Review of EGU-2025-2724: “Isotopic Stratification and Non-Equilibrium Processes in a Sub-Arctic Snowpack”

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?

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 $\delta^{18}O$ 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.

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.

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).

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.

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.

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 $H_2{}^{18}O$), but I do not see how it could be used to get from a raw δX to a corrected δX_{corr}. Perhaps there is a missing δX in the formula.

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

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

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.

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.

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.

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?

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

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

Technical Comments

L123 - What is the depth of the ground sensor?

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.

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

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?

L171 - The function yields δX_{corr} but only δX is presented instead in the text.

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).

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)

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

L380 - From what I understand, $\Delta\delta^{18}O= \delta^{18}O_{15} - \delta^{18}O_{5}$ (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.

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

L497 - I think there are Δ missing: isn’t it the $\Delta\delta^{18}O$ and Δd-excess that respond to ΔT?

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.

L596 - $\Delta\delta^{18}O$ is already defined as the difference in $\delta^{18}O$ between layers. Perhaps simply use the name “^{18}O disequilibrium (defined as measured $\delta^{18}O$ minus the theoretical $\delta^{18}O$ equilibrium value)”.

Supplementary Material

L3 - Remove “com”.