air diffusion, and ice and air thermal conductivity, respectively) and the microstructure.

The coupling of the water vapor transfer and heat transfer in the mixture theory is rigorously developed from the governing field equations for a volume fraction mixture theory, i.e., Eqs. (20-26). The reviewer's reference to Section 4.2 is not relevant as this entire section is devoted to simply showing the vapor density is saturated. Other than this purpose, the equations in Section 4.2 are never utilized elsewhere. As an aside, the key equations in Section 4.2 for the mass supply are not postulated but rather taken directly from the work of Hobbs (1974), *Ice Physics*.

Concerning the derivation of equation (57), the thermal flux  $q_{ha}$  in equation (49) includes a source term due to phase change. Why a source term is not present in the thermal flux  $q_i$  in equation (45)? Since the phase change occurs at the ice-air interface, it does not concern only one phase. This point must be clarified. This remark also hold for equations (66) and (67).

The reviewer is mistaken here as Eqs. (49) and (67) do not involve a source term due to phase change. Rather, these equations focus on the humid air as a pure substance and the energy flux developed follows the classic work on *Transport Phenomena* by Bird et al.(1960). In brief, the total energy flux for a mixture such as humid air may be written as

$$\mathbf{q} = \mathbf{q}^{(c)} + \mathbf{q}^{(d)}$$

where  $\mathbf{q}^{(c)}$  is the conductive flux and  $\mathbf{q}^{(d)}$  represents “contribution from the interdiffusion of the

various species present” (Bird, 1960). The explanation provided also serves to clarify why there is no comparable term in the energy flux for pure ice (Eqs. 45 & 66) as there is no diffusion present.

It seems that the main difference between previous works (see for example Albert et al. 1993, Calonne et al. 2014a) and the one presented in the paper is found in the definition of the source term  $\hat{c}$  given by equation (58). Is this term comparable to the one presented in Albert et al. 1993 and Calonne et al. 2014a? What are the main differences? What is the expression of  $\gamma^{l\#}(T)$ ? Is it given by the classical Clausius-Clapeyron relationship?

We have previously discussed the work of Albert (1993) and reiterate that their work is not relevant to the theory developed in this paper. In contrast, the work of Calonne (2014a) is closely connected to the present paper and provides several opportunities to draw comparisons and/or distinctions.

To begin, Calonne (2014a) develops macroscale equations that are identical in form to those arising in Foslien (1994) using the volume fraction mixture theory. The comparison is best shown by examining Eqs. (20) and (21) of Calonne with Eqs. (41) and (57) of the present work. Specifically, multiplying the RHS of Eqn. (20) in Calonne by  $(\rho_i/\rho_i)$  and relabeling  $(L_{sg}/\rho_i)$  as  $u_{sg}$  results in Eqn (41) of the present paper. Eqn. (57) is already identical in form to Eqn. (21) of Calonne. Using consistent units, the numerical value of  $(L_{sg}/\rho_i)$  of Calonne is 2.835e6 compared to Foslien’s value of  $u_{sg} = 2.85e6$ . The governing equations are, indeed, the same.

We are delighted to see that the multiscale expansion of Calonne produced the identical equations to that of the volume fraction mixture theory of Foslien. It is gratifying to see two distinctly different approaches produce the same result. While the equations governing the macroscale response are identical, the majority of Calonne’s work focusses on upscaling whereas the present paper focusses on the solutions of the macroscale response. We will also point out distinct differences in calculation of the effective diffusion coefficient.

In response to the reviewer’s query regarding  $\gamma_v^{sat}(T)$ , we did not use the classical Clausius-Clapeyron relationship directly. Rather, following Dorsey (1968)

$$\gamma_v^{sat} = \frac{101325}{760R_w\theta} 10^{\left\{ \frac{-2445.5646}{\theta} + 8.2312 \log_{10} \theta - (1.677006 \times 10^{-2})\theta + (1.20514 \times 10^{-5})\theta^2 - 6.757169 \right\}}$$

The above equation for the saturated vapor density was used because it provides an accurate representation of the vapor density through a broad range of temperatures.

