Response to Reviewer #1 for EGU-2025-2724: *Isotopic Stratification and Non-Equilibrium Processes in a Sub-Arctic Snowpack*

We thank the reviewer for their thoughtful and constructive comments on our manuscript. We are pleased that they found the dataset and analysis valuable, and we appreciate their recommendation for publication after minor revisions. We have carefully addressed all comments and added details where requested.

For clarity, the reviewer's original comments are shown in *italics*, followed by our responses in **blue plain text**.

This paper reports the first continuous measurements of isotopic vapor composition performed in a natural sub-arctic snowpack, alongside measurements of the meteorological and isotopic compositions of the overlying air and ice within the snowpack. The benefits of these measurements are two-fold: (i) they can be used as discriminants to disentangle and clarify the physical processes at play in snowpacks (e.g. vapor diffusive versus advective transport or isotopic (non)-equilibrium between the air and ice phases) and (ii) they document important post-depositional effects in snowpack that alter cryospheric isotopic records and hydrological interpretations. I found the experimental method and its description compelling. The obtained data are very valuable and of great help to further our understanding of vapor physics in snowpacks. The authors provide an extensive analysis of the data and put forward a sound interpretation in terms of processes at plays. The main drawback I could see is that due to the large amount of information, it is sometimes difficult to identify the salient points of this study. Personally, the major information I got from the article (besides the demonstration that vapor isotopic concentration can be continuously monitored) are: (i) vapor and ice in snow are in thermodynamical disequilibrium (contrary to what is usually assumed by snow/firn isotopic models) (ii) the soil appears have a detectable influence on the isotopic composition of the bottom snow, (iii) vapor isotopic composition shows snowpack-wide mixing events during high wind events, indicative of wind-induced ventilation, and (iv) there is a strong interaction between the vapor composition of the near surface snow and that of the overlying atmosphere (consistently with previous results reported in the literature). But streamlining the presentation of this amount of information is not an easy task and to be frank I do not have that many ideas on how to do it. In all cases, I think the complexity of the observed data (with correlations that strengthen/weaken over time) shows that the isotopic profiles (vapor and ice) in the snowpack result from the interaction of multiple processes and provide a nice benchmark to evaluate theoretical and numerical models. In conclusion, I think this study is well suited for the cryosphere and I recommend its publication after some minor revisions.

General Comments

Influence of pumping on water vapor measurements: The water vapor to be analyzed by the CRDS is sampled by pumping through 6.5m of tubing. I was of the general impression that the pumping of a gas mixture can introduce some "pressure-gradient fractionation". Has it been quantified and could it have a detectable influence on the measurements performed in this study or is the effect too small to affect the results?

We appreciate this point. In our setup, vapor was sampled through 6.5 m of 1/4" tubing (OD 6.35 mm, ID 5.8 mm) at ~40 mL min^{-1}(instrument pump flow rate). Using site conditions (median T = −5.9 °C, p = 998 hPa), air properties from the ideal-gas and Sutherland relations give $\rho \approx 1.30\ \mathrm{kg\ m^{-3}}$ and $\mu \approx 1.69\times10^{-5}\ \mathrm{Pa\cdot s}$.

Flow regime (Reynolds number).
$Re = (\rho\, v\, D) / \mu = (4\, \rho\, Q) / (\pi\, \mu\, D)$
With $Q = 6.667\times10^{-7}\ m^3\ s^{-1}$ and $D = 5.8\times10^{-3}$ m, we obtain $Re \approx 11$ (laminar).

Pressure drop (Hagen–Poiseuille law).
$\Delta P = (128\, \mu\, L\, Q) / (\pi\, D^4)$
With L = 6.5 m, this yields $\Delta P \approx 2.6$ Pa across the full line.

This pressure drop is negligible relative to atmospheric pressure and far too small to induce measurable pressure-gradient isotope fractionation. Hence, any pumping-related fractionation is negligible compared with the isotopic variability reported in this study.

