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-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.

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=8.7×10^{-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 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 snowpack 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 quantifies. 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

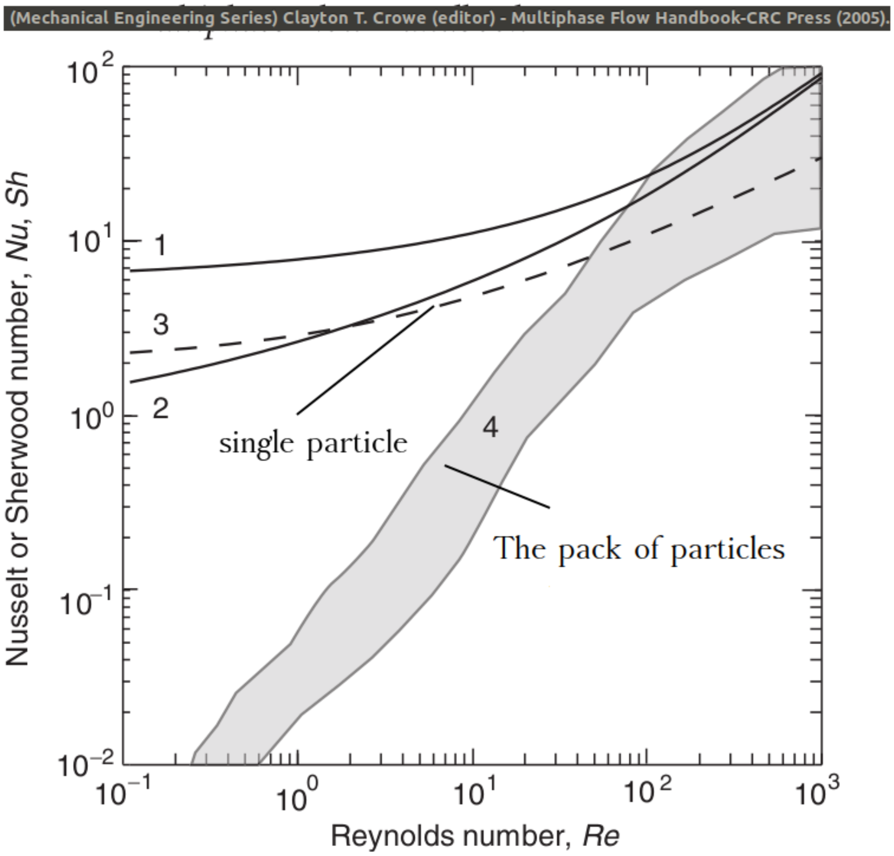

Figure 1: This figure is from Crowe (2005) to show a clear discrepancy for heat and mass transfer coefficients between pack of particles shown in region 4 and a single sphere shown in dashed line 3.