Herein, lies a difference between the present work and Calonne’s upscale modeling. Specifically, we wish to solve the governing equations over the physical domain of a snow cover and, as such, our numerical parameters must be valid over the entire range of densities and temperature one may expect to encounter. In contrast, the upscaling performed by Calonne effectively applies to an RVE with a single temperature and density. We mention this only to further clarify the focus of the two papers.

Finally, what is the relationship between  $k_s$  in (57) and  $k_i^{\text{eff}}$  and  $k_a^{\text{eff}}$  in (46) and (52), respectively? Concerning  $k_i^{\text{eff}}$  and  $k_a^{\text{eff}}$ , do they depend on the temperature only or other variables?

To begin, let us differentiate between  $k_i$  and  $k_i^{\text{eff}}$ .  $k_i$  is the standard thermal conductivity of ice as a pure substance. In contrast,  $k_i^{\text{eff}}$  represents an effective thermal conductivity of an ice phase in snow.  $k_i^{\text{eff}}$  differs from  $k_i$  due to the complex microstructural network of the ice phase. The tortuosity of the ice phase, for example, plays a role in  $k_i^{\text{eff}}$ . The only microstructure where the two would be equal for one-dimensional heat transfer would be the *pore microstructure* we use in the present paper. In a 3-D analysis, we don't believe the two terms are ever equal.

This same discussion applies to thermal conductivities of the humid air.  $k_{ha}$  is the standard thermal conductivity of humid air. In the case of snow, there is an apparent thermal conductivity denoted  $k_{ha}^{\text{eff}}$  that is different than  $k_{ha}$ , again caused by the complex microstructure of the humid air. In brief, just as the thermal conductivity of snow,  $k_s$ , is influenced by microstructure, so is  $k_i^{\text{eff}}$  and  $k_{ha}^{\text{eff}}$  as all three are macroscale quantities. As such, they would depend on a host of microstructural variables other than temperature.

Fortunately, for the time scales of interest in our work, we showed the constituents have a uniform temperature, thereby allowing the constituent energy equations to be added. The resulting thermal conductivity for snow,  $k_s$  effectively allows us to absorb  $k_i^{\text{eff}}$  and  $k_{ha}^{\text{eff}}$ . While we never compute these effective conductivities, it would be important to do so if one wanted to study non-equilibrium constituent temperatures on a short time scale with a mixture theory.

We can also make a direct connection of  $k_i^{\text{eff}}$  and  $k_{ha}^{\text{eff}}$  to the work of Calonne (2014a). First note that in mixture theory, the mixture energy flux is given by the sum of Eqs. (45) and (49). Assuming the macroscale (volume averaged) constituent temperatures are equal, the sum of Eqs. (45) and (49) leads to an effective thermal conductivity for snow given by

$$k_s = \phi_i k_i^{\text{eff}} + \phi_{ha} k_{ha}^{\text{eff}} \quad (1)$$

Now consider Eqn. (25) of Calonne (2014).

$$k^{\text{eff}} = \frac{1}{|\Omega|} \left( \int_{\Omega_a} k_a (\text{grad } \mathbf{t}_a + \mathbf{I}) d\Omega + \int_{\Omega_i} k_i (\text{grad } \mathbf{t}_a + \mathbf{I}) d\Omega \right)$$

The above equation may be rearranged as

$$k^{\text{eff}} = \phi_a \frac{1}{|\Omega_a|} \int_{\Omega_a} k_a (\text{grad } \mathbf{t}_a + \mathbf{I}) d\Omega + \phi_i \frac{1}{|\Omega_i|} \int_{\Omega_i} k_i (\text{grad } \mathbf{t}_a + \mathbf{I}) d\Omega \quad (2)$$

Comparing Eqs. (1) and (2) provides a mathematically clear interpretation of  $k_i^{\text{eff}}$  and  $k_{ha}^{\text{eff}}$  as:

$$k_{ha}^{\text{eff}} = \frac{1}{|\Omega_a|} \int_{\Omega_a} k_a (\text{grad } \mathbf{t}_a + \mathbf{I}) d\Omega$$

and

$$k_i^{\text{eff}} = \frac{1}{|\Omega_i|} \int_{\Omega_i} k_i(\text{grad } \mathbf{t}_a + \mathbf{I}) d\Omega .$$

This analogy will become important when discussing the effective diffusion coefficient.

