

Responses to Reviewer 1 for EGU-2025-3035:
“Simulating the effect of natural convection in a
tundra snow cover”

October, 2025

1 Answers to general comments, specific comments, and technical comments

Corresponding changes based on the comments from reviewer 1 and 2 and the additional changes in the revised manuscript are highlighted in blue color.

1.0.1 General Comments

1. 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.

Answer:

We thank the reviewer for thoughtful comments and efforts towards improving our manuscript and the expertise provided. We have revised the manuscript to explicitly discuss two scenarios, establishing the upper bound (no drifting-snow compaction) and lower bound (with drifting-snow

compaction) of convection’s influence. Our coupled solver, SNOWPACK-Foam (SNOWPACK-OpenFOAM coupling), is now capable of modeling convection even when the drifting-snow compaction model is active in SNOWPACK.

The key is in the interpretation: The original observation — that a hard slab blocks convection — is addressed by our lower bound of convection with drifting-snow compaction model scenario. When wind-driven drifting-snow compaction is active, it creates the high-density top slab layer. This high-density, low-permeability layer effectively blocks the large-scale convection cells from spanning the entire snowpack. Consequently, water vapor transport at the base is dominated by diffusion alone, and the snowpack develops a high-density top due to wind but lacks the strong de-densification and weak layer at the base that would be caused by convection. Therefore, the complete bi-layer structure (high-density top and low-density base) is not formed in this realistic scenario. The scenario without a hard slab is now presented as the upper bound of convection without drifting-snow compaction model, which is an idealized case showing the maximum possible de-densification due to convection.

2. 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-densification 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.

Answer:

We thank the reviewer for their comments on the potential limitations of the study. The impacts of physical assumptions are addressed later point-by-point in response to the General Comments, and we have also added further explanation regarding the boundary conditions used for both SNOWPACK and OpenFOAM.

The reviewer is correct that the intentional exclusion of wind compaction in the original manuscript’s main simulation was a deliberate choice to quantify the upper bound of convection without drifting-snow compaction model. This choice isolates and analyzes the effects of convection under ideal conditions, namely persistent low surface temperatures and moderate snow depths that drive strong vertical temperature gradients.

Crucially, in the revised manuscript, as suggested by the reviewer 1, we have now performed a set of simulations using the SNOWPACK-OpenFOAM coupled solver as SNOWPACKFoam that models convection when the drifting-snow compaction is active in SNOWPACK. This new lower bound of convection with drifting-snow compaction model scenario directly addresses the need to investigate convection in a more realistic tundra snowpack that develops a hard slab.

We realized the conclusion that the convection is irrelevant is indeed tricky and not entirely valid. The upper bound scenario (without a hard slab) is highly relevant for the early season or during long rest periods between wind events, when the snow density is low enough for convection to be triggered. We complement the reviewer’s thinking: indeed, the convection can trigger as long as it finds a low-enough snow density layer with a high-enough temperature gradient, and as long as the atmospheric forcing is not too windy to form the convection cells. Convection then acts to reduce the snow density (more than it increases the snow density), putting a considerable footprint—with large lateral variations—on the snow density, air temperature, and the cumulative snow density change due to vapor transport. We believe that modeling the upper-bound effect is essential to fully understand the competing processes and the snowpack’s evolution.

3. 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 point 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.

Answer:

We appreciate the reviewer's insight that this finding is crucial for tundra snowpacks, and the comment encouraged us to update our SNOWPACK model to the most recent version (<https://github.com/snowpack-model/snowpack>), which includes a drifting-snow compaction model introduced by Keenan et al. (2021); Wever et al. (2023). We have performed a dedicated set of simulations in which the SNOWPACKFoam coupled solver can model the convection when the drifting-snow compaction is active in SNOWPACK (the lower bound scenario), allowing us to directly address the realism issue. We have accordingly revised the Results and Discussion section to insist more on the comparison between our two scenarios.

4. 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.

Answer:

We thank the reviewer for raising the important question about the potential role of wind pumping (forced convection/advection), particularly in snowpacks where natural convection is suppressed by a hard slab.