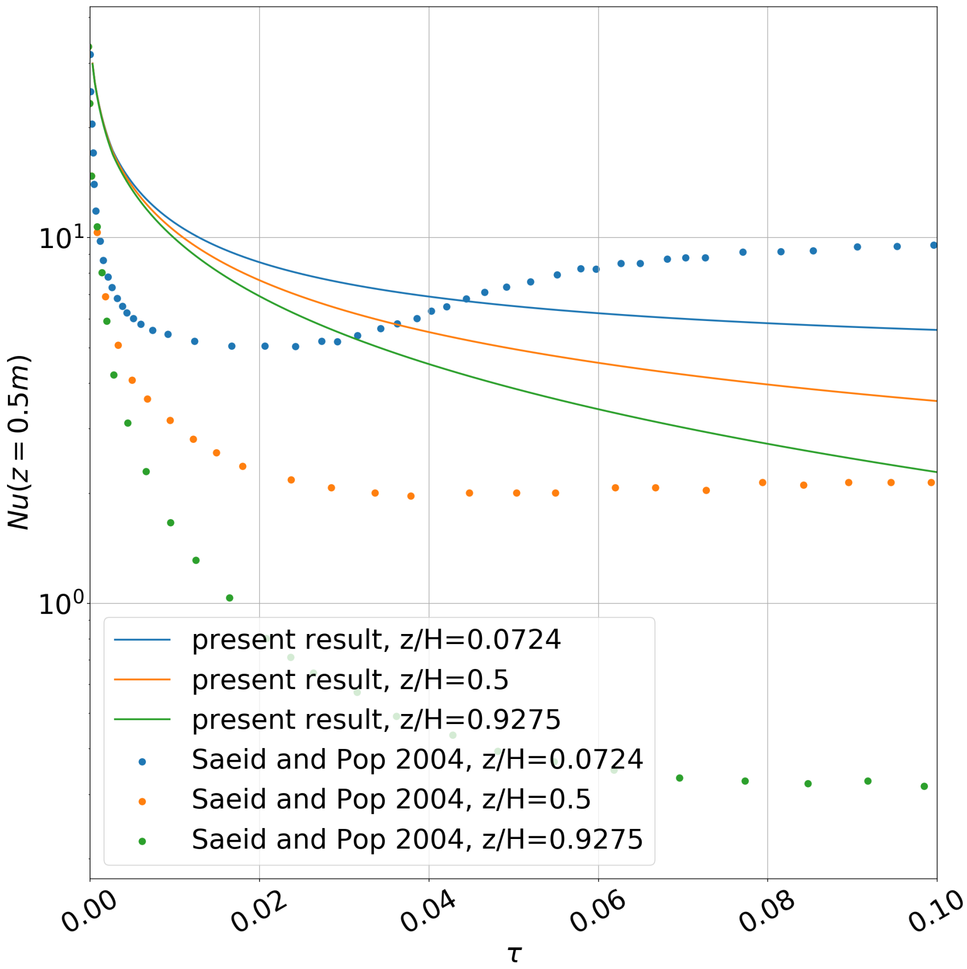

Figure 2: The temporal variation of the local Nusselt number using a large value of theoretical heat transfer coefficient for a single sphere.

as $h_m = \rho_i/(\beta\rho_{v,s})$. This is the case as Calonne et al. (2014) states that a larger value around $\beta = 10^9$ 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 $\beta = 9.7\times10^9$ 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 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 Tm? 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.

References

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Responses to Reviewer 2 for EGU-2025-3035: “Simulating the effect of natural convection in a tundra snow cover”

October, 2025

1 Answers to general comment, broad comments, and specific 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 comment

1. This is a review for “Simulating the effect of natural convection in a tundra snow cover”. Overall I really enjoyed this manuscript. Over the past few years I’ve read the various papers regarding the importance of estimating the near-ground low-density layer, and how difficult this is to simulate. The numerical scientist in me really likes the full-complexity model approach to attempt to bound the behaviour. A “what will this take” type of approach is great to see.

Answer:

We thank the reviewer for their exceptionally thoughtful comments and expertise, and for their enthusiasm toward our manuscript. We are genuinely thrilled that the reviewer enjoyed the work, and your positive feedback strongly encouraged us in the value of this research. We are particularly pleased that our full-complexity model approach to bounding the snowpack behavior is viewed as valuable, as this was the core intent behind our simulation design.

In response to suggestions and to further clarify the bounding nature of our work, we have restructured and extended the Results and Discussion section into distinct subsections. These revisions explicitly frame the two main simulation scenarios:

- The section now begins with a new introductory paragraph that directly compares field measurements with two key model outputs: the Lower Bound simulation (Convection + Compaction) and the SNOWPACK-only diffusion simulation (without compaction).

- The remainder of the section is now divided into two subsections to discuss the bounding scenarios:

  - Lower bound of convection with drifting-snow compaction model: Discusses the scenario where convection is realistically suppressed most of the time by the inclusion of a wind slab. We also note that convection might still be triggered in the early season before hard slabs form, or within the middle of the snowpack in a low-density layer located between two hard slabs, provided the surface is sufficiently cold and the weather is not too windy.

  - Upper bound of convection without drifting-snow compaction model: Discusses the scenario where convection operates freely (which corresponds to the main results presented in the original submission).

This refined organization makes the context and purpose of our approach to bounding convective effects much clearer to the reader.