The macroscopic description is fully defined by equations (41), (57) and (58), isn't it? All other expressions of the macroscopic modeling (equation (64) for example) are rather artificial from my point of view and are based on the relations (59) and (60), which are maybe true under particular conditions, but not in general (any value of temperature, temperature gradients...). The hypotheses behind these approximations are not clear and merit to be detailed. What is the domain of validity? Are they consistent with the theoretical and numerical results presented in Calonne et al. 2014a?

While it is technically correct to say the macroscopic model is defined by Eqs. (41) and (57), it would be more accurate to replace Eqn. (41) by Eqn. (63). Eqn. (63) reflects the fact that the vapor density is saturated for the time scale of interest. A saturated vapor density affords a remarkable simplification in that a constitutive law is not needed for the mass supply,  $\hat{c}$ .

The reviewer has a misconception about Eqn. (58) and perhaps all of Section 4.2. This entire section is devoted to showing the vapor is saturated for the time scales of interest in the paper. Perhaps we simply could have made this statement, as other researchers have done in the past, but we felt it was important to justify this claim. To be clear, we do not use Eqn. (58) in our results presented in Section 6.

Eqn. (58) is only utilized to show the vapor density is saturated. That said, we'll note striking similarities with Calonne (2014a) in that  $\hat{c}$  in Eqn. (58) involves the specific surface area of snow, temperature, and the difference  $(\gamma_v - \gamma_v^{\text{sat}})$  all of which also appear in Calonne's definition of the mass supply. Our expression is taken directly from the text: *Ice Physics* (Hobbs, 1974).

In order to avoid any misunderstanding, I think that  $k_s^{\text{con}+d}$  should be defined as an apparent effective conductivity. It is important also to note that  $k_s^{\text{con}+d}$  is not the *"thermal conductivity that would be measured experimentally when studying heat transfer through a snow cover"* as suggested by the authors. Indeed, we are only able to measure a temperature field or/and some heat flux. The 'thermal conductivity' is always deduced through an inverse analysis of these measurements, and so depends on the model under consideration. If the analysis is done with the relation (57) or the relation (64), it will give  $k_s^{\text{con}+d}$  or  $k_s$ , respectively.

We are quite comfortable with the suggestion to refer to  $k_s^{\text{con}+d}$  as an "apparent effective thermal conductivity." In addition, we will modify the phrase in italics above to better describe the meaning of this term. In all numerical analyses presented, we use Eqs. (61) and (63), hence,  $k_s$  is the parameter used.

In section 5.1, the determination of  $k_s^{\text{con}+d}$  and  $D_s$  for a particular microstructure is not easy to follow. What are the differences between equations (45) and (66), and between (49) and (67)? In addition, Kaempfer et al. 2009 (Part B, Fig 3) show that the contribution of the phase change to the temperature and vapor flux at the interface depends on the orientation of the normal at the interface with respect to the orientation of the temperature gradient. It seems that equation (67) and (71) do not take into

account the orientation, i.e. the contribution of the phase change will be the same for the interfaces whose normal are perpendicular (lamellae microstructure) or parallel (pore microstructure) to the direction of the temperature gradient. Could you clarify this point and refer to Kaempfer et al. 2009.

There are notable differences between Eqs. (45) and (66) as well as Eqs. (49) and (67). These are articulated in our discussion above related to differences between  $k_i$  and  $k_i^{\text{eff}}$  as well as  $k_{ha}$  and  $k_{ha}^{\text{eff}}$ . The important equations in the development of  $k_s$  in Section 5.1 are Eqs. (66) and (67). By starting at the *microscale*, albeit with idealized microstructures, we are afforded the opportunity to use the true thermal conductivities of ice ( $k_i$ ) and humid air ( $k_{ha}$ ).

We previously commented on the origins of Eqs. (67) and (71). They are not related to phase change and are valid for humid air as a pure substance in the atmosphere. Again, the energy flux in a mixture involves a conductive flux and a contribution from the interdiffusion of water vapor and air, see: *Transport Phenomena* by Bird et al.(1960).