The SNOWPACK-OpenFOAM coupling as SNOWPACKFoam solver can impose a ventilation flow velocity due to wind pumping as a top boundary condition, provided that this velocity is available in the forcing data. However, as reviewed in Jafari et al. (2022), there is high uncertainty and a wide range of potential impact reported in the literature for the air flux caused by wind pumping, making it difficult to parameterize accurately without site-specific measurements.

The analysis in Jafari et al. (2022) showed that for idealized snowpacks with initial uniform low density (e.g., 150 kg m^{-3}), the natural convection cells are intrinsically strong. Even when tested with a large literature-reported ventilation velocity (0.05 cm s^{-1}), wind pumping could not disturb the formed convection cells because the wind-induced flow was approximately one order of magnitude smaller than the natural convective velocity scale (U_{conv}). This is the key finding: wind pumping is negligible when natural convection is strongest. Therefore, in the realistic tundra snowpack (our Lower bound scenario) where the hard, low-permeability slab not only suppresses the stronger natural convection but also physically restricts air movement, wind pumping is highly unlikely to penetrate deeply and have a significant impact on water vapor transport. Based on this prior result, we safely ignore wind pumping in our current simulations.

We confirm that the justification for ignoring wind pumping, based on the quantitative analysis of Jafari et al. (2022), has been incorporated into the revised manuscript. This discussion has been added as the last item as assumptions and limitations considered for SNOWPACK-OpenFOAM coupling in "Materials and Methods" and also included in the "Conclusion" section.

5. 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 hm 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 $hm=8.7 \times 10^{-5} \text{ m s}^{-1}$), 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 simu-

lations and how does the uncertainty on the kinetics coefficients impact the conclusion drawn from the simulation in this paper?

Answer:

Yes, we agree with the reviewer that the equations used to model the snow-pack in OpenFOAM are based on chemical and thermal non-equilibrium over a Representative Elementary Volume (REV). Our parameters are effective values defined at the macro-scale, not the pore scale. The fast and slow kinetic vapor limits mentioned are typically pore-scale concepts. In the macro-scale approach, we use effective thermal conductivity, effective water vapor diffusivity, and, critically, bulk heat and mass transfer coefficients (h_c and h_m). The values reported in this manuscript and our past studies are for a bulk macro-scale and are significantly reduced compared to the theoretical value for a single sphere.

In this paper and our previous studies, we used volume-averaged equations of the mass continuity and heat transfer for each phase based on primitive macroscopic quantities. Therefore, for the closure, we need to use the bulk (REV scale not local scale) heat and mass transfer coefficients as defined in this study as h_c and h_m respectively. We explained that the total mass transfer coefficient for a pack of grains (snow element) is smaller than the one we obtain for a single ice sphere. Irregardless of what we assume as the vapor concentration on the ice/pore interface (for example infinitely fast kinetic as assumed by Fourteau et al. (2021) to achieve saturation condition on ice/pore interface), for a single sphere with a maintained vapor concentration on its interface inside a large air domain (valid also for dilute system) with a different vapor concentration, the steady-state theoretical solution shows that $Sh = 2$. However, for a bed of small particles (snow), the mass transfer coefficient (total) will be much smaller as explained in detail in previous studies (Jafari et al., 2020, 2022; Jafari, 2022; Jafari and Lehning, 2023).

As we referred to Crowe (2005), this difference in heat and mass transfer coefficients between a single grain and a pack of particles is clearly shown in the FIGURE 5.26 by Crowe (2005) (also shown here in Figure 1). The only parametrization could correctly represent this limited total heat and mass transfer coefficient is from the experiment by Ebner et al. (2015). This is clearly shown for the temporal variation of local Nu by comparing Figure 2, shown here for a larger value of theoretical Nu).

