Review of EGU-2025-3035: “Simulating the effect of natural convection in a tundra snow cover”

This paper uses simulations to explore the impact of convection in snow at Bylot Island, in the Canadian high-Arctic. The question is to know the influence of convection cells on vapor fluxes/phase changes and on the density profiles of snowpacks, and whether it can explain the bi-layer structure (with a light basal layer) observed at Bylot (which is also observed more or less throughout the entire continental Arctic). The main conclusions from what I understand are that (i) in a snowpack without a top slab, convection cells can explain a light de-densification in the first 20 cm with a light densification above, which is qualitatively in line with the expected bi-layer structure, but (ii) that the presence of a top slab blocks the convection cells to the base, modifying the pattern of sublimation/deposition which gets weaker (if I understood) and is no longer in line with the bi-layer structure.

The topic is important and the paper makes a valuable contribution with adequate tools. Applying the convection models developed in the last few years by the authors in an attempt to simulate the actual snowpack at Bylot is a great idea to investigate how convection might play in a realistic setting. My main question concerns the potential limitations of the conclusions and their adequacy to the actual snowpack at Bylot. In short, what are the impacts of the physical assumptions (boundary conditions, absence of wind pumping, or the chosen water vapor physics) on the results (the light de-dendification in the bottom half when the slab is absent, and the suppression of convection when a slab is present)? Also, since convection cells are suppressed in the presence of a hard slab, should we conclude that convection is irrelevant for the Bylot snowpack, where most of the snow season presents a slab on top?

The paper is suited for The Cryosphere and would make a great contribution to our understanding of macroscopic water vapor fluxes in snowpacks. But as mentioned above, I think the paper should further analyze its hypotheses and relation to actual tundra snowpacks (i.e. snowpacks with a hard slab), and would thus benefit from major modifications. Essentially, as discussed in the General Comments below, I would encourage the authors to focus more on a realistic case with a wind slab (even if the parametrization is far from perfect), to investigate or discuss the role of wind pumping, and to consider a broader range of water vapor condensation coefficient if possible.

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General Comments

Impact of top slab: The notion that the top hard slab in tundra snowpacks blocks the formation of convection cells spanning the whole snowpack is very interesting and is the major information/result of the article for me. I think the paper should insist more on this poin as it is crucial for tundra snowpacks. Unfortunately, it was not clear to me until L317 that it was actually observed in dedicated SNOWPACK-OpenFOAM simulations and not simply inferred from the low permeability of the slab. I also think that showing and discussing the

convection cells and deposition/sublimation field in an already bi-layer snowpack would be quite beneficial to have a broader idea of how convection in the Arctic (where the bi-layer structure develops early in the season) might actually look like. While studying the case without a top slab is interesting to quantify the upper hand of the impact of convection (but also for the early season before the formation of the slab or for subarctic snowpacks without slabs), it is a bit strange to spend so little time analyzing a somewhat realistic Bylot stratigraphy when the title specifically mentions the tundra snow cover.
Also I'm not sure that we need the same level of precision between the wind compaction and convection formulation to reach meaningful conclusions (I might be over-interpreting what is written in the paper L48-50 and L276-277 here). For sure, interpreting density measurements in the top slab in terms of vapor deposition would require to disentangle the effect of wind-compaction, which requires an accurate description of the wind-compaction. I also agree that in general, the overall accuracy of a complete snowpack model is limited by its weakest parts. But I still think that a robust conclusion on the impact of top slabs on convection cells can still be reached even though the representation of wind slabs in snow models remains simplistic.

Wind Pumping: Wind pumping has been proposed as a mechanism for forced convection/advection in snowpacks (for instance Sturm and Johnson, 1991, report convection events that sometimes match with periods of high-wind, albeit for a subarctic snowpack), but is not discussed in the manuscript. Could it be that while natural convection is suppressed in Arctic snowpacks due to the low permeability of hard slabs, wind pumping events remain important? Could wind pumping be integrated in the SNOWPACK-OpenFOAM set-up (following Jafari et al., 2022) and its role quantitatively analyzed for the Bylot site? If not, a discussion on the potential role of wind pumping in tundra snowpacks would be nice I think.