1.0.2 Broad comments

1. First, this paper absolutely needs a set of research questions to guide the model development. Indeed the goals allude to this around L65. But it needs to be research questions otherwise it's "I ran the model and it did Y". I raise this because I think this paper sells itself a bit short in the discussion and conclusion. These are really valuable insights into a) the difficulty and b) importance of this process. I use models like Snobal and FSM in most of my work because it is computationally fast enough to deploy at a high resolution over millions of km. I'm left a bit unsatisfied as a snow modeller as to the next steps. Indeed you note some, but I mean practical next steps for the community. OpenFOAM is a massive hammer that is not going to be deployed over anything but a small point-scale domain (as done here) due to the computational and IC costs. So, I would really like a discussion on how, by answering these RQs, the authors can advance the more “applied” science. First, it's clear that even though massive computational effort was used, the results are still not correct

(e.g., density profiles). So, how do the authors think these results can inform parameterizations in other less complex models? Is it possible to parametrize around this without the full physics of OpenFOAM? I get very excited with these results as they are a clear step, but they are bespoke. What's next in advancing the snow modelling communities characterization and simulation of these layers?

Answer:

We thank the reviewer for this highly insightful and constructive critique. We strongly agree that the manuscript must clearly articulate its contribution to the applied snow modeling community and provide a clear, step by step path forward. We have addressed this broad critique through the following revisions in the manuscript:

1) Explicit Research Questions (RQs): We have revised the Introduction (around L65) to establish two core Research Questions (RQs), which now guide the entire narrative as:

"In this paper, we focus on two core research questions: To what extent does two-dimensional natural convection, simulated by the full SNOWPACK-OpenFOAM model, modify the one-dimensional temperature and density profiles of an Arctic tundra snow cover compared to standard diffusion-only models? and How do realistic snow physics, specifically wind slab formation (drifting-snow compaction), influence the occurrence and overall impact of natural convection in Arctic tundra snowpacks, thereby defining a lower bound for convective effects?."

2) Advancing Applied Science: Parameterization and Density Fidelity: the reviewer raises the critical question of how these bespoke, high-complexity results can inform scalable 1D models and why the density profiles still show error.

- We address the density profile error directly in the Results and Discussion section (end of the third paragraph). An important, yet negative result is that our setup is still unable to produce snow density profiles, which are close to observation. This could be due to effects of vegetation and heterogeneity discussed in Jafari and Lehning (2023) or some other physics not fully understood. This motivates further research, which is beyond the scope of the current paper, however.
- Parameterization Strategy (Conclusions): We detail our strategy in the Conclusions section in the last paragraph, clarifying the difficulties

and proposing a practical solution as:

"We acknowledge that a simple parameterization based on classical dimensionless numbers (like the Rayleigh number, Ra) for the entire snow column is problematic. Snow properties (the snow porosity, the effective thermal conductivity, and the intrinsic permeability) vary dramatically and constantly, making it difficult to define a single, representative Ra for a one dimensional model. The practical step forward is to use the high-fidelity SNOWPACK-OpenFOAM solution as a "truth set." We will extract the resulting effective convective vapor flux as a function of local one dimensional snow properties (density, grain type, the snow element temperature difference as ΔT) within snow layers. This generated data can then be used to calibrate or tune to find a new parameter as the water vapor diffusion enhancement factors (α_v) to be employed in one dimensional models, which is the most feasible way to integrate these complex two dimensional results into large-scale simulations."

2. Second, this paper needs a tighten w.r.t how some of the sections are presented. I guess it should have been obvious early on, but it wasn't that this was a 2D (x,y) model instead of a 1D col model with the processes added. I mean, it's obvious as I write this, but when I was reading the paper it didn't immediately jump at me. I think that there ought to be a small schematic just showing the (numerical) experimental setup in the main text to lead the reader through what is obvious to the authors (perhaps an opportunity to graphically detail the layer coupling?). I found the equation at l135 to come out of no where, and either walking the reader through this more or cutting it would be my suggestion.

   Answer:

   Thank you for these excellent suggestions on improving the manuscript's clarity. We agree that the dual-dimensionality of the coupled model and the necessity of the modified energy equation need clearer introduction. We have addressed the request for a clearer representation of the 1D/2D domains by enhancing Figure 1 (which shows the coupling procedure). The revised Figure 1 now includes a schematic that explicitly illustrates the 1D column (SNOWPACK) domain and the 2D domain (OpenFOAM), visually detailing the layer-by-layer coupling and the flow of information between the models. This makes the dual-dimensionality obvious earlier in the paper.