If the mentioned references by reviewer (e.g. Kaempfer and Plapp., 2009, Hansen et al., 2015 or Braun et al., 2024) indicate that a higher value for the interface kinetic growth coefficient (β) should be used, this is in accordance with our argument about limited bulk mass transfer coefficient

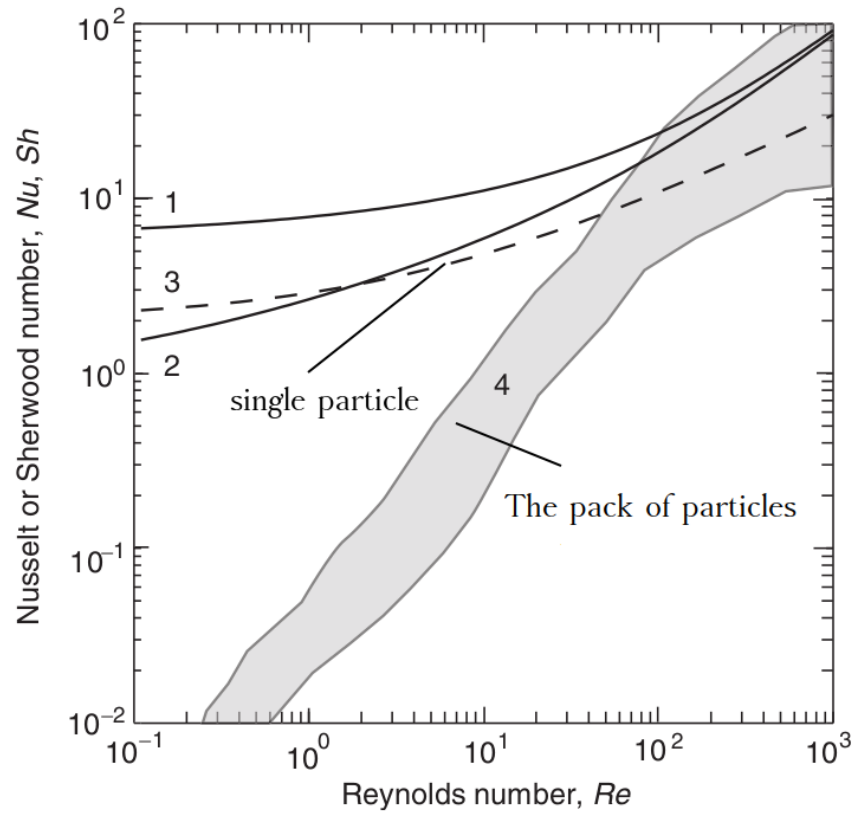


Figure 1: This figure is from Crowe (2005) to show a clear discrepancy for heat and mass transfer coefficients between pack of particles shown in region 4 and a single sphere shown in dashed line 3.

