

# ***Review of A macroscale mixture theory analysis of deposition and sublimation rates during heat and mass transfer in snow by Hansen and Foslien***

## **General comments**

The paper addresses homogenization of a phase-changing snowpack within a multi-constituent mixture theory and volume-fraction based closure for the transport coefficients. The upscaling is simplified to a one-dimensional problem which is solved numerically for an idealized snowpack containing a crust.

The paper is based on a sound methodology, well written and comprehensive. However, the work appears to be largely detached from many uncited contributions on upscaling and mixture theory put forward in the previous decades:

The main topic of the paper, mixture theory of snowpacks, has been addressed by various authors before e.g. [1, 2, 3]. All of them are multi-constituent approaches, some of them include phase changes. The present approach of heat and mass transfer is eventually reduced to 1D heat transfer, by coupling the vapor field tightly to the macroscopic temperature under the assumption of equilibrium. This strategy constitutes the foundation of all major operational snowpack models for a long time, cf. CROCUS [8], SN THERM [7] or SNOWPACK [9]. These models have been used elsewhere to address testcases similar to the examples presented here. More recently, the problem of upscaling pore-scale heat and mass transfer in snow has also been studied within a two-scale expansion [6]. The latter approach confirms the commonly used form for the macroscopic equations (61,62) and touches, among other things, the role of latent heat. The two-scale expansion technique also provides the governing equations for the transport coefficients for arbitrary microstructures. Simplified models for the transport coefficients have been presented by [5] and [4] which can be contrasted to the present, volume fraction based approach (It is explicitly mentioned in the paper, though, that the present conductivity model is considered as a simplified approach, which does not include anisotropy). In addition, the derivation of the present, volume-fraction-based closure for the transport coefficient often refer to the MSc thesis (Foslien 1994) which is provided as a supplement. From my point of view this is not appropriate. If necessary, all relevant parts should be moved to the main manuscript.

I am not fundametally against another contribution of another mixture-theory paper with another set of temperature-profile plots. But the benefit of the present approach should become more obvious. In any case, a major revision would be required to avoid repetition of previous developments and to carve out specialties of the present model for a particular application. There is certainly room for such an “intermediate complexity analysis”. On one hand, operational snowpack models [8, 7, 9] are hardly been used anymore to study idealized situations. On the other hand, more recent advancements [6] have not been incorporated into operational models. Given the testcases, which are already included, it might e.g. be helpful to focus more on the details of heat and vapor transport near crusts. Crusts are discussed elsewhere in literature and constitute a key issue for operational snowpack

models due to near-crust metamorphism and implications for avalanche formation. Recently some high-res temperature measurements were conducted [11], revealing the emergence of a “super-gradient” near the crust as the potential origin of near-crust faceting. From my point of view, the present model could made a valuable contribution by providing careful, numerical evidence that the observed features *cannot* be recovered within the present coupling of heat and mass transfer (This is at least what I expect). This would have a strong implication on snowpack modeling which are in need to predict these things. Indeed, also the opposite outcome would give a good conclusion.

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## General comments

**p1505,l14:** What does “normally” refer to here?

**p1508,l14:** It might be advantagous to explicitly state  $\gamma_m(x) = \gamma_i\chi_i(x) + \gamma_{ha}(1 - \chi_i(x))$  in terms of the indicator function  $\chi_i(x)$  of the ice phase.

**p1510,l12:** Ambiguous. Rather: the heat capacity is heterogeneous at the microscale, but homogeneous in the ice phase.

**Eq 20-22:** Are all equations similarly taken form Hansen (1991)? Maybe add the citation (again) in l.19.

**p1514,l17:** Here it might be helpful to replace the loose statement “are implicitly scaled the volume fraction” by a characterizing equation. I think this is important, also in view of the “effective diffusion” issue (see comment there)

**p1516,l12-14:** Important sentence.

**p1519,l14:** Maybe refer explicitly to  $u_{sg}$  as “latent heat”.

**Sec. 4.1:** It might be helpful to actually evaluate the time scales with characteristic parameter values.

**Sec. 5:** If required, necessary details of the Foslien 94 model should be generally included in the paper. But the heat conductivity problem of laminate microstructures is a textbook example, where the effective properties are characterized by the arithmetic and harmonic average of the phase conductivities [10]. So it might be appropriate to directly state Eq.(74) with a such a citation, and noting that one of the phase conductivities is a modified one, which also includes the vapor term (according to eq 65).

**Eq. 75:** Likewise, the conductivity model could be shortened:  $k_{pore}$  and  $k_{lam}$  are the well known effective conductivites of a laminate (with volume fractions  $\phi_i$  and  $\phi_a$

and phase conductivities  $k_i$  and  $k_{ha} + u_{sg}D_{va}d\gamma^{sat}/d\theta$ ) parallel and perpendicular to the laminate orientation, respectively. The final model for the effective conductivity of snow (eq. 75) is then a weighted average (with *ad hoc* weights) of both orientations. Or is the explanation given in l.12-19 (p1529) meant to be a justification for the coefficients  $\phi_i, \phi_{ha}$  in Eq (75)? (It's clear that the lineal fraction is equal to the volume fraction, but i can't relate this fact to eq 75).

**p1530,l10:** Where does eq 78 come from?

**p.1531,1532:** Regarding the discussion of effective diffusion: I think some confusion in literature about the effective diffusion in snow arised because the “effective diffusion coefficient” does not always refer exactly to the same thing. Sometimes it is defined as the diffusion coefficient in the phase averaged (dispersed) density equation, sometimes as the coefficient in the volume averaged equation (differing by a factor of volume fraction, that why the aforementioned “implicit scaling with volume fraction” should be made explicit) And sometimes its meant to be a relation between the macroscopically applied vapor gradient and the resulting macroscopic flux (which would then also be affected by the source term in the diffusion equation). Here the present work should contribute to further clarify these things during the discussion here. This is also required to discuss Fig. 6. and the sentence (p1516,l12-14) in view of the statements in [6].

**p1534,l8-12:** I don't understand the comment about the boundary condition here. I think a subtle point about the model (eq 63-65) is behavior of the vapor phase at the boundary. By saying  $\gamma_v(x) = \gamma^{sat}(\theta(x))$ , the mass supply is automatically prescribed by the temperature gradient at the boundary, isn't it?

**fig. 6:** What does “no branch”, “vertical branch” mean?

**fig. 10/11/12:** Why not including 1,5,10 days curves in *all* of the plots.

## References

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