Characterization of the soil: Has the ice/vapor in the underlying soil been isotopically characterized? Or at least, do the measurements in the bottom part of the snowpack give a consistent picture of the isotopic nature of the soil vapor (enriched δ 18O and low d-excess if I follow L613)? This seems quite important to me in order to be able to discuss the effects of water vapor flux from entering the snowpack from the ground.

We did not directly sample the isotopic composition of the underlying soil (neither soil pore vapor nor soil ice) in this study. However, the basal snow and co-located pore-space vapor (0–5 cm) consistently exhibit enriched $\delta^{18}O$ and depressed d-excess relative to overlying layers, particularly during the early/mid periods. This vertical pattern and its persistence when upward temperature gradients favor vapor export from the ground aligns with the reviewer's interpretation of a soil-vapor influence on the bottom of the snowpack. We have clarified this inference in the Discussion near L613, noting that while the signal is consistent with a soil endmember, quantification requires direct soil vapor measurements, which we identify as a priority for future work.

Access to data: I did not manage to access the restricted data. If I understand correctly, they contain all relevant processed data discussed in the text (meteorological data, snowpack samples data, vapor composition data, precipitation/surface composition data, and snow temperature/humidity data). I trust the authors to have included all that (and maybe some more that I'm forgetting), but I just wanted to be sure.

Thank you for flagging this. The dataset has been uploaded to our repository and is currently under restricted (embargoed) access for peer review. In line with our policy, the archive will be made public upon publication (DOI listed in the Data Availability statement). If the reviewer would like to inspect the data during review, we will be happy to provide temporary private access upon request.

The repository contains all processed data used in the manuscript.

Specific Comments

L46 - As far as I understand the impacts of snow decrease on soil temperature cannot be simplified to a consistent acceleration of soil warming. Thinner snow covers also favor the cooling of the permafrost during winter. The net impact of a changing snow cover on the ground can either be a warming or cooling effect, depending on the particular conditions at play (e.g. Lawrence and Slater, 2010).

We thank the reviewer for this clarification. We agree that the effect of reduced snow cover on soil temperature is not unidirectional. A thinner snowpack can indeed enhance soil cooling during winter by reducing insulation, while in spring and summer it may lead to earlier soil warming due to reduced shading and earlier melt onset. We have revised the sentence at L46 to better reflect this dual effect and now cite Lawrence and Slater (2010) to acknowledge that the net impact depends on seasonal timing and local conditions.

L65 - I would not use "predominantly" here, as I do not understand what could be a third type of process that is neither a surface nor a sub-surface process.

We appreciate the reviewer's observation. Our intention was simply to distinguish between surface-driven processes (e.g., sublimation/condensation at the snow–atmosphere interface) and subsurface processes (e.g., vapor diffusion and recrystallization within the snowpack). The word "predominantly" is unnecessary and potentially confusing, so we have removed it in the revised manuscript.

L95 and elsewhere - For me snow is the resulting (macroscopic) mixture of ice + humid air. Therefore, the notion of (dis)-equilibrium between the air and the snow sounds a bit odd. I understand that sometimes "snow" is used interchangeably with "ice", but since this paper focuses on the distinguishing the isotopic composition of the air phase to that of the ice, it might be worth to use the word "ice" rather than "snow" when specifically discussing the composition of the ice matrix.

We thank the reviewer for this valuable suggestion. We agree that using the term "snow" to describe the isotopic composition of the solid phase can be misleading, since snow is indeed a mixture of ice grains and pore-space vapor. To avoid ambiguity, we have revised the text throughout to use "ice" when specifically referring to the isotopic composition of the solid matrix and reserved "snow" for the snowpack as a whole (ice + vapor).

L171 - I do not understand how the calibration and correction of the measurements was performed. The text implies that there is only one a and b per isotopes (so one {a,b} for DHO and another {a,b} for H2 18O), but I do not see how it could be used to get from a raw δX to a corrected δXcorr. Perhaps there is a missing δX in the formula.

