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.

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.

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.

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.

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.

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 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.

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.

The residence time hypothesis is a central component of the manuscript's theoretical interpretation. However, vapor residence time is neither directly measured nor quantitatively modeled. 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.

Minor comments:

Methods:

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

- 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?
- How did you regulate the isotopic composition of the ambient vapor to have it consistent between each snow type?
- Why exactly 24 hours and was steady state e.g. in RH achieved?
- 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.

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.

Discussion:

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

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

- Line 161: Why 24 hours and did you reach steady state e.g. in relative humidity?
- 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?
- Line 180-182: Can you provide data about the different temperature gradients you had within the snowpack for each experimental run?
- 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.
- Line 230: Can you provide more information why you set n=1, 0.7 and 0.5 for different airflow rate?
- 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.
- 741: Why would you measure pore-space humidity? Previous study showed that the pore-space humidity in snowpack is saturated.

Technical comments:

- Line 153: Linespace missing -> "and V is..."
- Line 294: There is something missing at the beginning of the sentence.

Specific scientific questions

- Could specific surface area explain more variance than porosity?
- Could edge effects in Petri dishes influence sublimation?
- Were isotope measurements corrected for memory effects?
- Did snow density evolve during the 24-hour experiments?
- Why analyse only beginning and end rather than intermediate evolution?