



Interactive comment on “Impact of water vapor diffusion and latent heat on the effective thermal conductivity of snow” by Kévin Fourteau et al.

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Following a recent publication by the same authors showing that the macroscopic water vapor diffusion is not enhanced in snow (Fourteau et al., 2021), this paper presents a thorough, self-consistent and well written study of the impacts of the kinetics of the sublimation and deposition of water vapor onto ice in snow. It describes the inextricable coupling of heat conduction and water vapor transport in snow considering two limiting cases, slow and fast kinetics. After a detailed but nevertheless concise theoretical part, the authors present a numerical exercise on quite a few measured snow microstructures. This all leads to new parameterizations of thermal conductivity as a function of density at fixed temperatures and to new insights into the influence of water

C1

vapor transport on the effective thermal conductivity of low density snow and near the melting point of ice.

Nevertheless, let me comment on two issues I had while reading the paper. First, the authors state in the abstract (lines 9-11), “Comparison of our numerical simulations with literature data suggests that the fast kinetics hypothesis could be a reasonable assumption to model snow physical properties.”, which is re-iterated in the discussion at lines 385-386 and in the conclusions at lines 428-429. On the other hand, on lines 386-388, the authors state, “That being said, further work is required before robustly assessing whether mass and heat transport in snow should be treated using the slow kinetics hypothesis, the fast kinetics hypothesis, or an intermediate case.”. The author should clarify that inconsistency, stating what they really mean, and adapting abstract and conclusions accordingly. Indeed, I cannot imagine that the slow kinetics – and intermediate cases – are not applicable to snow. At the same time, not all physical and mechanical properties of snow will depend on these findings, do they? Second, while I welcome “These new data and parametrizations [that] are primarily meant to be used by detailed snow physics model.” (see Eq. 17 on line 340), I am not sure how I would do this easily as each parameterization is given for a fixed temperature only. Could the authors comment on this and make a corresponding note in the text?

I therefore recommend accepting the paper after the authors addressed this issue and consider doing the minor editorial revisions as suggested in the uploaded PDF-file.

Finally I apologize to the authors for my belated review, mostly due to other urgent commitments.

Fourteau, K., Domine, F., and Hagenmuller, P.: Macroscopic water vapor diffusion is not enhanced in snow, 15, 389–406, <https://doi.org/10.5194/tc-15-389-2021>, 2021.

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Please also note the supplement to this comment:

C2

<https://tc.copernicus.org/preprints/tc-2020-317/tc-2020-317-RC3-supplement.pdf>

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2020-317>, 2020.