We appreciate the reviewer's careful reading. We have clarified the notation and equations in the Methods. Humidity corrections were applied using a nonlinear regression of the form $\delta X_{corr} = a + {}^{b}/_{q}$, where δX_{corr} is the difference between observed isotopic value and the actual standard isotopic value ($\delta^{18}O$ or $\delta^{2}H$), *q* is the water vapor mixing ratio, and *a* and *b* are empirically determined constants.

L173 - Why isn't the correction applied to snowpack vapor measurements as well?

This humidity correction was applied to both the ambient-air vapor dataset and the snowpack vapor dataset. We have corrected this point in the revised manuscript to avoid confusion.

L180 - What are the two corrections? There is one for humidity but it is not clear to me what is the other one.

We thank the reviewer for noting this lack of clarity. In addition to the humidity correction described above, we also applied a scale correction to place the CRDS measurements on the VSMOW2 scale. This was done by measuring liquid standards of known isotopic composition that were vaporized and introduced to the analyser and deriving linear offsets for $\delta^{18}O$ and $\delta^{2}H$. Together, the two corrections are: (i) the humidity-isotope correction, and (ii) the VSMOW2 scale normalization.

L231 - I just wanted to mention that the fact that the relative humidity of the vapor was measured within the snow during the whole season and consistently shows saturation is very valuable. It is a crucial piece of information for vapor physics dynamics.

Thank you for highlighting this point. We have removed the statement implying persistent saturation. As Reviewer 2 also noted, our in-snow RH probes were unheated (to avoid perturbing the snowpack) and likely experienced frosting, which can artifactually report RH ≈ 100%. Using the CRDS water vapor mixing ratios together with co-located snow temperatures, we infer that pore-space vapor was undersaturated. The Methods now state that in-snow RH probe data are not used quantitatively and that RH over ice is instead computed from CRDS humidity and temperature. We also note in the revised discussion (in response to Reviewer 2's major comment) that processes such as barometric pumping can contribute to pore-space undersaturation, further supporting the interpretation that the snowpack was not persistently saturated.

L235 - If possible please include a brief description of the snowpack stratigraphy. It doesn't have to be super precise I think, but the potential presence (or absence) of slabs or crusts that impede water vapor movement could be important information.

We have added a brief, qualitative stratigraphy description. While we did not conduct an extensive grain-crystal analysis, field notes and hand-lens observations indicate: a coarse, low-cohesion basal layer; a distinct mid-pack refrozen ice layer that formed after the first warm event; and finer, more cohesive layers above. We acknowledge that a detailed stratigraphy analysis will be very beneficial and we prioritize this in future work.

L280 - Perhaps rename the section to precise that it focuses on variations at the seasonal time scale, when the previous section focused on diurnal variations.

Thank you for the suggestion. We have revised the heading to clarify the time scale:
"3.3 Seasonal Variations in Isotopic Composition of Water Vapor in the Snowpack–Atmosphere Continuum."

L295 - I understand the strengthening of the correlation between the 5 and 15cm levels could suggest some sort of enhanced mixing in the later period (I'm thinking of wind pumping based on the latter part of the article). But isn't the argument partly countered by the fact that the correlation between the ambient air and the snowpack diminishes over time, that could suggest reduced mixing between the snowpack and the ambient air?

Rather, late-season conditions, low RH, higher winds, and stronger radiation, introduce additional processes in the ambient air (e.g., enhanced surface sublimation and kinetic fractionation, boundary-layer regime shifts, and episodic advection) that increase high-frequency isotopic variability in the ambient air, which do not readily transmit into the snowpack.

L479 - Could you explain and/or specify why the relation between ambient δ 18O and T in the early period indicates near-equilibrium? The equilibrium fractionation factor decreases with temperature. I would thus expect a negative correlation between δ 18O and T if the ambient vapor was in isotopic equilibrium with some ice source. But perhaps the authors meant something else.

