

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.

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 method (volume averaging method, homogenization based on asymptotic developments...) that capture the interface processes.

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.

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 holds for equations (66) and (67).

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 $\gamma^{sat}(T)$? Is it given by the classical Clausius-Clapeyron relationship?

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

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?

In order to avoid any misunderstanding, I think that k_s^{con+d} should be defined as an apparent effective conductivity. It is important also to note that k_s^{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^{con+d} or k_s , respectively.

In section 5.1, the determination of k_s^{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.

Please could you (i) clarify the difference between k_i^{eff} , k_i , and k_{ha}^{eff} , k_{ha} , and (ii) precise the numerical values or expressions of k_i , k_{ha} , γ^{sat} , 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)?

2. Specific comments

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

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

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 K m^{-1}) where rounded grains from sintering have normally been observed (Flin and Brzoska, 2008).”

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.”

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.

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.

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.

Page 1515, line 6: delete a “of”.

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

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?

Page 1535, line 16: same above comment.

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).

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).

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