   Regarding justification for Equation at L135, we recognize that the energy equation appeared without sufficient context. We have chosen to keep the equation because it is mathematically essential for reproducibility of our numerical setup. As we noted in the original manuscript ("...except for the changes due to presence of water and the heat source/sink term from shortwave radiation absorption..."), this equation is the corrected energy balance specific to our coupled solver (SNOWPACKFoam). It differs from the standard convective solver used in idealized snowpack studies by Jafari et al. (2022) because it has been explicitly adjusted to account for the continuous mass and energy adjustments required during the SNOWPACK-OpenFOAM coupling. Keeping this modified energy equation allows researchers to accurately reproduce the numerical setups, whereas momentum and continuity equations remain standard and are referenced only by citation.

1.0.3 Specific comments- w/c = word choice

1. L1: “straightforward” OpenFOAM is almost never considered simple!

   Answer:

   Thank you for catching this confusing terminology. We fully agree that describing anything related to OpenFOAM as "straightforward" is misleading and undersells the complexity of the effort. The term was intended to refer to our direct numerical solution approach for convection, not the complexity of using the OpenFOAM platform itself.

   We have replaced "straightforward" with "direct" in the manuscript (L1 in the original draft) to accurately reflect the nature of our solution method while avoiding the implication of simplicity.

2. L3 convection of what?

   Answer:

   Thanks. We changed to "convection of water vapor" in the revised manuscript.

3. L4 “feeds” w/c

   Answer:

   Thanks. We replaced "feeds" with "transfers". The revised sentence will read as "OpenFOAM simulates convection in two dimensions based on

SNOWPACK snow profiles and transfers the convective vapor fluxes back to SNOWPACK."

4. L5 “numerical observed“ -> simulated?

   Answer:

   Thanks. We changed "numerical observed" to "simulated" in the revised manuscript.

5. L5 “coupler” remove? w./c -> model?

   Answer:

   Thanks. We replaced "coupler" to "coupling" in stead in the revised manuscript.

6. L6 “only if [..] surface layers” perhaps remove

   Answer:

   Thanks for the suggestion. We removed the entire line of "only if we neglect wind slab formation for surface layers." in the revised manuscript.

7. L7 “downward” w/c -> vertical?

   Answer:

   Thanks. We changed it to "vertical" in the revised manuscript.

8. L9 “in the SNOWPACK” insert “non-coupled” or similar to clarify

   Answer:

   Thanks for the suggestion. We added "non-coupled" in L9 and we have in the revised manuscript as "...respectively makes a consistent representation in the non-coupled SNOWPACK one-dimensional profile difficult".

9. L11 “this effect” what effect? be specific

   Answer:

   Thanks. In L11, We replaced "this effect" to "convective water vapor transport" in the revised manuscript.

10. L12 “and its interaction[. . .]” within snowpack or the surface wind model?

    Answer:

    Thanks for the suggestion. We added "within snowpack" at the end of L12 in the revised manuscript as "... and its interaction with snow settling and metamorphism within snowpack."

11. L17 “i.e” -> i.e.,

    Answer:

    Thanks. Done."

12. L22 “finite” as opposed to infinite distances?

    Answer:

    Thanks. Yes."

13. L22 “and changes in”

    Answer:

    Thanks. Done."

14. L24 “significant” w/c as significant implies statistical significance

    Answer:

    Thanks. We replaced “significant” with "statistically significant" in L24 in the revised manuscript."

15. L25 “convection” within the snowpack

    Answer:

    Thanks. We added "within the snowpack" at the L25 in the revised manuscript."

16. L27 “convection of water vapour” a small blurb about what this is might be helpful

    Answer:

    Thank you for this constructive suggestion. We agree that adding a concise physical description of water vapor convection in snow will enhance clarity for the broader readership. We have inserted a short explanatory phrase around L27 to describe the process as a buoyancy-driven movement of air and vapor:

    We added a clause defining the process, such as: "... However, convection of water vapor, the buoyant movement of air and moisture driven by temperature and air density gradients, is not captured in conventional snow models leading to large errors in simulated snow prop....."

17. L28 “convectional” do the authors mean conventional?

    Answer:

    Thanks. Yes now it is corrected to "conventional" in the revised manuscript.