Thank you for this valuable comment. We agree that if the ambient vapor were in strict isotopic equilibrium with local ice, a negative correlation between δ^{18}O and temperature would be expected because the equilibrium fractionation factor increases with decreasing temperature. In our case, the observed positive δ^{18}O-T correlation in early winter instead reflects the hydrological cycle isotope–temperature relationship, where colder air masses are isotopically more depleted due to upstream condensation, and warmer air masses are less depleted. This suggests that during the early period, boundary-layer stability limited snowpack–atmosphere exchange, allowing the vapor isotopes to closely follow regional air mass history and radiative cooling, rather than snow surface equilibrium. We have revised the text accordingly for clarity.

L618 - I do not follow why temperature serves as a diagnostic of the diffusion-advection transition. Could you elaborate?

We thank the reviewer for this comment and agree that our original phrasing could be misleading. We have rephrased the text accordingly. Barometric pumping was active throughout the season (new analysis, see response to major comment of Reviewer 2), but periods of increasing temperature coincided with low relative humidity and high wind speeds, which together caused ambient vapor d-excess to rise. These enhanced d-excess signals were then pumped into the snowpack, reinforcing advective transport. Thus, temperature itself is not the mechanistic driver of the diffusion–advection transition, but it acts as a covariate indicating boundary-layer conditions (low RH, high wind, elevated vapor d-excess) that favoured ventilation over diffusion.

Technical Comments

L123 - What is the depth of the ground sensor?

I assume to you mean the soil temperature sensor. I was at a depth of 5 cm. This has been added to the text

Fig 1. - If possible, it might be interesting to put a picture of the actual setup. It could be done in the supplementary material not to clutter the main part of the article.

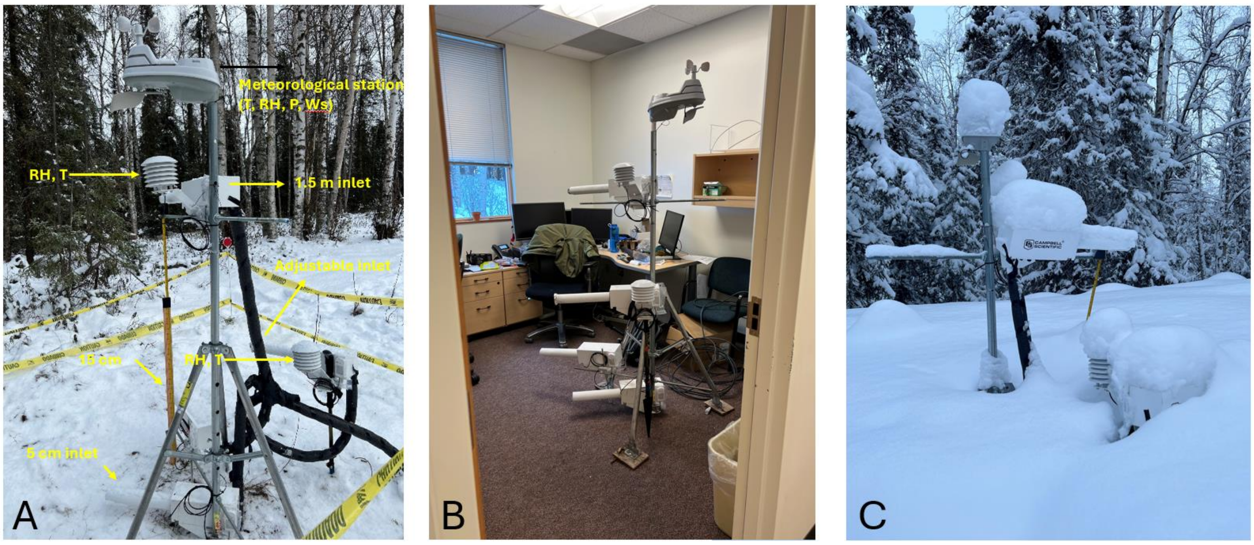

Figure 1. Field and laboratory setup of the vapor isotope monitoring system. (a) Deployed tower at the University of Alaska Anchorage showing vapor intakes at 5 cm and 15 cm above ground within the snowpack and the 1.5 m ambient air intake, along with meteorological sensors measuring RH, T, wind speed, and pressure. Heated and insulated tubing connected each inlet to the isotope analyzer. (b) Laboratory assembly and testing of the system prior to field installation. (c) Field deployment after snow accumulation.

