Review of EGU-2026-2589: “Snow Structure Modulates the Isotopic Imprint of Sublimation”

The manuscript addresses an important problem: how snow structure modulates isotope fractionation during sublimation. The controlled wind-tunnel experiments are novel and the dataset is valuable because it combine controlled airflow, humidity, multiple snow types and isotope measurements under laboratory conditions.

The paper is generally well written and the experiments appear carefully executed. The conclusions—that airflow controls sublimation rate whereas snow structure influences isotopic evolution—are supported by the observations.

However, I think the manuscript repeatedly moves beyond what the data actually demonstrate. Throughout the Discussion the authors present theoretical interpretations (ventilation, pore connectivity, residence time, internal vapor recycling) that are not directly measured. In several places, empirical correlations are interpreted almost as theoretical proof.

Consequently I think the paper requires major revision, primarily because the interpretation needs to be more carefully aligned with the available evidence.

We thank the reviewer for the constructive evaluation of our manuscript. The reviewer’s comments are reproduced in blue, while our responses are provided in black.

We agree with the reviewer’s central concern that mechanisms involving pore-space ventilation, vapour residence time, pore connectivity, and internal vapour recycling were discussed too strongly despite not being measured directly.

We have revised the manuscript throughout to distinguish clearly between measured observations and process interpretations. The unmeasured mechanisms are now presented as possible explanations rather than demonstrated controls.

Major comment:

The central conclusion of the manuscript appears to overstate the extent to which porosity measurements demonstrate the proposed mechanism. The interpretation throughout the manuscript implies a causal chain from snow structure to pore connectivity, vapor residence time, and finally isotope modification. However, only the relationship between snow structure and porosity is directly constrained by the measurements, whereas the other steps are concluded based on this. In particular, the manuscript repeatedly attributes isotopic differences to factors such as pore connectivity, tortuosity, vapor pathway length, and vapor residence time, but none of these parameters were quantified. Therefore, the interpretation in Sections 4.2–4.4 should be revised to more clearly distinguish measured relationships from theoretical hypotheses, and the wording should be adapted accordingly.

The original Discussion overstated the mechanistic interpretation of the observed porosity–isotope relationships. Our measurements directly constrain initial bulk density and porosity and their empirical association with isotopic change, but they do not directly constrain pore connectivity, tortuosity, vapour pathway length, permeability, or vapour residence time.

We have therefore revised Sections 4.2–4.4 to distinguish clearly between measured relationships and proposed physical mechanisms. Statements implying that porosity directly controls specific vapour-transport processes have been removed or reformulated as hypotheses consistent with the

observations. Bulk porosity is now treated as an experimentally measured descriptor of snow physical state and as a possible proxy for unmeasured microstructural properties, rather than as direct evidence for a particular mechanism.

Accordingly, the revised Discussion emphasizes that the experiments demonstrate snow-type-dependent and porosity-associated differences in isotopic response, while the roles of pore connectivity, tortuosity, permeability, vapour pathway length, and vapour residence time remain plausible mechanistic interpretations that cannot be independently resolved from the present measurements.

Porosity may not be the dominant variable controlling the observed isotopic response. There are several alternative structural parameters which could also explain the observed differences, like crystal structure, specific surface area (SSA), grain curvature, and/or permeability. These properties are likely to correlate with porosity, making it difficult to isolate the independent effect of porosity. Throughout the manuscript, porosity is sometimes treated as the primary controlling variable, whereas the experiments in fact compare three snow types with distinct formation histories and potentially structural differences. The authors should acknowledge that porosity may function as a proxy for an unmeasured microstructural property rather than being the only or dominant driver of the observed isotopic variations.

Bulk porosity is not necessarily the dominant control on the observed isotopic response, and our experimental design does not allow its independent effect to be separated from other snow structural properties.

The three snow types differed in porosity and may also have differed in specific surface area, grain curvature, pore connectivity, tortuosity, and permeability. Because these properties may covary with porosity, the observed porosity–isotope relationships cannot be interpreted as evidence that porosity itself is the unique or primary causal variable.

We have therefore revised the manuscript throughout to remove statements that present porosity as the dominant mechanistic control. Instead, porosity is now described as a directly measured bulk descriptor of snow structural state and as a possible proxy for other unmeasured microstructural properties. We also explicitly acknowledge that the observed isotopic differences may result from a combination of correlated structural characteristics that cannot be resolved individually with the present measurements.

The manuscript concludes that Craig-Gordon (CG) "systematically overpredicts enrichment" but the model is missing one important measured quantity: ambient vapor isotope composition. Instead the authors eliminate ambient vapor using relative comparisons. Mathematically this is ok but physically this is problematic. The cancellation assumes identical vapor isotope composition, identical exchange histories and no snow-type-specific vapor modification. These assumptions needs much more discussion. Otherwise readers may conclude "CG fails" when in reality "CG under these assumptions fails." Those are different conclusions.

In response to this concern and related comments from the other reviewer, we have removed the Craig–Gordon modelling analysis. This avoids drawing conclusions about model performance that are not sufficiently constrained by the available experimental data.