Please could you (i) clarify the differences between  $k_i^{\text{eff}}$ ,  $k_i$  and  $k_{ha}^{\text{eff}}$ ,  $k_{ha}$  and (ii) precise the numerical values or expressions of  $k_i$ ,  $k_{ha}$ ,  $\gamma^{i\#}$ , and  $u_{sg}$  that has been used to plot the model on the figures 4,5,6, and 7? In my opinion, the model can be compared to other experimental and numerical values only if they have been obtained using the same modeling or hypothesis (or at least comments are required). Equation (77), which include phase change effects, seems relevant to describe the temperature evolution of  $k_s$  computed on 3D images by Calonne et al. 2011. However these numerical values have been obtained without taking into account the phase change in the simulations. Is it thus reasonable to do such comparison (Figure 5)?

We have thoroughly addressed the differences between  $k_i$  and  $k_i^{\text{eff}}$  as well as  $k_{ha}$  and  $k_{ha}^{\text{eff}}$ .

Regarding specific numerical values, we define the thermal conductivities of ice and humid air using interpolation of standard tabulated values ranging from -80°C to 0°C for ice and -100°C to 0°C for humid air. A lower limit of -100°C for air was based solely on the available data, that is, data for air at -80°C was not found. We exercised our code over the entire range of densities from air to solid ice and temperatures ranging from -70°C to 0°C.

As a specific material property example; at -10°C,  $k_i = 2.3 \text{ W m}^{-1} \text{ K}^{-1}$ , identical to Calonne (2014a). At 0°C,  $k_{ha} = 0.0243 \text{ W m}^{-1} \text{ K}^{-1}$ , but, again, interpolated values are used for temperatures ranging from -100°C to 0°C.

We very much appreciate the reviewer's interest in making model comparisons. To that end, we would be pleased to supply our extremely well commented *MATLAB* code that generates all results of the paper. Indeed, we would be happy to make this a part of the **Supplementary Material** at the discretion of the Editor. The code not only provides clear information on all numerical values, but also provides references on where the values were obtained.

Regarding Eqn. (77), the reviewer has hit on a subtle but important point in our development. Eqn. (77) is precisely the thermal conductivity as *predicted* by Foslien's model. The thermal conductivity with diffusion is obtained by substituting Eqs. (77) and (78) into (65). The presence of  $D_{v-a}$  in the thermal conductivity is no different than the presence of thermal conductivity in the expression for  $D_s$ . The results are a consequence of a direct application of heat and mass transfer using first principles on the idealized microstructures—the parameters simply do not separate at the continuum level. In brief, Eqn. (77) is the appropriate equation for making comparisons with the data of Calonne (2011) that are shown in Figure 5 of the present paper.

## 2. Specific comments

Title: “snow” → → “dry snow” to be more precise.

We will make the change suggested by the reviewer here.

Page 1504, line 18 and throughout the paper: “thermal conductivity” → → Should it be “effective thermal conductivity”?

We would prefer to stay with our use of the word thermal conductivity to describe the thermal conductivity of snow. When we introduce the phrase “effective thermal conductivity, for example pg 1529, L9, we are referring to the thermal conductivity that includes the effects of energy transfer due to mass transport. Admittedly, the possibility of confusion may arise from either choice.

Page 1505, line 13: “For instance, faceted crystal growth has been observed at low temperature gradients where rounded grains from sintering have normally been observed (Flin and Brzoska, 2008).” → → Should be replaced by “For instance, slightly faceted crystal growth has been observed at a low temperature gradient ( $3 \text{ K m}^{-1}$ ) where rounded grains from sintering have normally been observed (Flin and Brzoska, 2008).”

We will make the change suggested by the reviewer here. The suggested language provides a more precise description.

Page 1505, line 15: “In contrast, Pinzer and Schneebeli (2009) note that rounded grain forms have been observed in surface layers under temperature gradient conditions.” → → Should be replaced by “In contrast, Pinzer and Schneebeli (2009) note that rounded grain forms have been observed in surface layers subjected to alternating temperature gradients of opposite direction.”

We will make the change suggested by the reviewer here. Again, the suggested language provides a more precise description.

Page 1506, line 7: “However, in the last two decades, the use of X-ray computed tomography has profoundly altered experimental and theoretical research for snow at the microstructural level.” → → You should add reference about the first 3D images of snow.

