Review of the manuscript titled "Multiscale modeling of heat and mass transfer in dry snow: influence of the condensation coefficient and comparison with experiments"

The manuscript reports on the multiscale modeling of the heat and mass transfer in dry snow by using the homogenization technique to derive the macroscopic equations. The heat transfer is ruled by the conduction mechanism in ice and water vapor phases whereas the mass transfer of water vapor is described by the Fick's 2nd law. At the ice/fluid interface, the Hertz-Knudsen relation is used to describe the sublimation/deposition mechanism. For different order of magnitude of the sublimation/deposition rate, different cases (A, B1, B2 and C) are considered giving rise to different macroscopic models characterized by the effective coefficients. The details of the homogenization procedure are given in the supplementary document while the main results of the macroscopic models are summarized in the manuscript.

First, I verified the homogenization procedure in the "long" supplementary document for the cases A, B1, B2 and C. The notations are quite heavy with the superscript $\star$ for each term and it can be simplified. In general, I agree with these results except for the case B2 (see my comments below). For high order of magnitude of the sublimation/deposition rate (or high value of α), this gives the same result obtained from the volume averaging method reported in Moyne et al. (1988) for the heat and mass transfer with condensation/evaporation problem in porous media.

The paper is interesting and well written. The development of the multiscale models is rigorous with a well-posed ε-models from the dimensional analysis. For this reason, I recommend the paper for publication after revision.

Major comments:

1. The authors should explicitly explain the choice of using the Hertz-Knudsen equation for describing the vapor flux at the solid/fluid interface.

   Instead of using an equilibrium condition at the solid/fluid interface as (the curvature effect is neglected)

$$\begin{aligned} k_i \nabla T_i \cdot \mathbf{n} - k_a \nabla T_a \cdot \mathbf{n} &= \frac{L_{sg}}{\rho_i} D_v \nabla \rho_v \cdot \mathbf{n} \\ \rho_v &= \rho_{vs} \qquad \text{at } \Gamma_{fs}, \end{aligned} \tag{1}$$

   the authors introduce Hertz-Knudsen law to take into account the non-equilibrium state for small value of α. It means that an "complementary resistance" is added at the solid/fluid interface. This point should be clearly discussed at the beginning.

   Moreover, maybe it should be better to define the latent heat of sublimation by $L = L_{sg}/\rho_i$ in J/kg.

2. Dimensional analysis: the ratio of the heat conductivities of ice and air is

$$[K] = \frac{k_{ic}}{k_{ac}} = 96 \simeq \mathcal{O}(\varepsilon^{-1}) \tag{2}$$

   However, the authors assume that $[K] = \mathcal{O}(1)$. This point needs to be clarified.

3. I don't agree with the result of the model B2. From Eqs. B2.22 and B2.23 together with the periodicity condition, we have

$$\int_{\Gamma} (\rho_v^{(1)} - \rho_{vs}^{(1)}) dS = 0 \tag{3}$$

where $\rho_v^{(1)}(\mathbf{x}, \mathbf{y}, t)$ is periodic function depending on $\mathbf{x}$ and $\mathbf{y}$. This can not ensure that $\rho_v^{(1)} = \rho_{vs}^{(1)}$ on Γ is a unique solution.

I suggest that the authors should find a solution for $\rho_v^{(1)} - \rho_{vs}^{(1)}$ by linearity as

$$\rho_v^{(1)} - \rho_{vs}^{(1)} = \boldsymbol{\chi} \cdot \boldsymbol{\nabla}_x T^{(0)} \tag{4}$$

combined with a solution for $\rho_{vs}^{(1)} = \gamma \mathbf{r}_a \cdot \boldsymbol{\nabla}_x T^{(0)}$, so that from Eqs. B2.22 and B2.23, we can obtain a consistent closure problem with a coupled term at the solid/fluid interface.

4. In my opinion, the effect of the sublimation/deposition needs to be better discussed in the macroscopic results in the Section 2.6. For example, for the case C, what I understand is that considering the sublimation/deposition at the solid/fluid interface refers to a classical heat conduction problem for ice and air without sublimation/deposition with a modified air conductivity being $k_a + k_{dif}$.

5. I find that the discontinuity between the models B and C is quite surprising. Let consider only a heat conduction problem with a resistance at the solid/fluid interface as reported in Auriault et al. [1]. All the one equation models can be deduced from one to other. The discontinuity appears when passing from two equations models to one equation model. However in this work, the one equations models are not continuous. By revisiting the model B2 (see my comment 3), can we obtain the continuity of the models?

6. Page 17, line 410: it was concluded that if a temperature gradient is applied along $\mathbf{e}_2$, the model A (or B) will not predict any mass variation.

In this direction, $D_{22}^{eff} = 0$ and at the steady state, we have $\rho_v^{(0)} = \rho_{vs}^{(0)}(T^{(0)})$ which varies according to the Clausius Clapeyron's law.

7. Page 21, line 470: for the water vapor boundary conditions at the top and bottom, why the Robin boundary condition is imposed instead of using the zero-flux as applied for the macro-scale simulations?

8. Comparison between DNS and macroscopic simulations: In Figs. 11(a) and (b) for ΔT, we observe clearly that by increasing α, the DNS result tends to the one of model C and for higher value of α ($\alpha > 1$), we may have a good agreement between the DNS and the model C as expected. However, in Fig. 11(f), why the result of the DNS for $\alpha \to 1$ does not tend to the case C for $\dot{\phi}$?

Moreover, as the model C is independent on α, I suggest that to compare with the simulation of the model C, for the mass transfer problem at the pore scale, the equilibrium should be used at the solid/fluid interface $\rho_v = \rho_{vs}$ at Γ_{fs}, instead of using the Robin condition involving the parameter α.

9. It is observed that the model B and C can not predict correctly the behavior of sublimation/deposition in the vicinity of the boundary, in comparing with the DNS. In my opinion, it refers to a boundary layer problem (several works in the literature try to fix this problem encountered in simulation of homogenized models).

10. The terminology "boundary condition" used to describe the solid/fluid interface condition is not correct. Please modify this sentence to "interface condition".

References

[1] J. -L. Auriault, H. I. Ene, Macroscopic modelling of heat transfer in composites with interfacial thermal barrier, *Int. J. Heat Mass Transfer*, **37** (1994).