The treatment of airflow effects remains largely qualitative. The manuscript frequently discusses processes such as boundary layer thickness, ventilation, turbulent renewal, and vapor residence time as potential controls on isotopic fractionation; however, surface wind speed and other direct measures of airflow conditions were not quantified. Although the authors acknowledge this limitation in the Methods section, the Discussion does not consistently maintain this distinction between measured variables and concluded processes. Statements such as "turbulent renewal limits isotopic discrimination" are physically plausible, but they represent a proposed mechanism rather than an experimentally demonstrated relationship in this study. The interpretation should therefore be revised to emphasize that airflow-related effects are hypotheses supported by the observed isotopic patterns, rather than directly constrained drivers of the reported results.

Our experiments directly constrain the imposed airflow regime through fan rotational speed and corresponding bulk tunnel airflow, but we did not measure near-surface wind speed, boundary-layer thickness, turbulence, or vapour residence time at the snow surface. These processes therefore cannot be treated as directly demonstrated controls on isotopic fractionation.

We have revised the Discussion accordingly. Statements implying that turbulent renewal, boundary-layer thinning, enhanced ventilation, or reduced vapour residence time were experimentally demonstrated mechanisms have been removed or reformulated as process hypotheses. The revised manuscript now distinguishes between the measured result that isotopic responses differed systematically among the imposed zero-, low-, and high-airflow regimes and the physical mechanisms that may explain those differences.

For example, statements such as "turbulent renewal limits isotopic discrimination" are now phrased more cautiously to indicate that stronger airflow may enhance atmospheric exchange and reduce the isotopic imprint retained in the snow, but that the relevant near-surface transport processes were not directly measured. Boundary-layer thickness and vapour residence time are therefore discussed only as plausible interpretations consistent with the observed airflow-dependent patterns, rather than as experimentally constrained drivers.

We have applied this distinction consistently throughout the Discussion so that conclusions are based on the measured airflow regimes and isotopic responses, while the associated boundary-layer and vapour-transport mechanisms are clearly identified as hypotheses.

The manuscript attributes differences between snow types to structural properties affecting the vapor transport. However, all samples were levelled prior to the experiments to produce a uniform surface. This preparation step may itself modify the near-surface microstructure through compression, destruction of fragile dendrites, or changes in pore connectivity and permeability. Because these effects are likely to differ between fresh, aged and laboratory snow, the levelling procedure may have changed the surface layer that directly controls sublimation and pore ventilation. Please discuss how sample preparation may have influenced the measured isotopic response and whether this could contribute to the observed differences between the snow types.

The snow samples were handled gently during preparation. Snow was placed into the Petri dishes, and the surfaces were carefully levelled with a soft brush to ensure uniform exposure to the airflow. No deliberate compression or compaction was applied, and particular care was taken to minimize disturbance of the snow crystals.

Nevertheless, we acknowledge that even gentle handling and levelling may alter the near-surface microstructure, with effects that may differ among snow types. Since these changes were not measured, we cannot completely exclude their contribution to the observed isotopic differences. We have therefore added this limitation to the Discussion and clarified that the measured isotopic differences reflect the prepared snow samples as used in the experiments.

The manuscript assumes that the wind tunnel is a closed environmental chamber. However, I could not find any validation of chamber leak-tightness or quantification of air exchange with the surrounding cold container. The reported uniformity of RH and temperature along the tunnel demonstrates spatial homogeneity but does not necessarily demonstrate isolation from the external environment. This is particularly relevant for the zero-airflow experiments, where even modest leakage could influence humidity and the isotopic composition of the surrounding vapor. Please clarify whether leak testing were performed, and discuss how possible exchange with the surrounding cold room might influence the interpretation of the sublimation experiments.

The wind tunnel is mounted on a vertical lift and lowered against the base plate and rubber gasket to define the flow channel and minimize bypass flow. The tunnel itself is not airtight: the upstream inlet and downstream outlet remain open to the surrounding temperature-controlled container atmosphere. During ventilated experiments, the outlet fans draw container air through the tunnel, whereas during zero-airflow experiments the fans are switched off but passive exchange through the open inlet and outlet remains possible.

In the zero-airflow experiments (EXP5 and EXP8), the fans were switched off, eliminating forced airflow through the tunnel. However, the inlet and outlet remained open to the surrounding container atmosphere, so passive exchange could still occur. We have therefore clarified that “zero airflow” refers to the absence of fan-driven ventilation rather than isolation of the tunnel atmosphere.

We have revised the Methods to describe this configuration more clearly and have expanded the presentation of the measured environmental conditions. Revised Figure R1 now shows the experiment-mean temperature and relative humidity measured separately at the wind-tunnel inlet, midpoint, and outlet, together with the conditions measured in the surrounding container. The error bars show the temporal variability at each measurement location. In addition, Figure R2 explicitly separates temporal variability within each experiment from spatial variability among the three tunnel sensor positions. These figures therefore provide a quantitative description of the atmospheric conditions experienced during each experiment, including the zero-airflow experiments, without assuming that the wind tunnel was isolated from the surrounding container.

Moderate comments:

The only use of bulk isotope measurements poses a major challenge for making meaningful theoretical conclusions from the findings because any vertical or spatial isotopic gradients within the snowpack are lost. This limits the ability to discuss internal vapor transport processes, isotope redistribution pathways, or the development of microscale fractionation patterns. But these mechanisms are forming a central component of the manuscript’s interpretation. Although the authors note this limitation, it deserves more emphasis in the Discussion section.