Water vapor thermodynamics: The equations used to model the snowpack in OpenFOAM are based on chemical non-equilibrium. This is embedded in the reaction constant h_m that is small enough to allow significant local chemical disequilibrium between the phases. If I'm not wrong, in terms of vapor deposition kinetics, this is equivalent to a condensation coefficient of about 10^{-6} (for h_m=8.7 10^{-5} m s^{-1}), when some papers put it above 10^{-3}, more in the diffusion-limited range and where chemical equilibrium is usually expected (e.g. Kaempfer and Plapp., 2009, Hansen et al., 2015 or Braun et al., 2024). How would faster vapor kinetics influence the simulations and how does the uncertainty on the kinetics coefficients impact the conclusion drawn from the simulation in this paper?

Specific Comments

L54 I think this discussion on why the top slab is neglected and how it would impact the simulation should not be done in the introduction. I would rather see it in the Material and Methods and/or the Results and Discussion.

L75 to 86 I do not understand the coupling strategy between SNOWPACK and OpenFOAM described in this paragraph. I think I understand the overall idea behind Fig. 1: starting from consistent SNOWPACK/OpenFOAM states, SNOWPACK is run first to compute some processes, then OpenFOAM is updated with the output of SNOWPACK by modifying its mesh and its variables (this ensures that the two simulation domains are consistent), OpenFOAM solves for the vapor deposition/sublimation and finally sends the results back to SNOWPACK for a final update. But I'm a bit lost on what is actually done and would not be able to reproduce the coupling strategy from the text alone.

I think Figure 1 could be more detailed to help the reader. For instance:
- What are the conditions at the start of integration? I guess initial conditions for both SNOWPACK and OpenFOAM and which are consistent (for instance average density at a given horizon in OpenFOAM matches that of SNOWPACK).
- What is solved in SNOWPACK?
- How is the synchronization/interpolation from SNOWPACK to OpenFOAM done? What info are concretely transferred and how are they applied to OpenFOAM? Is the temperature field computed by SNOWPACK simply used as boundary conditions for OpenFOAM? How do we ensure full consistency between the two simulation domains?

L87 to 98 I'm not sure to understand what is meant here. Notably I think I'm missing the point of these computations, and why applying mass changes/settling from one model to the other is so intricate.

L92 Does SNOWPACK compute vapor diffusion even in its coupled version with OpenFOAM? If so, why compute this process in SNOWPACK when the OpenFOAM version is expected to be richer?

L107 In the idea of a future development of SNOWPACK-OpenFOAM, is it possible that replacing the SNOWPACK temperature field by the averaged temperature profile from OpenFOAM breaks energy conservation deduced by SNOWPACK? Why not simply skip the resolution of energy conservation in SNOWPACK, if it is to be done by OpenFOAM in the end?

L176 What are the typical and maximum differences observed between the full transient and flow freezing methods?

L205 Water vapor convection has been reported at a subarctic site as continuous for years with thin snowpacks but as sporadic for a year with a thicker snowpack (Sturm and Johnson, 1991). This is well in line with the results of this study and might be worth mentioning.

L262 and Fig 10 What is the definition of the snow temperature T_m? Does it correspond to the temperature that a thermometer will read? If so, large lateral temperature differences are consistent with the observations of Sturm and Johnson (1991), and might be worth mentioning.

L266 and Fig 12 It might also be worth showing the results of Fig 12 earlier in the Material and Methods, when the flow freezing technique is introduced. It is a clear demonstration that the freeze flowing technique has "acceptably small differences" for the winter 2017-2018 compared to the full integration method.

Technical Comments

L48 and L309 I found the formulation “accuracy of less than 10%” strange. Intuitively for me, 100% accuracy means the model is perfect and 0% means it is really bad. Perhaps say that the model “shows an error level of less than 10%”.

L91 It is not clear to me what “absolute” means.

L230 The definition of “cumulative density change” could be provided earlier.

References

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