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

General comments

The paper has significantly improved upon revision and my previous comments were taken largely into account. I greatly appreciate the inclusion of the discussion of the diffusion enhancement which adds to the general awareness of that subtle issue.

Regarding my provocative comment "yet another mixture theory paper with another set....", this was indeed not meant to be disrespectful, and hopefully not perceived in such a way. It just reflects my opinion that people will have difficulties in assessing the main achievements of the present work and sorting it into the context. This is confirmed also by discussions I had with co-workers from operational snowpack modeling about the paper. Along these lines it is worth to point out that the author's rebuttal comment "operational models have been replaced by the impressive direct numerical modeling" does by no means reflect present activities in the community. I think there would have been an opportunity to increase the impact of the paper if differences or similarities to operational models were addressed. Anyway, this is however not crucial for the acceptance of the paper. However two points should be revised before accepting the paper for publication in TC.

- **Remarks added to the summary are misleading.** The present theory is manifestly driven exclusively by temperature (and temperature gradients) alone since deposition and sublimation (eq 72) at any time can be fully computed from the temperature boundary conditions, as exemplified in (Sec.6.2) for time dependent Dirichlet conditions. Hence the paragraphs (p41, l24–p42,l8) vs (p41, l24–p42,l8) constrasting these aspects is in my opinion misleading/wrong. The difference rather lies in the latent heat. From my point of view, the mentioned comparison between Fig 15 and Fig 16 rather suggests a paragraph like this (or similar)

  > *The present model accounts for latent heat and thereby includes a feedback of the (vapor) mass conservation equation on the energy equation. If vapor conservation is simplified under the assumption of local equilibrium (saturation), this feedback manifests itself by a vapor transport term in the thermal conductivity (eq 74) and thereby in the temperature θ (eq 73) which in turn affects the sublimation and deposition rates via eq 72. As a consequence, the "simple" temperature profiles near the surface (Fig. 15) may lead to rather complex sublimation and deposition profiles in the same region (fig 16). This is caused by two-way coupling of heat and mass, for which deposition and sublimation cannot be directly inferred from temperature alone.*

  The discussion of this point in relation to Fig 15,16 would also relate to Calonne 2014a, last sentence 13401: *"This error is enhanced for the large temperature gradients. In* *conclusion, the effect of phases changes at the macro scale should be taken into account via QT and Qv for a better precision, and especially when snow experiences large temperature gradients"* which would add to the impact of latent heat discussed in p38,l5-10 (low density snow). Its the combination of temperature (gradients) and density which renders latent heat (and the feedback of vapor transport and recrystallization on the heat equation) important or unimportant. Text passages which are also a bit ambiguous in this respect and affected by this issue:

- (p42,l18) its not a *decoupling* it is in fact a more complex coupling/feedback in the sense given above.
- p2,l13-15
- p3,l7-13

In fact, I would be very interested in a reference simulation (equivalent to Fig 16) for which latent heat is simply set to zero, i.e. $u_{sg} = 0$ (if not included in the paper I would be really glad to maybe receive such a comparison via private communication)

- **Diffusion enhancement needs some clarification** As said before, I appreciate that the issue has been included here (p33,l11-p37,l22) since it has not been addressed before in relation to a homogenization method. Unfortunately the main messages are not clear to me from the paragraph.
  - What is the connection of D_s (eq 88) to D_{eff} from Calonne (eq 89)? The paragraph (p35,l22) sounds to me as if D_{eff} from Calonne is giving the same value for both model microstructures (*lamellae* and *pore*) while D_s from eq 88 is giving different things. The latter (reflected by the statements in p35,l15 and p36,l11-94) is clear. But is it also clear that D_{eff} from Calonne is not able to distinguish both microstructures and thereby giving the same dependence on pore volume fraction?
  - I dont understand (p37,l3-8) as a possibility to relate both models. What are the quantities which are suppposed to be compared?
  - What is considered to be the modified Foslien model? The green line in Fig 7 is just the function $f(\rho) = (\gamma_i - \rho)/\gamma_i$ (in the notation of the paper). Where does it come from?

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Specific comments

p10,l17: "the present work does not involve momentum balance" in contrast to eq 22 where the momentum balance is explicitly written down, in addition to the Section (3.3) entitled "Momentum balance" noting that it is not further considered. Why not just abandon it completely?

references: Lowe, H.R. $\to$ Löwe, H.; Richei $\to$ Riche; Morlandr $\to$ Morland ($2\times$) and some other typos...