We agree that the bulk isotope measurements from EXP1–EXP8 cannot resolve vertical isotope gradients and therefore cannot, by themselves, identify internal vapour-transport pathways or

microscale fractionation processes. We have strengthened this limitation in the revised Discussion and reduced mechanistic interpretations based solely on the bulk data.

To address this limitation, we added two process experiments (EXP9 and EXP10) in which the upper and lower 1 cm of the snow were analysed separately relative to the measured initial isotopic composition (Fig. R3). These experiments were conducted under strong imposed thermal forcing (~920 and ~1000 K m^{-1}, respectively). These values represent base-to-air boundary-gradient proxies rather than measured internal snow-temperature gradients. Nevertheless, the large temperature differences imposed across the approximately 2 cm snow layer in EXP9 and EXP10 indicate that substantial internal temperature gradients were likely present in both experiments, although their exact magnitude and vertical distribution were not measured. The experiments were conducted under contrasting airflow regimes: EXP9 had zero forced airflow, whereas EXP10 had high airflow.

Figure R3 shows that vertical isotope structure developed under both conditions and would have been obscured by bulk sampling. Exposed samples showed enrichment of the upper layer relative to the lower layer in $\delta^{18}O$ and $\delta^{2}H$, together with lower d-excess. Covered samples showed the opposite vertical pattern: the upper layer had lower $\delta^{18}O$ and $\delta^{2}H$ and higher d-excess than the lower layer.

The contrasting vertical patterns in the covered and exposed samples are consistent with internal isotope redistribution under the imposed temperature gradient. In the covered samples, sublimation from relatively warm ice surfaces in the lower part of the snow could leave the residual lower-layer ice isotopically enriched, while the resulting vapour is transported toward the colder upper part of the snow. In the exposed samples, the strong enrichment of the upper layer is consistent with an additional surface-sublimation imprint superimposed on this internal redistribution. Because pore-vapour isotopic composition, internal snow temperatures, and local vapour fluxes were not measured, the specific pathway responsible for the vertical isotope contrast cannot be uniquely determined.

The exposed isotope response was also smaller in high-airflow EXP10 than in zero-airflow EXP9 despite greater net mass loss in EXP10. EXP10 was somewhat more humid than EXP9 (66.9 versus 56.9% RH), and its initial snow was denser and less porous (0.322 versus 0.263 g cm^{-3}; porosity 0.649 versus 0.713). We therefore do not treat EXP9 and EXP10 as a strictly controlled airflow pair. Their comparison suggests a possible airflow influence. It also agrees with the EXP1–8 observation that greater sublimation does not necessarily produce greater isotopic modification.

The revised manuscript therefore distinguishes between the bulk responses quantified in EXP1–EXP8 and the depth-resolved process evidence from EXP9–EXP10. The latter demonstrates that vertical isotope structure can develop under experimental conditions, while the specific internal sublimation, vapour-transport, exchange, and deposition pathways are treated as plausible interpretations rather than uniquely demonstrated mechanisms.

The residence time hypothesis is a central component of the manuscript's theoretical interpretation. However, vapor residence time is neither directly measured nor quantitatively modelled. The discussion should therefore use more cautious language when referring to this hypothesis rather than state direct experimental confirmation. This distinction would help to clarify the difference between the observed isotope responses and the proposed physical processes responsible for it.

Vapor residence time was not directly measured or quantitatively modelled in our experiments. The measured result is that high-airflow experiments were generally associated with greater mass loss but smaller mass-normalized $\delta^{18}O$ and $\delta^{2}H$ changes. Shorter vapour residence time and enhanced

atmospheric renewal under high airflow are physically plausible explanations for this pattern, but they cannot be demonstrated directly from our measurements. We have therefore revised the Discussion to present residence-time effects as a process hypothesis rather than an experimentally demonstrated control.

Minor comments:

Methods:

Can you provide the distance between the different sensors to better understand the spatial variability in atmospheric conditions.

The three atmospheric sensors were positioned along the longitudinal centreline of the 140 × 80 × 40 cm wind tunnel at 27.5, 70, and 112.5 cm from the upstream end, respectively. All three sensors were located at the same lateral position (40 cm) and height (20 cm), giving a separation of 42.5 cm between adjacent sensors and 85 cm between the first and third sensors. These sensor positions and distances have now been added to the Methods.

The atmospheric conditions measured at the inlet, midpoint, and outlet are summarized in Fig. 2, while their spatial and temporal variability is shown separately in Fig. S1. In Fig. S1, spatial variability represents the variability among the experiment-mean values of the three tunnel sensor positions, whereas temporal variability represents variability through time during each experiment.

How many snow Petri dishes were placed into the wind tunnel and are they placed parallel to the wind field or not? Did you also run each snow type separately or did you mixed them in each experiment?

In each experiment, 12 Petri dishes were placed simultaneously in the wind tunnel, with four dishes for each of the three snow types. The dishes were arranged in a 3 × 4 configuration on the temperature-controlled base plate. Their snow surfaces were horizontal, and airflow passed over the dish array along the longitudinal axis of the wind tunnel.