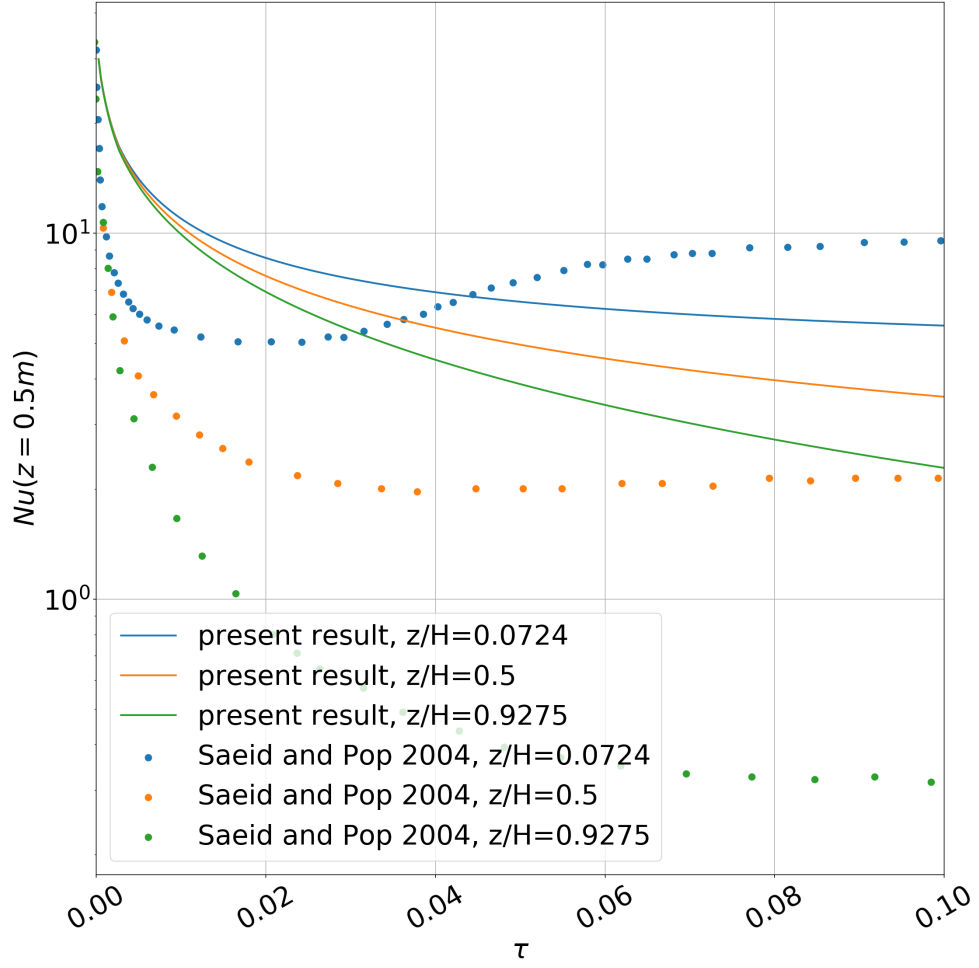


Figure 2: The temporal variation of the local Nusselt number using a large value of theoretical heat transfer coefficient for a single sphere.

as $h_m = \rho_i/(\beta\rho_{v,s})$. This is the case as Calonne et al. (2014) states that a larger value around $\beta = 10^9 \text{ s m}^{-1}$ must be used to be matched experimentally Neumann et al. (2009). This value is similar to one measured by Ebner et al. (2015) as $\beta = 9.7 \times 10^9 \text{ s m}^{-1}$ but almost one order magnitude smaller.

However, we are convinced that you referred to α (linked to the inverse of β) used for the local diffusive flux at interface as $J_v = \alpha v_{kin}(\rho_v - \rho_{vs})$. In Krol and Löwe (2018), α is called the kinetic coefficient while in Kaempfer and Plapp (2009) as the condensation coefficient and in Fourteau et al. (2021) as the sticking coefficient. However, Kaempfer and Plapp (2009) used the kinetic coefficient for β and our previous studies and Calonne et al. (2014) both also used a similar name as interface kinetic growth coefficient for β . As we mentioned before, this apparent discrepancy between bulk mass transfer coefficient and the one for a single sphere has been clearly referred to in the text books in Kunii and Levenspiel (1991); Crowe (2005).

However, in studies for the pore scale vapor transport between snow grains (Kaempfer and Plapp, 2009; Krol and Löwe, 2018; Fourteau et al., 2021) that use or assume higher values for local mass transfer coefficient on ice surface (different from bulk value), as far as we know, they never numerically calculated and analyzed the bulk mass transfer coefficient to explain its large difference from the theoretical value for a single sphere. Using or indicating a higher value for local mass transfer coefficient on the ice surface does not mean that the bulk mass transfer coefficient for the whole pack of snow grains is not limited compared to the values for a single sphere. As interestingly analyzed by Fourteau et al. (2021), for the infinitely fast surface kinetics assumption (very large value for local mass transfer coefficient on ice surface), it can be seen from their figure 3 for vapor flux that sublimated vapor will be all or partly redeposited later on neighboring ice grain. This has been exactly mentioned by Fourteau et al. (2021) that the vapor flux does not need to go around the ice grain and is rather moving from ice grain to ice grain, in agreement with the suggestion of Yosida et al. (1955) and the numerical simulations of Pinzer et al. (2012). This results in the fact that the net mass transfer between ice and air for all grains is limited compared to the theoretical value for a single sphere that has one-way mass transfer (a maintained uniform vapor concentration on its interface inside a large air domain with a different vapor concentration) and this is in agreement with our argument that the entire specific surface area may be not active for mass transfer inducing much lower estimations of the bulk mass transfer coefficient. However, this needs to be confirmed by pore scale simulations to check how much grain-to-grain mass transfer reduces the bulk mass transfer coefficient.