18. L29 “Subarctic” -> subarctic

    Answer:

    Thanks. We have changed "Subarctic" to "subarctic" everywhere in the revised manuscript.

19. L34 love the ecological tie in. super important

    Answer:

Thank you for this encouraging comment. We are glad that the ecological connection resonated with you, as we believe it is essential to emphasize the real-world significance of accurate snow modeling beyond pure physics. We will retain this sentence as is.

20. L38 “SNOWPACK, as a [. . .]” remove as

    Answer:

    Thanks. Done.

21. L48 “direct” for the snow modeller not versed in numerical methods you might contextualize direct versus alternatives.

    Answer:

    Thanks. The term "direct" in our original sentence refers to the fact that our model uses a Direct Numerical Solution (DNS) method for solving the coupled flow and transport equations.

    We have revised the surrounding text in the manuscript to provide context for the broad readers. The revised sentence now reads: "The convection model used in this paper is a direct numerical solution (DNS) that solves the full set of coupled mass and energy transport equations, offering a high-fidelity alternative to traditional parameterizations. This model shows an error level between 3% to 10% (Jafari et al., 2022).""

22. L48 “accuracy between [. . .]” versus what?

    Answer:

    Thanks. We changed it to "This model shows an error level between 3% to 10% compared to the numerical benchmark (Jafari et al., 2022)." in the revised manuscript.

23. L50 I find this sentance awkward and difficult to fully comprehend. It doesn’t lead well into the next sentence either, so it’s not super clear what the authors wish to convey.

    Answer:

Thanks. We remove the part as "It is important to explicitly state a key ....which are tailored to Alpine conditions." in the revised manuscript.

24. L52 remove ", such as those [. . .] Island"

    Answer:

    Thanks. Done.

25. L53, L54 "no physically accurate" . . . "rough approximation" suggests it's at least partially physically accurate ??

    Answer:

    Thanks. As also suggested by reviewer 1, We added "partially physically accurate" and we have the revised L53 and the rest of paragraph as:

    "Arctic snowpacks are substantially influenced by wind compaction—a process for which no physically accurate, widely accepted model exists. The current parameterizations, including those proposed by Gouttevin et al. (2018) and recent studies by Keenan et al. (2021); Wever et al. (2023) for drifting-snow compaction, offer only a rough approximation (partially physically accurate). Including wind compaction in the simulation results in a high-density surface layer that suppresses the formation of convection cells, rendering natural convection less active."

26. L55 "natural convection" as opposed to?

    Answer:

    Thanks for the clarification request. We use "natural convection" to explicitly distinguish our modeled process from "forced convection" (i.e., wind pumping), which is the other major form of air movement in snow. Thus, we have added "driven by buoyancy forces" after "natural convection".

27. L61 "is not possible" why?

    Answer:

    Thanks for asking for this clarification. We agree that this point, while intuitive to 2D/3D solver users, must be explicitly stated for the broader snow modeling community.

We clarify that modeling natural convection is not possible in 1D snow models because natural convection is fundamentally a 2D or 3D phenomenon (it requires lateral dimensions to form flow cells).

We have revised the surrounding text in the Introduction (around L61) to explicitly state the justification. The revised sentence now reads:

"Modeling convection in one-dimensional snow models is not possible as one-dimensional models cannot capture the spontaneous formation and lateral flow fields of two dimensional convection cells..."

28. L70 turn these into research questions that will guide and support this manuscript but also inform the next steps

    Answer:

    Thanks. As answered in the first broad comment, we have added few lines for the research questions in the revised manuscript as:

    "In this paper, we focus on two core research questions: To what extent does two-dimensional natural convection, simulated by the full SNOWPACK-OpenFOAM model, modify the one-dimensional temperature and density profiles of an Arctic tundra snow cover compared to standard diffusion-only models? and How do realistic snow physics, specifically wind slab formation (drifting-snow compaction), influence the occurrence and overall impact of natural convection in Arctic tundra snowpacks, thereby defining a lower bound for convective effects?.

29. L72 strong disagree that this is a straightforward approach.

    Answer:

    Thanks. As answered earlier in the first specific comment, we changed "straightforward" to "direct" in the revised manuscript.