Laboratory snow, fresh natural snow, and old natural snow were present together in each of EXP1-8 and were exposed simultaneously to the imposed thermal and airflow regime. The four dishes of each snow type served as sacrificial samples for successive sampling stages.

How did you regulate the isotopic composition of the ambient vapor to have it consistent between each snow type?

The isotopic composition of ambient vapour was neither actively regulated nor measured. However, the wind tunnel was located within a temperature-controlled shipping container and operated as an open-flow system, with air drawn from the common container atmosphere through the tunnel. All three snow types were simultaneously exposed to the same common tunnel/container atmosphere within a given experiment. Nevertheless, local vapour immediately above or within each snow sample could have been modified by snow–atmosphere exchange, so identical local vapour isotopic composition among snow types cannot be verified. The Craig–Gordon modelling has therefore been removed from the revised manuscript.

Why exactly 24 hours and was steady state e.g. in RH achieved?

The experiments were run for approximately 24 h because the cooling-system compressor required defrosting after one day of continuous operation. RH settled into a sustained, relatively stable range after approximately 0.92–2.42 h after the conditions were set. We have clarified this in the Methods.

Your choice of n = 1 for zero-airflow, 0.7 for low airflow, and 0.5 for high-airflow in Craig-Gordon model deserves more justification and appear somewhat empirical.

In the revised manuscript, we have removed the Craig–Gordon modelling section and the associated airflow-dependent n parameterization. This concern therefore no longer applies to the revised analysis.

Results:

Could you provide the initial isotopic composition of the different snow types? If you assume same ambient vapor isotopic composition, this will help the reader to see potential additional effects like absolute isotopic difference between snow and ambient air and whether this has an addition impact on the sublimation effect for the different snow types.

We have added the initial isotopic composition of the three snow types to the revised manuscript. Across EXP1–8, fresh snow had initial $\delta^{18}O$ values of −17.6 to −16.4‰ and $\delta^{2}H$ values of −134 to −126‰; laboratory snow ranged from −7.26 to −7.04‰ for $\delta^{18}O$ and −53.8 to −52.0‰ for $\delta^{2}H$; and old snow ranged from −16.3 to −14.4‰ for $\delta^{18}O$ and −123 to −108‰ for $\delta^{2}H$.

Discussion:

This could be shortened because there is considerable repetition of porosity, structure, ventilation, residence time. The main message becomes diluted.

We have substantially shortened the Discussion. Repeated explanations involving porosity, ventilation, and vapour residence time were consolidated, and the revised text now separates measured relationships from proposed physical mechanisms more clearly.

Specific comments:

Line 64: To ensure technical accuracy, please consider replacing "condensation" with "deposition", as the process describes a direct transition from a gas to a solid phase.

We rephrased the sentence.

Line 119: How did you check that your setup is sealed?

Answered above.

Line 132-133: Can you provide the distance between the different sensors? You are mention in Line 118 that the wind tunnel is 140 cm long but based on Figure 1, I would expect this is between the "Inlet Temperature Control (T and RH Regulation)" and "Fans (Wind Regulation)". The exact distance will help

the reader to better understand the spatial variability in atmospheric conditions.

The three sensors were positioned along the longitudinal centerline of the 140 × 80 × 40 cm wind tunnel at 27.5, 70, and 112.5 cm from the upstream end, respectively. All three sensors were located at the same lateral position (40 cm) and height (20 cm), giving a separation of 42.5 cm between adjacent sensors and 85 cm between the first and third sensors. These sensor positions and distances have now been added to the Methods.

The atmospheric conditions measured at the inlet, midpoint, and outlet are summarized in Fig. 2, while their spatial and temporal variability is shown separately in Fig. S1. In Fig. S1, spatial variability represents the variability among the experiment-mean values of the three tunnel sensor positions, whereas temporal variability represents variability through time during each experiment.

Line 137, 139, 140: How long did you store the snow before using it for the experiments? Could you provide pictures of the snow grains of the different snow structures to visualize the structural difference.

All three snow types were collected or produced before the experimental series and stored between experiments. The experimental series was conducted consecutively, so the storage duration increased across the sequence of experiments.

Representative photographs of the snow grains are not available for all three snow types.

Line 146: How did you levelled the snow surface? Could you elaborate whether this process could have a significant impact on your e.g. ventilation process? I would assume that this level process will significant change the surface snow properties and inhibit the ventilation process and therefore the isotopic exchange.

Snow was gently placed into the Petri dishes, and the surfaces were carefully levelled with a soft brush to ensure uniform exposure to the airflow. No deliberate compression or compaction was applied. Nevertheless, even gentle handling and levelling may alter the near-surface microstructure, with effects that may differ among snow types. These changes were not directly measured, therefore we cannot completely exclude their contribution to the observed isotopic differences. We have therefore added this limitation to the Discussion and clarified that sample preparation may introduce additional uncertainty into the interpretation of snow-type effects.

Line 161: Why 24 hours and did you reach steady state e.g. in relative humidity?

The experiments were run for approximately 24 h because the cooling-system compressor required defrosting after one day of continuous operation. RH settled into a sustained, relatively stable range after approximately 0.92–2.42 h after setting the conditions. We have clarified this in the Methods.