It is not that we rely on "two values/papers", but as Kaempfer and Plapp (2009) says that "no detailed and accepted experimental or theoretical

estimates of β are available" and as far as we know, we rely on the two only available experiments (Neumann et al., 2009; Ebner et al., 2015) representing limited mass transfer coefficient. And, it is the formulation by Ebner et al. (2015) that leads to the best match against numerical benchmark for temporal thermal variations as discussed in Jafari et al. (2022); Jafari (2022) for model verification.

Note that we used higher values for the mass transfer coefficient (as for a single sphere) without any numerical issues. The Implicit/Explicit treatment for the source term has been explained in the OpenFOAM programmer's guide as: "The implicit source term changes the coefficient of the diagonal of the matrix. Depending on the sign of the coefficient and matrix terms, this will either increase or decrease diagonal dominance of the matrix. Decreasing the diagonal dominance could cause instability during iterative solution of the matrix equation. Therefore OpenFOAM provides a mixed source discretisation procedure that is implicit when the coefficients that are greater than zero, and explicit for the coefficients less than zero". For mass source/sink term in mass continuity of water vapor as $h_m a_s (\rho_{vs} - \rho_v)$, we used the term $-h_m a_s \rho_v$ as implicit that goes on the left hand side and increases the diagonal dominance while the term $h_m a_s \rho_{vs}$ is treated explicitly. Even using a very large value as $h_m = 1$ (five orders of magnitude larger than the values used in thesis) is numerically manageable. Again, note that higher transfer coefficients cannot correctly capture the thermal variations (as discussed here for Figure 2) and the formulation by Ebner et al. (2015) has been verified against numerical benchmark in Jafari et al. (2022).

1.0.2 Specific Comments

1. 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.

Answer:

We agree with the reviewer that the discussion regarding the initial assumption of neglecting the top hard slab was misplaced in the Introduction. We have removed the main part of this discussion from the Introduction (starting around line L54 in the original manuscript). Since our revised manuscript now includes a set of simulations with the drifting-snow compaction model active (our lower bound scenario), we no longer neglect the top slab's formation. The revised manuscript presents two scenarios (with and without the slab) and the discussion of their physical impact, comparison, and limitations are now correctly placed in the Materials

and Methods and Results and Discussion sections. We have the newly added/revised lines in blue color in the revised manuscript.

2. 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.

Answer:

We appreciate the reviewer pointing out the lack of clarity regarding the SNOWPACK-OpenFOAM coupling strategy. We have generated a thoroughly revised explanation below, which we will use to update the manuscript text and Figure 1 to ensure the coupling strategy is fully transparent and reproducible.

We clarify the questions raised above:

Initial Conditions and Consistency: The simulations start from a fully consistent state. The overall simulation period runs from the 1st-5th of September to the 1st of July for all six years. SNOWPACK is initialized using a .sno file that defines the initial conditions for the soil (3 meters, 32 elements) with an initial temperature of 274.87 K and specified volumetric fractions for air of 12.5 %, water of 25 %, ice of 0 %, and soil of 62.5 %

and no pre-existing snow cover.

What is solved in SNOWPACK? SNOWPACK primarily acts as the 1D thermo-mechanical driver. In a single SNOWPACK time step, it solves for processes like settlement, liquid water transport, compaction (including drifting-snow compaction), and the long-term temperature evolution in both the soil and the snowpack, taking into account all important processes such as settling, metamorphism, and melting-refreezing (Lehning et al., 1999; Bartelt and Lehning, 2002; Lehning et al., 2002b,a).