We are not aware of what specific references should be used here but we will research the matter.

Page 1506, line 20: You should add the work of Calonne et al. 2014a on the effective diffusion coefficient of vapor in snow, computed from a series of 3D images. Also, page 1545, it will be interesting to add a comparison between your results and their values in Figure 6.

The reviewer has raised many thoughtful points and we appreciate the opportunity for the discussion. The discussion that follows on the effective diffusion coefficient for snow is perhaps one of the most interesting of all.

First we wish to draw the reader’s attention to Calonne’s definition of effective Diffusion coefficient given by

$$\mathbf{D}^{\text{eff}} = \frac{1}{|\Omega|} \int_{\Omega_a} D_v (\text{grad } \mathbf{g}_v + \mathbf{I}) d\Omega$$

Following our previous discussion on effective thermal conductivity, one can rewrite the above as

$$\mathbf{D}^{\text{eff}} = \frac{|\Omega_a|}{|\Omega|} \frac{1}{|\Omega_a|} \int_{\Omega_a} D_v (\text{grad } \mathbf{g}_v + \mathbf{I}) d\Omega = \phi_a \frac{1}{|\Omega_a|} \int_{\Omega_a} D_v (\text{grad } \mathbf{g}_v + \mathbf{I}) d\Omega$$

Introducing the notation

$$\mathbf{D}_a^{\text{eff}} = \frac{1}{|\Omega_a|} \int_{\Omega_a} D_v (\text{grad } \mathbf{g}_v + \mathbf{I}) d\Omega$$

leads to

$$\mathbf{D}^{\text{eff}} = \phi_a \mathbf{D}_a^{\text{eff}}$$

Of interest here is the volume fraction of the humid air constituent leading the average diffusion coefficient in the humid air phase.

Now we wish to perform a thought experiment by comparing the results of Calonne to those of Foslien for the idealized microstructures we identified in the paper as the *pore microstructure* and the *lamellae microstructure*. Foslien developed expressions for one-dimensional energy transfer from first principles for each of these microstructures, guided by *Transport Phenomena* (Bird, 1960).

First, consider the *pore microstructure* where the ice phase and the air phase are in parallel to the energy flux. Calonne's numerical results will produce an effective diffusion coefficient of  $D^{\text{eff}} = \phi_{ha} \mathbf{D}_a^{\text{eff}}$ . The presence of the humid air volume fraction is in precise agreement with the present paper (see the first term in Eqn. 78, sans the ice volume fraction which arises from the combined snow model). The interpretation of the humid air volume fraction is quite clear as the ice phase limits the amount of area for humid air mass transport to occur.

Now consider applying Calonne's definition to the *lamellae microstructure* where energy transfer is perpendicular to the layers. Calonne's definition will again produce an effective diffusion coefficient of

$$\mathbf{D}^{\text{eff}} = \phi_a \mathbf{D}_a^{\text{eff}} \tag{3}$$

In words, the average (effective) mass diffusion coefficient in the air phase is scaled by the volume fraction of the humid air. We would not agree with this definition as the ice phase in this microstructure enhances mass diffusion because i) the ice does not block any pathway (parallel to the temperature gradient) for diffusion and ii) the ice actually shortens the pathway needed for the water vapor to travel via "hand-to-hand" mechanisms of Yosida (1955).

Now consider Foslien's analytical model of the *lamellae microstructure*, again developed from first principles:

$$D_a = \left( \frac{k_i D_v}{\phi_i \left( k_a + u_{sg} D_v \frac{dy_v^{\text{sat}}}{d\theta} \right) + \phi_{ha} k_i} \right)$$