Line 174-175: Can you elaborate a bit more what do you mean by "substantially smaller than would be expected"? Are you expecting a slightly warmer base than the base plate because of the polypropylene Petri dishes? What difference would you expect?

the temperature within the snow and at the snow–Petri dish interface was not measured. The polypropylene Petri dish provided thermal insulation between the −1 °C base plate and the snow, so the temperature at the bottom of the snow was expected to be lower than the base-plate temperature and influenced by the surrounding colder air. However, we cannot quantify this temperature difference from our measurements. We have therefore revised the text.

Line 180-182: Can you provide data about the different temperature gradients you had within the snowpack for each experimental run?

Temperature was not measured within the snow samples, so the actual internal temperature gradients cannot be determined. We therefore calculated a nominal temperature gradient from the difference between the −1 °C base-plate temperature and the mean tunnel air temperature across the 2 cm snow depth. Across EXP1–EXP8, the nominal temperature-gradient magnitude ranged from approximately 55 to 963 K m^{-1}. These values represent the imposed thermal boundary conditions and should not be interpreted as measured internal snow temperature gradients. We have now added the nominal temperature gradients to the figure summarizing the experimental conditions and clarified this distinction in the Methods.

Line 183-186: Are these sensor data without any snow samples inside the wind tunnel? Did the temperature and relative humidity decreased/increased along the wind tunnel distance or did it change arbitrary? What was the flow rate? Can you provide a sensitivity analysis on the expected mass loss error based on the temperature and humidity variation? This would help the reader to get an idea about the precision of your measured data.

The temperature and relative-humidity data were recorded during the experiments with the snow samples present in the wind tunnel. We did not observe a consistent increase or decrease from inlet to outlet; the small spatial differences varied among experiments.

The experiments covered three airflow regimes: zero airflow, low airflow, and high airflow, corresponding to bulk tunnel air velocities of approximately 0, 0.7, and 3 m s^{-1}, respectively.

To assess the sensitivity to variability in the atmospheric conditions, we quantified both temporal and spatial variability in temperature and RH for all experiments. Across EXP1–EXP10, tunnel temperature temporal variability ranged from 0.39 to 3.01 °C and spatial variability from 0.10 to 1.19 °C. Tunnel RH temporal variability ranged from 2.70 to 11.78 percentage points and spatial variability from 0.58 to 4.35 percentage points. We have added these results, together with the corresponding container temperature and RH, to the revised figures.

We did not convert these variations into a direct mass-loss error because mass loss was measured gravimetrically and snow-surface temperature and the mass-transfer coefficient were not measured.

Line 230: Can you provide more information why you set n=1, 0.7 and 0.5 for different airflow rate?

We agree that the choice of n values was empirical. In the revised manuscript, we have removed the Craig–Gordon modelling section and the associated airflow-dependent n parameterization. This concern therefore no longer applies to the revised analysis.

Line 276-277: Can you elaborate why you can assume same ambient vapor isotopic composition to all snow types? I would assume that your vapor isotopic composition changed significantly during you experimental campaign because you are not specifically creating a well controlled isotopic vapor stream.

The isotopic composition of ambient vapour was neither actively regulated nor measured. However, the wind tunnel was located within a temperature-controlled shipping container and operated as an open-flow system, with air drawn from the common container atmosphere through the tunnel. Because all three snow types were exposed simultaneously during each experiment, they therefore experienced the same incoming background atmosphere. Nevertheless, local vapour immediately above or within each snow sample could have been modified by snow–atmosphere exchange, so identical local vapour isotopic composition among snow types cannot be verified. The Craig–Gordon modelling has therefore been removed from the revised manuscript.

In the revised manuscript, we have removed the Craig–Gordon modelling and the associated assumption of a prescribed common ambient vapor isotopic composition. We have also clarified that ambient vapor isotopes were not measured and acknowledge this as a limitation when interpreting differences among experiments.

741: Why would you measure pore-space humidity? Previous study showed that the pore-space humidity in snowpack is saturated.

Our intention was to refer to the isotopic composition of pore-space vapor, which was not measured in our experiments. We have clarified this now.

Technical comments:

Line 153: Linespace missing -> "and V is..."

Corrected.

Line 294: There is something missing at the beginning of the sentence.

We have corrected the sentence.

Specific scientific questions

Could specific surface area explain more variance than porosity?

Yes, specific surface area (SSA) could plausibly explain additional, or even greater, variability than porosity because it more directly reflects the ice–vapour interfacial area available for sublimation, deposition, and vapour–ice exchange. However, SSA was not measured systematically in our experiments, so we cannot test its explanatory power relative to porosity.

Could edge effects in Petri dishes influence sublimation?

Yes. The shallow 2 cm snow samples and the Petri-dish walls could influence local airflow, heat transfer, and sublimation near the dish perimeter. These edge effects were not quantified in the present experiments. All snow samples were exposed using identical Petri-dish dimensions and the same experimental configuration, which provides a consistent geometry for comparison among snow

types. We acknowledge that interactions between the dish geometry, airflow, and individual snow structures may still have contributed to the observed variability and have added this as a limitation.

Were isotope measurements corrected for memory effects?

Potential instrumental memory effects were minimized during the isotope analysis by measuring each sample seven times and discarding the first three injections. The reported isotope value was calculated from the remaining four injections. The measurements were also normalized to the VSMOW2 scale using laboratory reference standards. We have clarified this procedure in the Methods.

