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

Still to be revised (line numbers refer to the texdiff-document)

p8,l12: (and below) $\theta_m \rightarrow \theta_{\mathrm{m}}$

p25,Eq.(67,68): $\gamma \rightarrow \gamma_{\mathrm{v}}$

p24: (and throughout) $\gamma^{\mathrm{sat}} \rightarrow \gamma_{\mathrm{v}}^{\mathrm{sat}}$

p35,l3: "normalized a values" $\rightarrow$ "normalized values"

p36,l17: "coeffficient" $\rightarrow$ "coefficient"

p31, l19,20: (As discussed offline) State that the transport coefficients are *postulated* and not derived here as given by Eq. (87,88). This has some implications (see comments below)

p35-37, Fig7: Presently the prediction of Eq. (88) is in striking difference to Fig 9 in (Calonne 2014) where the effective medium result $D_{\mathrm{s}}^{\mathrm{eff}} = D_{\mathrm{v-a}}(3\phi_{\mathrm{ha}} - 1)/2$ captures the simulations reasonably well throughout the entire density range. Presently it is argued that the effective diffusion coefficient from the present work must be rescaled by ϕ_{ha}^2 to explain this difference. It is not comprehensive from the manuscript *why* the postulated model in Eq. (88) requires rescaling to match Calonne. By the same token, it is not comprehensive why on p.35-37 the comparison to Calonne is elaborated in great detail, while in "none of the individual numerical predictions shown in [..] Fig.7 use the definition [..] in Calonne" (p38,l8-10)

Given that Eq. (88) must be postulated and cannot be deduced from Eq. (84) alone, there is another explanation which should be considered on p.37: The offline discussion has revealed that $D_{\mathrm{s}}^{\mathrm{eff}}$ and $k_{\mathrm{s}}^{\mathrm{eff}}$ are not uniquely determined, since any additive contribution could be assigned either to $D_{\mathrm{s}}^{\mathrm{eff}}$ or $k_{\mathrm{s}}^{\mathrm{eff}}$ (by comparing (84) and (86)). Having such a contribution assigned to the wrong side (in writing down (87)/(88)), it is still possible (because of orders of magnitude of the prefactors, etc...) that the effective conductivity is in good agreement with some samples (Fig.5) while the density dependence of the effective diffusion constant is in disagreement (Eq (88) vs Calonne, Fig 7). This possibility should be mentioned. If the authors believe that Calonne is wrong (as clearly indicated in the rebuttal letter by saying "To be clear, we do not believe...." and below on p7 in the rebuttal) then this should be stated more explicitly in the paper. But I believe this viewpoint is difficult to defend, given the ambiguity in the transport coefficients. Presently the conclusions drawn from p35-38 are not clear at all.

p39, l14: (Related to the previous comment) The "excellent agreement" for D_s^{eff} seem overstated, given the striking differences between Fig. 7 and Fig9 in (Calonne 2014)

p43,p44: Also some formulations in the summary appear overstated, given that transport coefficients have to be postulated (e.g. p.44,l9,10)

Kind regards, Henning Löwe