Dividing numerator and denominator by  $k_i$  and recognizing the thermal conductivity of ice is



roughly 100 times that of air reveals that, to first order, Foslien predicts an enhanced diffusion coefficient given by

$$D^{\text{eff}} = D_v / \phi_{ha} . \quad (4)$$

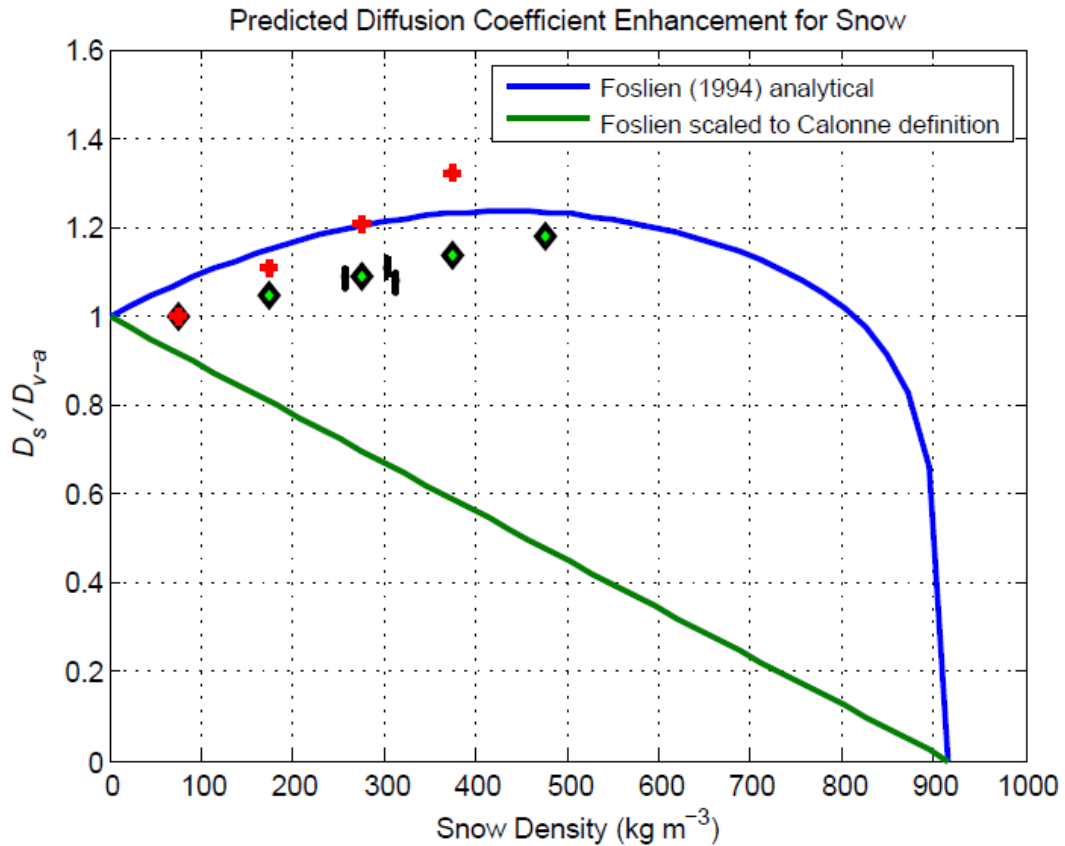
As noted above, we believe this enhanced value is correct in that the ice phase in this microstructure is not blocking diffusion but is instead enhancing diffusion through the “hand-to-hand” notion of Yosida. Indeed, given an ice volume fraction of 50%, we would expect an effective diffusion coefficient for this microstructure to be near double what would be found in air as water vapor would only have to travel half the distance compared to the distance travelled in humid air alone. We note that the volume fraction in the denominator was not artificially introduced and naturally arose in the development.

Comparing Equations (3) and (4) suggests that if one were to multiply Foslien’s effective diffusion coefficient for the *lamellae microstructure* by  $\phi_{ha}^2$ , then the results of Foslien’s model may fall somewhat in line with Calonne’s data shown in Figure 9 (2014a). Of course this assumes the Foslien model of snow described in the paper is somewhat realistic. Certainly, the thermal conductivity predictions of Foslien are supportive of this view.

Figure 1 below shows the results of this exercise. The comparison of the modified Foslien diffusion coefficient with Calonne’s results (Figure 9 of 2014a) is surprisingly close! In any event, we believe our analysis here helps to explain the differences in the computation of  $D^{\text{eff}}$ .

It is also worth noting that none of the data plotted from the numerical predictions in Figure 1 below (Figure 6 of the present paper) use Calonne’s definition of the effective diffusion coefficient. Christon (1994) evaluates an average mass flux traveling through the boundaries of the RVE in a fully-coupled heat and mass transfer analysis. Pinzer (2012) evaluates the mass flux in a slice in the RVE, perpendicular to the temperature gradient—an approach very similar to Christon. We believe these approaches to computing an effective diffusion coefficient are quite direct, leaving little room for alternate interpretations.