Did snow density evolve during the 24-hour experiments?

We cannot determine this from our measurements. Snow density and porosity were calculated only from the initial snow mass and Petri-dish volume. During sublimation, both snow mass and sample volume may have changed, and the change in volume was not measured. Estimating density later in the experiment from mass loss alone would therefore require assuming that the sample volume remained constant or that sublimation occurred uniformly across the Petri-dish surface. We cannot verify either assumption. For this reason, we use only the initial density and porosity as measured bulk properties and do not estimate their evolution during the experiments.

Why analyse only beginning and end rather than intermediate evolution?

Intermediate isotope samples were collected during the experiments using a sacrificial-sampling design, with a separate Petri dish removed at each sampling time. In the original submission, we focused on the initial-to-final change because the main objective was to compare the net isotopic modification among snow types and experimental conditions over the full experiment, and we therefore did not include the intermediate measurements in the main analysis.

Following the reviewer's suggestion, we have now included the intermediate isotope data to show the temporal evolution during the experiments. Because each intermediate time point represents a different Petri dish, the sequence may also contain some between-dish variability. We therefore retain the initial-to-final change as the primary metric for quantitative comparison among experiments and snow types, while the intermediate samples provide additional information on how the isotope signal developed through time.

Appendix A. Additional covered–exposed experiments

EXP9 and EXP10 were follow-up process experiments conducted after completion of the main EXP1–EXP8 series to provide additional constraints on internal isotope redistribution and surface atmospheric exchange. At that stage, the remaining available snow consisted of a mixture of fresh and laboratory-produced snow rather than a single defined snow type. Consequently, EXP9 and EXP10 were not part of the original snow-type comparison and were analysed separately from EXP1–8. They were not included in the original submission but are included here because they provide an additional process constraint on the mechanisms discussed in the revised manuscript.

For each experiment, six Petri dishes were prepared from the mixed snow. Three dishes were left exposed to the wind-tunnel atmosphere, while three were covered with Petri-dish lids. The lids were not airtight and therefore strongly reduced, but did not completely prevent, vapour exchange with the overlying air. EXP9 was conducted under zero airflow, whereas EXP10 was conducted under high airflow.

At the end of each experiment, the remaining snow in each Petri dish was sampled separately from the upper 1 cm and lower 1 cm of the ~ 2 cm snow layer. These depth-resolved samples were analysed for $\delta^{18}O$ and $\delta^{2}H$, with d-excess calculated from the measured isotope values. Changes were evaluated relative to the initial isotopic composition of the mixed snow used in each experiment. This comparison assumes that the snow was vertically isotopically homogeneous at the start of EXP9 and EXP10.

Both experiments were subjected to very strong imposed thermal forcing. he temperature-controlled aluminium base plate was maintained near −1 °C while the overlying tunnel air was much colder, giving boundary-gradient proxies of ~920 K m^{-1} in EXP9 and ~1000 K m^{-1} in EXP10. These values describe the imposed base–air thermal forcing and should not be interpreted as measured internal snow-temperature gradients.

The covered–exposed design was used to assess whether isotope redistribution within the snow persisted when direct atmospheric sublimation was strongly reduced. Covering decreased net mass loss by more than 90% in both experiments. Despite this reduction, isotopic differences developed between the top and bottom 1 cm of the covered snow. In contrast, the exposed samples showed clear enrichment of $\delta^{18}O$ and $\delta^{2}H$ in the top 1 cm together with lower d-excess. The isotope response in the exposed top layer was stronger in zero-airflow EXP9 than in high-airflow EXP10, despite greater mass loss in EXP10.

These experiments therefore provide an additional constraint consistent with internal isotope redistribution persisting when atmospheric mass loss is suppressed, while the additional enrichment in exposed samples is consistent with a surface-sublimation imprint superimposed on internal redistribution.