30. L80 maybe it's difficult to do but I think a figure illustrating this section would be helpful.

Answer:

Thanks. As answered earlier in the second broad comment, we revised figure 1 and it now includes a schematic that explicitly illustrates the 1D column (SNOWPACK) domain and the 2D domain (OpenFOAM), visually detailing the coupling and the flow of information between the models.

31. L81 “internal elements” w.c -> computational elements?.

    Answer:

    Thanks. Done.

32. L91 OF=OpenFoam, SN=Snowpack needs to be in the text

    Answer:

    Thanks. We have revised the entire relevant item to improve readability and added an explicit definition, stating: "Please note that the subscript OF and SN refer to OpenFOAM and SNOWPACK, respectively."

33. L105,120 could be shorter and tighter.

    Answer:

    Thanks. We shortened the entire paragraph in the revised manuscript as:

    "Currently, OpenFOAM provides only the laterally-averaged snow density change rate (due to vapor transport) to SNOWPACK. Future work will enhance this by incorporating the laterally-averaged temperature profiles from OpenFOAM back into SNOWPACK’s initial conditions and using the density change rate directly in SNOWPACK’s metamorphism calculation. It is theoretically possible to use multiple parallel SNOWPACK columns across the OpenFOAM domain to capture lateral heterogeneity. However, the resulting differences in compaction and snow height would create surface discontinuities, complicating the current dynamic mesh strategy. Numerically, this challenge can be addressed by using separate, disconnected meshes for each SNOWPACK domain, though careful attention would be required for defining boundary conditions and information exchange across these discontinuities."

34. L105 “only feedback” I think this shoudl be re written in the vernacular of couplers e.g., flux exchange between the models etc

    Answer:

    Thanks. We removed "feedback" in the revised manuscript.

35. L135 This eq comes out no where I found, and it’s complex enough to need a lot of time to read

    Answer:

    Thanks. As mentioned in the answer for second broad comment, we have chosen to keep the equation because it is mathematically essential for reproducibility of our numerical setup in the revised manuscript.

36. L135 “presented as” is confusing. was this modified from Jafari 2022 into this? or something else?

    Answer:

    Thanks for highlighting the potential confusion. We confirm that the energy equations presented are indeed modifications of the full energy equations found in Jafari et al. (2022).

    The revised text now reads: "The heat transfer equations for the ice-water mixture and the gas phase are similar to those presented in Jafari et al. (2022), but have been modified to account for the presence of water and the heat source/sink term from shortwave radiation absorption. The resulting modified energy equations for the gas phase and ice-water mixture, essential for the reproducibility of our coupled solver, are presented as:"

37. L138 Jv is missing definition

    Answer:

    Thanks. We have added the definition as "J_v is the diffusive water vapor flux" in the revised manuscript.

38. L164 Courant of 5. This feels a bit adhoc of a choice. How sensitive are the results to this choice?

    Answer:

    Thanks. The choice is governed by the highly transient nature of the coupled problem, where the thermal boundary conditions change every 15 minutes due to SNOWPACK. While our previous steady-state convection analysis (e.g., Jafari et al., 2022) showed that results are insensitive to Courant values up to 200, such high values compromise stability and accuracy in a highly dynamic, coupled system.

39. L164 PIMPLE = ?

    Answer:

    Thanks. We added a small explanation in L164 where PIMPLE is mentioned as "Note that it is possible to use the PIMPLE algorithm (a hybrid pressure-velocity coupling algorithm for solving incompressible flows) for higher values of the maximum Courant number in OpenFOAM..." in the revised manuscript.

40. L167 “faster” It might be faster but is the result right? Providing the wrong answer faster isn’t interesting. I think your text suggests it’s fine w.r.t error, but this can be tightened up a lot.

    Answer:

    The term "faster" refers to the computational efficiency gained by using OpenFOAM’s PIMPLE algorithm and adaptive time stepping (Courant number). For quasi-steady problems with fixed thermal boundaries, higher Courant numbers can achieve speed without sacrificing accuracy. However, in our case, we have highly dynamic thermal boundary conditions (driven by SNOWPACK). For such transient problems, stability and accuracy necessitate lower Courant numbers. We emphasize that the overall computational gain comes from optimizing the stability of the coupled solution by choosing lower Courant numbers, which indirectly makes the simulation as fast as possible while maintaining a stable and accurate result.