In closing, we stand by the predicted diffusion coefficient presented in the paper. That said, one could readily run the mixture theory analysis with the definition of Calonne or any other past investigators. We look forward to further research on this challenging problem.



**Figure 1.** Foslien’s original model and a second curve where the contribution of the lamella microstructure is modified by  $\phi_{na}^2$ .

Page 1506, line 24: It will be relevant to present the work of Löwe et al. 2013 and Calonne et al. 2014b on the thermal conductivity parameterization based on analytical model.

We will include a discussion of the work of Löwe et al. 2013 and Calonne et al. 2014b as suggested.

Page 1515, line 6: delete a “of”.

We thank the reviewer for bringing this typographical error to our attention.

Page 1519, line 8: The title of Section 3.4 and 3.5 should be the same sentence structure.

We will alter the sentence structure of the headings to be consistent. Again, we thank the reviewer for pointing this out.

Page 1532, line 3: “Their finite element predictions show a diffusion coefficient for snow to be very nearly that of diffusion of water vapor in air, perhaps an enhancement of 5–13 % for snow compared to diffusion of water vapor in air based on the data provided in Fig. 11” →→ This is not in agreement with the conclusion of Pinzer et al. 2012 “Our data provide evidence to support the argument that there is no diffusion enhancement in snow”. Could you clarify this point?

We are confused by the point being made here in that we state on pg 1532, L22-24, “In brief, we are in agreement with the view of Pinzer et al. (2012) in that any enhancement of water vapor diffusion in snow compared to diffusion of water vapor in air is minimal.” Perhaps there is disagreement over the term “minimal?” From an engineering perspective we would agree that there is no enhancement.

We arrived at the enhancement values of 1.05-1.13 based on the finite element predictions shown in Figure 11 of Pinzer (2012). The specific values represent the bounds of the finite element predictions for the three data sets shown in Figure 11. We presented the data in the paper after an email exchange with M. Schneebeli. If our interpretation of this data is incorrect, we can certainly remove the data from Figure 6.

Page 1535, line16: same above comment.

We are confused by this comment here as pg1535, L16 has no connection to the previous comment and is, instead, addressing some results predicted by the mixture theory.

## Reference

Albert, M. R., & McGilvary, W. R. Thermal effects due to air flow and vapor transport in dry snow. *Journal of Glaciology*, 38(129), 273-281 (1992).

Bird, R., Stewart, W., and Lightfoot, E.: *Transport Phenomena*, John Wiley and Sons, New York (1960)

Calonne, N., Flin, F., Morin, S., Lesaffre, B., du Roscoat, S. R., & Geindreau, C. Numerical and experimental investigations of the effective thermal conductivity of snow. *Geophysical Research Letters*, 38(23), (2011).

Calonne, Geindreau, Flin Macroscopic Modeling for Heat and Water Vapor Transfer in Dry Snow by Homogenization, *J Phys Chem B*, 118 13393 (2014a).

Calonne, Geindreau, Flin Study of a temperature gradient metamorphism of snow from 3-D images: time evolution of microstructures, physical properties and their associated anisotropy, *The Cryosphere*, 8, 2255 (2014b).

Christon, M, Burns, P., and Sommerfeld, R.: Quasi-steady temperature gradient metamorphism in idealized dry snow, *Numer. Heat Tr. A-Appl.*, 25, 259-278 (1994)

Dorsey, N.: *Properties of Ordinary Water-Substance*, Hafner, New York (1968)

Hobbs, P.: *Ice Physics*, Clarendon Press, Oxford (1974)

Kaempfer, T. U., & Plapp, M. Phase-field modeling of dry snow metamorphism. *Physical Review E*, 79(3), 031502 (2009).

Löwe, Riche, Schneebeli A general treatment of snow microstructure exemplified by an improved relation for thermal conductivity, *The Cryosphere*, 7, 1473 (2013).

Yosida, Z. *Physical studies of deposited snow: thermal properties I*, Institute of Low Temperature Science (1955)