Synchronization, Interpolation, and Boundary Conditions: OpenFOAM only models the snowpack part of the domain, as convection in the underlying soil is assumed negligible. The temperature profile computed by SNOWPACK is used to set the thermal boundary conditions (at the top and base of the snow layer)—yes, the temperature field is used as boundary conditions—and to initialize the internal temperature field for the 2D OpenFOAM domain. Please note that the 2D temperature field solved within the OpenFOAM domain is not updated from SNOWPACK.

Synchronization is achieved via a dynamic mesh strategy implemented using the SNOWPACK-OpenFOAM coupling library. The vertical snow density profile and layer heights computed by SNOWPACK (which accounts for settlement and compaction) are continuously used to reconstruct OpenFOAM’s computational mesh. This ensures that the 2D OpenFOAM domain remains fully consistent with the vertical evolution of the 1D SNOWPACK profile (a prerequisite we ensured for all our simulations). Physical information transferred to OpenFOAM includes the updated mesh geometry, the updated temperature at top and bottom boundaries, and the updated density field. After OpenFOAM solves for the 2D air flow and vapor transport (sublimation and water vapor deposition), the resulting density change is averaged horizontally across the 2D domain for each specified height and fed back to SNOWPACK to update its internal state for the next mechanical/thermal step.

We confirm that the detailed description of the coupling strategy has been incorporated into the revised manuscript. We added this information as a new item in the "Materials and Methods" section, right after the introductory line: "The assumptions and limitations considered for SNOWPACK-OpenFOAM coupling are explained as follows:". This new item covers the initial conditions, SNOWPACK’s role, and the dynamic synchronization process.

3. 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.

Answer:

We understand the reviewer’s confusion regarding the complexity of the mass exchange procedure detailed in lines L87 to L98. The intricacy stems from the critical need to maintain full consistency between the 1D SNOWPACK domain and the 2D OpenFOAM domain while simultaneously preserving the lateral variability generated by the OpenFOAM convection solver.

This is achieved by separating the snow mass and volume changes into two distinct categories:

1D Changes (SNOWPACK-driven): Changes due to compaction, settling, melting, and refreezing. These processes are inherently 1D (uniform in the lateral direction) and are solved exclusively by SNOWPACK.

2D Changes (OpenFOAM-driven): Changes due to vapor transport and sublimation/deposition driven by convection. These are 2D (non-uniform in the lateral direction) and are solved exclusively by OpenFOAM.

The mass transfer procedure detailed in L87-L98 is the tracking mechanism required to apply the new 1D SNOWPACK state to the 2D OpenFOAM domain without erasing the crucial 2D lateral variations. To clarify this in the revised manuscript, we have replaced the complex procedural formula with a conceptual explanation, which clearly separates the 1D and 2D changes.

4. 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?

Answer:

Thank you for seeking this important clarification. No, in the coupled SNOWPACKFoam version, SNOWPACK does not compute vapor diffusion.

In the coupled solver, all vapor transport—including both diffusion and convection—is handled exclusively by the 2D OpenFOAM domain. This strategy is precisely to leverage the richer, spatially-resolved transport

solution provided by the OpenFOAM solver. SNOWPACK's only role regarding vapor transport is to receive and implement the density adjustments provided by OpenFOAM to maintain mass consistency within the 1D column.

We agree that since this question was raised, the manuscript is not crystal clear on this delegation of processes. Therefore, we have added a concise sentence to the "Materials and Methods" section (within the new item detailing the initial and boundary conditions and the coupling strategy) to explicitly state that all vapor transport (diffusion and convection) is delegated solely to the OpenFOAM solver.

5. 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?

Answer:

We appreciate this excellent question regarding the partitioning of energy conservation, and we confirm that this capability—replacing the SNOWPACK temperature field with the averaged OpenFOAM profile—is indeed an idea for future development and is not yet implemented in the simulations presented in this manuscript.

Regarding the hypothetical concern: No, introducing the averaged temperature profile from OpenFOAM would not break the energy conservation of the SNOWPACK model. The purpose of this future capability is to introduce the one-dimensional equivalent thermal effect of the two-dimensional convective/advective flow into the SNOWPACK profile, which is necessary to fully capture the physics of convection.