L151 - Is there a reason why the fourth line was not included in the study in the end?

he original purpose of the fourth inlet was to measure vapor isotopic composition just above the snowpack surface (<5 cm). However, as the season progressed this inlet was repeatedly buried by new snowfall, requiring frequent re-adjustments to keep it near the surface. This made the vertical position of the inlet inconsistent over time, complicating comparisons with the fixed-height inlets (5 cm, 15 cm, 1.5 m). In addition, snow captured around the inlet likely altered the vapor in its immediate vicinity, introducing potential artifacts. To ensure comparability and consistency across measurement levels, we decided not to include data from this fourth inlet in the analysis. We now clarify this in the revised Methods section.

L171 - Just out of curiosity, is there a theoretical reason to search the correction under this specific form, or it is based on the shape of the curves in Fig. S1?

The correction function we used does not arise from a strict theoretical model of isotope fractionation with humidity. Instead, the form was chosen empirically to best represent the calibration data (Fig. S1), consistent with the nonlinear humidity dependence reported in earlier CRDS studies (Weng et al. 2020). In other words, the “shape” of the function is based on the data rather than a first-principles derivation, but it is physically motivated in that humidity-dependent optical effects in the cavity are known to produce such nonlinear behaviour.

L171 - The function yields δXcorr but only δX is presented instead in the text.

δX_corr is the difference between observed isotopic value and the actual standard isotopic value this has been corrected in the manuscript

L179 - It seems that this sentence just restates what has been said in the paragraph above (standards and ambient measurements are corrected with some non-linear function of q).

This sentence has been removed as it was redundant

L223 and Fig. 2 - Precise that this is the relative humidity of the overlying atmosphere (not to be confused with the relative humidity of the snow vapor)

Thanks this has been corrected to specify ambient relative humidity both in the text and the figure caption

L255 and L629 - I think the word “more” is missing.

Thanks this has been added

L380 - From what I understand, $\Delta\delta^{18}O = \delta^{18}O_{15cm} - \delta^{18}O_{5cm}$ (the difference in isotopic composition between spatial points in the snowpack, which can be applied both for the ice matrix and the vapor). If so, please indicate it clearly so it is not confused with the ice-vapor isotopic difference.

Thanks, the sentence has been rephrased

L379 to 396 - The split between the two paragraphs is a bit strange to me. It is §{Δδ 18O in ice} then §{Δδ 18O in vapor + Δd-excess in ice and vapor}. I would rather expect §{Δδ 18O in ice and in vapor} and then §{Δd-excess in ice and vapor}.

The paragraphs have been restructured

L497 - I think there are Δ missing: isn’t it the Δδ 18O and Δd-excess that respond to ΔT?

Thanks, Δ has been added

L535 - It seems that the last part of the paragraph is a just a re-wording of what has been stated above (namely that surface sublimation release high d-excess vapor, that then increase the d-excess of the ambient air and of the deeper snow when wind pumping is present). Consider removing it to lighten the text.

Thanks, the redundant text has been removed

L596 - Δδ 18O is already defined as the difference in δ 18O between layers. Perhaps simply use the name “ 18O disequilibrium (defined as measured δ 18O minus the theoretical δ 18O equilibrium value)”.

Thanks this has been changed

Supplementary Material L3 - Remove “com”.

Done

References:

Weng, Y., Touzeau, A., and Sodemann, H.: Correcting the impact of the isotope composition on the mixing ratio dependency of water vapour isotope measurements with cavity ring-down spectrometers, Atmos. Meas. Tech., 13, 3167–3190, https://doi.org/10.5194/amt-13-3167-2020, 2020.

Lawrence, D. M., & Slater, A. G. (2010). The contribution of snow condition trends to future ground climate. Climate Dynamics, 34(7), 969–981. https://doi.org/10.1007/s00382-009-0537-4