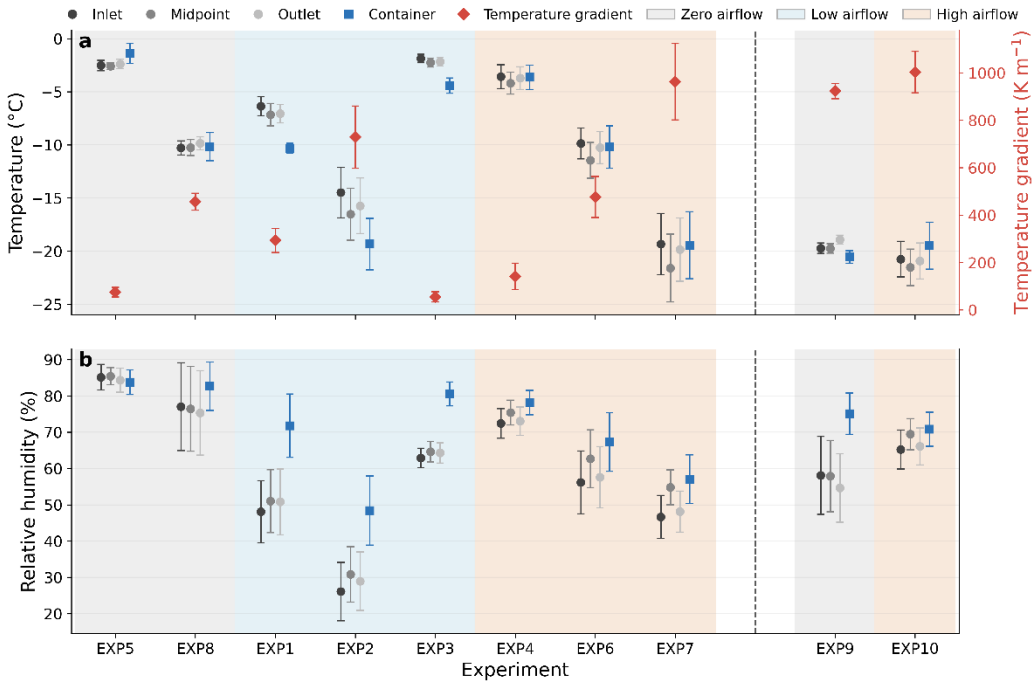


Figure R1. Atmospheric and thermal conditions during EXP1–EXP10. The upper panel shows mean air temperature measured at the wind-tunnel inlet, midpoint, and outlet, together with the mean temperature in the surrounding container. The lower panel shows the corresponding relative humidity. Error bars indicate ±1 SD of temporal variability over each experiment. Background shading groups experiments by airflow regime: zero airflow (0 m/s), low airflow (0.7 m/s), and high airflow (~3 m/s). Red diamonds in the upper panel show the nominal temperature-gradient magnitude calculated from the difference between the −1 °C base-plate temperature and mean tunnel air temperature across the 2 cm snow depth; error bars reflect combined spatial and temporal variability in tunnel air temperature.

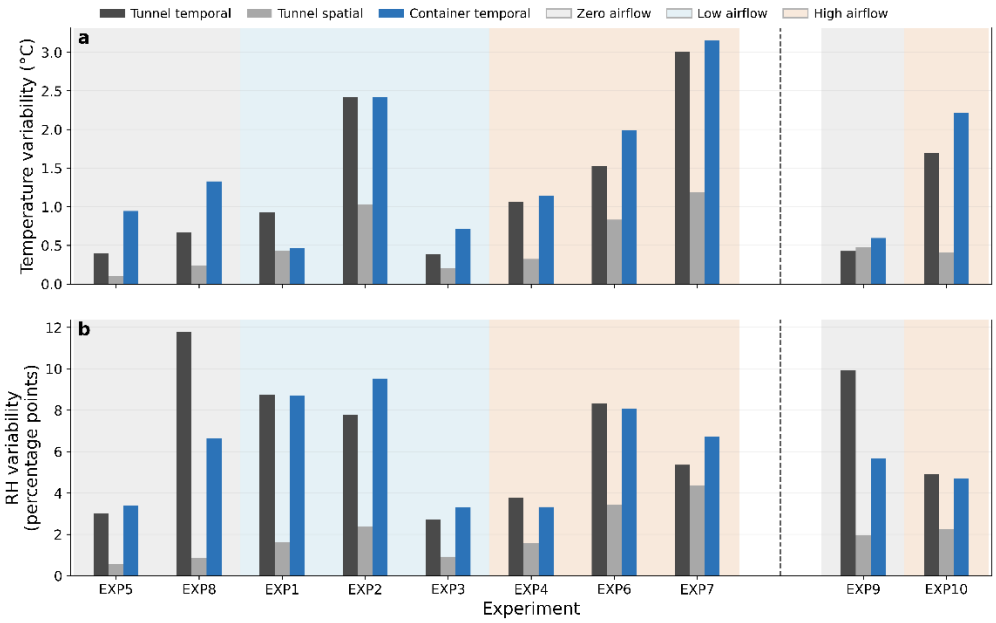


Figure R2 Spatial and temporal variability in temperature and relative humidity during EXP1–EXP10. The upper panel shows temperature variability, and the lower panel shows relative-humidity variability. Tunnel temporal variability was calculated as the standard deviation through time of the mean of the inlet, midpoint, and outlet sensors. Tunnel spatial variability was calculated as the standard deviation among the experiment-mean values of the three tunnel sensor positions. Container temporal variability represents the standard deviation through time measured by the single sensor in the surrounding container; spatial variability is therefore not available for the container. Background shading groups the experiments by airflow regime.