We cannot skip the resolution of energy conservation in SNOWPACK for the crucial reason of the boundary condition. SNOWPACK is required to compute the long-term energy budget and heat fluxes at the top (snow surface) and bottom (snow/soil interface) boundaries. These time-varying fluxes and temperatures are absolutely essential as boundary conditions for the 2D OpenFOAM domain.

By averaging the OpenFOAM temperature profile and feeding it back (in future versions), we ensure that the SNOWPACK temperature profile accounts for the smoothing/modification caused by convection.

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

Answer:

Thank you for this specific question regarding our methodology. The typical and maximum differences observed between the full transient (full integration) and flow freezing methods are demonstrated in Figure 12 (Figure 12 in the original manuscript).

For snow density, the difference between the two strategies is generally small, with the maximum observed difference being between 5 and 10 kg m⁻³. For cumulative snow density change, the difference follows the same order of magnitude. This small difference supports our choice of the computationally efficient flow freezing strategy.

We have added a clarifying sentence to the last paragraph of "Results and Discussion" in the revised manuscript where the mentioned figure is discussed to emphasize this finding.

7. 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.

Answer:

Thanks. We appreciate the reviewer's excellent suggestion and agree that mentioning the work of Sturm and Johnson (1991) provides valuable external support for our findings.

The observation that convection is continuous in thin snowpacks but sporadic in thicker snowpacks perfectly aligns with our results, which show continuous convection in the low-density Upper Bound scenario and sporadic convection in the dense, thicker Lower Bound scenario.

We have integrated the following sentence and citation into the first paragraph in "Results and Discussion" section.

8. 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.

Answer:

Thanks for the comment. Yes, T_m is the snow temperature defined in Jafari et al. (2022) as the intrinsic phase average of the gas and ice phases (the air-ice mixture temperature). This temperature does correspond to the temperature that a standard thermometer would read when placed in the snowpack.

We agree that the large lateral temperature differences observed in our simulations (shown in Fig. 10 of the original manuscript) are consistent with the non-uniform thermal structures caused by convection cells observed in the field, such as those reported by Sturm and Johnson (1991).

We have added this citation into the second last paragraph in the "Results and Discussion" section of the revised manuscript to connect our numerical findings to the existing observational literature.

9. 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.

Answer:

Thanks for the suggestion. We mentioned and referred to this figure in the "Material and Methods" section where the flow freezing technique is introduced as:

.....We found that (1) it takes 21 hours (with four MPI processors) of computer runtime while it takes only 5 hours with "flow-freezing" and (2) the averaged cumulative density changes have shown acceptably small difference as shown later in figure 12 (in the original manuscript) that the maximum difference in snow density is only between 5 and 10 kg m⁻³

1.0.3 Technical Comments

1. 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%”.

Answer:

Yes, we thank the reviewer for catching this imprecise wording in the manuscript. We agree that the term "accuracy of less than 10%" is confusing.

We have revised the text in both lines (L48 and L309 in the original draft) to clarify that the model comparison showed a defined range of error: we changed the phrase to "that shows an error level between 3 % to 10 %".

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

Answer:

Thanks. The ambiguous term "absolute" was used in the original text (L91) in the context of calculating mass changes for the complex coupling procedure.

As detailed in the response to the third specific comment (regarding L87-L98), we have completely replaced that confusing, formula-driven paragraph with a clearer conceptual explanation.

The new text makes the goal of the procedure explicit by separating the 1D mass changes (SNOWPACK: settling, melting/refreezing, water transport, and metamorphism) from the 2D mass changes (OpenFOAM: vapor transport and convection) without double-counting.

Therefore, the revised manuscript text (replacing the entire item 1 in the original draft) no longer uses the ambiguous term "absolute," making this point resolved.

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

Answer:

Thanks. We agree that the definition of "cumulative density change" was introduced too late. To resolve this, we have moved the line defining "cumulative density change" in the revised manuscript to two earlier, more

appropriate locations. It is now placed earlier in the same paragraph where the term is first used. It is also placed earlier in the "Materials and Methods" section (in item 5 of the original manuscript), where the quantity is necessary for the coupling description.

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