41. L175 “computer runtime” suggest you use the HPC vernacular of “wall clock” or similar. You need to distinguish between core hours and wall

clock either way.

Answer:

L175 has been revised as follows:

"We found that (1) the simulation required approximately 21 hours of wall-clock time using four MPI processes (i.e., about 84 core-hours in total), whereas the 'flow-freezing' approach reduced this to about 5 hours of wall-clock time, and (2) the averaged cumulative density changes exhibited acceptably small differences."

42. L177 “small differences” define small, e.g., $<$??

    Answer:

    Thanks. As also suggested by reviwer 1, we mentioned the values for the differences in the revised manuscript as "...exhibited acceptably small differences as shown later in figure 27 the maximum difference in snow density is only between 5 and 10 kg m^{-3}..."

43. L205 Above I note that explaining what this convection process is would help the reader. e.g., including the info from here would be good. “It's a process that is dependent upon thermal gradients and high density,. . . .”

    Answer:

    Thanks. We have added an explanation for the convection in the revised manuscript as "...Canadian High Arctic, conditions to trigger convection (the buoyant movement of air and moisture driven by temperature and air density gradients) are at least partially fulfilled".

44. L2XX these plots are really nice”

    Answer:

    Thank you for the very kind feedback! We are truly thrilled that you enjoyed the plots and found them informative.

45. L2XX I wonder if fig 3 and 4 could be made a 4 pane single plot?

    Answer:

    Thank you for this excellent suggestion. We agree that consolidating the figures will improve the manuscript's flow and visual efficiency. We have implemented this change in the revised manuscript by merging the related figures:

    1) Figure 3 and Figure 4 (original manuscript) have been merged into a single, comprehensive plot detailing the cumulative density change across scenarios.

    2) Figure 5 and Figure 6 (original manuscript) have also been merged into one figure, which presents the snow density change rate due to vapor transport.

46. L239 Figure 7 caption, define H and $\rho_{s,cum}$ If it's the same snow height as fig 5,6, please use the same y axis label

    Answer:

    Thank you for pointing out the necessity for clearer figure definitions and standardized axes. We have implemented the following revisions:

    1) We added the definitions for the key variables ($\Delta\rho_{s,cum}$ and ρ_s) in the caption, which now reads as:

    "Comparison between diffusion from SNOWPACK simulation (without drifting-snow compaction) and convection from SNOWPACK-OpenFOAM simulation (without drifting-snow compaction) for cumulative snow density change ($\Delta\rho_{s,cum}$) and snow density (ρ_s) profiles on 12 April 2015..."

    2) We standardized the y-axis label to "snow height [cm]" to ensure consistency across all relevant figures (Figures 2 and 6).

    3)We note that measured snow density profiles were removed from this specific figure. The detailed comparison with measurements for both the SNOWPACK and SNOWPACK-OpenFOAM simulations (including drifting-snow compaction) is now placed and discussed earlier in the "Results and Discussion" section to improve narrative flow.

47. FIgure 8, 9 need axis labels

    Answer:

    Thanks. Done.

48. L286-9 great result

    Answer:

    Thank you for the positive feedback! We are pleased that you found these results insightful and appreciate the encouragement.

49. L309 “benchmark” w/c/ observations?

    Answer:

    Thanks. This benchmark is a numerical benchmark and not observations. We change it to "numerical benchmark" in the revised manuscript.

50. L333 It would be great to have a simple concluding sentence somewhere in here where you can attribute X% uncertain to ignoring this process. A nice quotable sentence for other papers to cite, to really hit home ignoring this, even if not perfect, is costing X% in uncertainty”

    Answer:

    Thank you for the excellent suggestion to include both key uncertainties. We have revised the concluding sentence to be concise and highly citable, quantifying the impact on both density and temperature:

    The finalized, quotable sentence at the end of last paragraph in the Conclusion section reads as follows:

    "In summary, the substantial two-dimensional variations observed, up to 90 kg m^{-3} (115%) in localized snow density and up to 5 K (30%) in temperature, demonstrate that ignoring the two-dimensional nature of natural convection introduces over 100% uncertainty in localized density prediction and up to 30% uncertainty in snow temperature in conventional one-dimensional models, underscoring the necessity of addressing this process."

51. L335 Open science and data section?

    Answer:

    Thanks. It is now added in the revised manuscript.

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