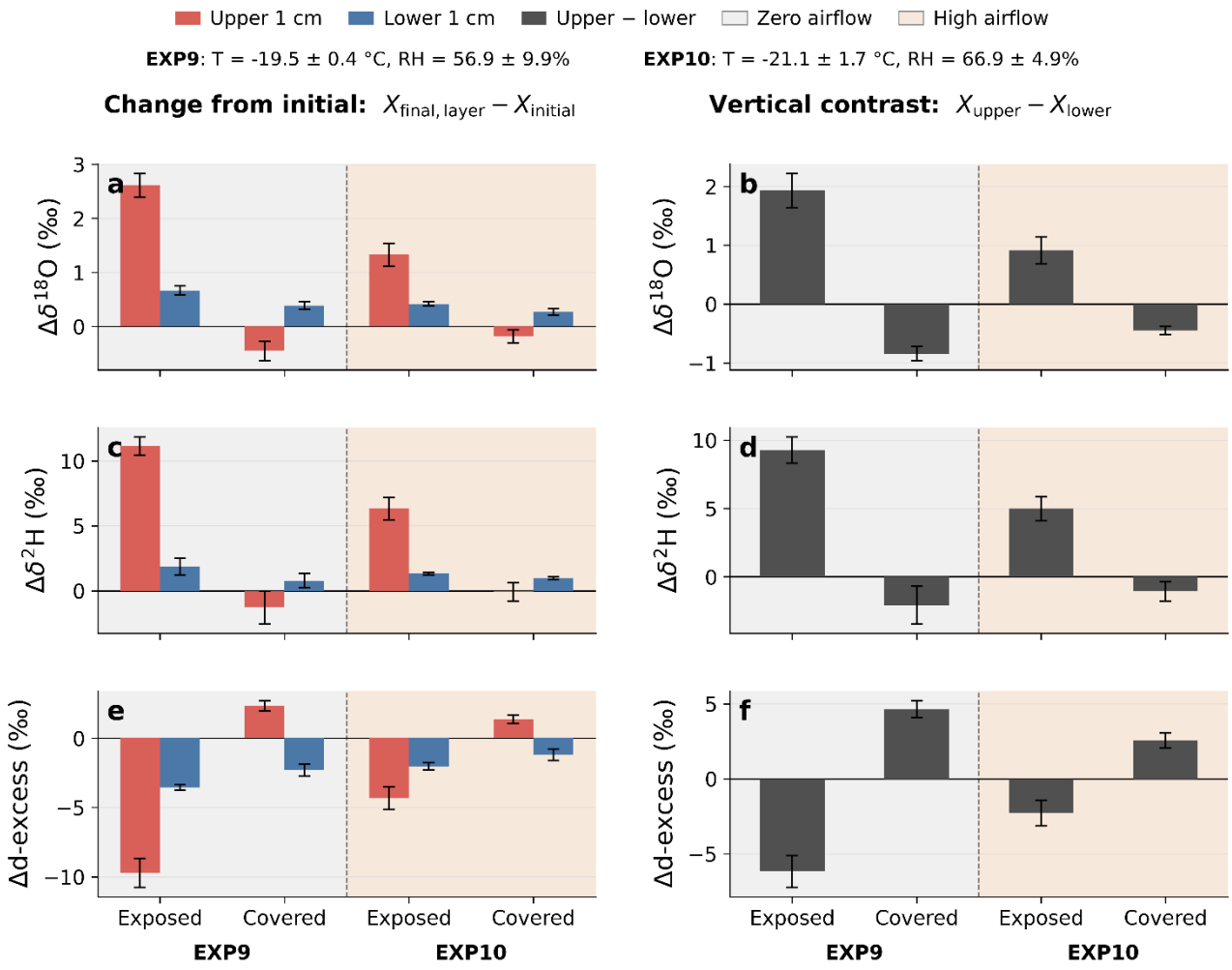


Figure R3 Depth-resolved isotopic changes in the process experiments EXP9 and EXP10. Panels (a, c, e) show changes in $\delta^{18}O$, $\delta^{2}H$, and d-excess, respectively, in the upper and lower 1 cm of the snow relative to the measured initial isotopic composition. Panels (b, d, f) show the corresponding final vertical isotope contrasts. Error bars indicate ±1 SD among three replicate samples. Background shading indicates the imposed airflow regime: zero airflow in EXP9 (grey) and high airflow in EXP10 (light peach). Mean tunnel conditions were −19.5 ± 0.4 °C and 56.9 ± 9.9% RH in EXP9 and −21.1 ± 1.7 °C and 66.9 ± 4.9% RH in EXP10; uncertainties indicate temporal ±1 SD. The same measured initial isotope composition was used as the reference for the upper and lower layers, assuming that the prepared snow was initially isotopically homogeneous with depth.

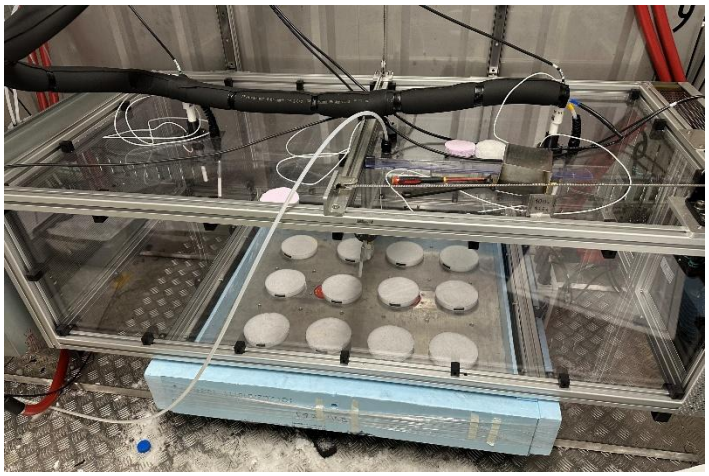


Figure R4 Photograph of the wind-tunnel setup before initiation of an experiment. Twelve Petri dishes (four per snow type) were arranged on the temperature-controlled aluminium base plate inside the wind tunnel. The dishes were filled shortly before each experiment after the target thermal conditions had been established.