https://egusphere.copernicus.org/preprints/egusphere-2024-745#RC1

This novel study employs ring wind tunnel experiments combined with stable water isotope analysis to investigate airborne snow particle metamorphism. A main finding is that vapour deposition drives snow particle growth and rounding supported by the observed isotopic fractionation and concurrent SSA decrease. It is inferred that particles and air inside the saltation layer are not in thermal equilibrium as is commonly assumed in blowing snow models. Any mechanical particle fragmentation or coalescence likely play a smaller role in the observed particle size changes as they would not induce isotope fractionation. In turn, the water stable isotopic fractionation induced by airborne snow metamorphism needs to be taken into account when extracting climate information from ice cores, especially at dry and windy locations.

We thank the reviewer for the detailed feedback and are delighted about the short, yet accurate and precise summary of our manuscript which agrees very well with our own perception of the main take-home messages of this study. We address the reviewer's comments individually and in detail below (answes in green). In addition to edits based on the reviewers' comments, we updated a few inconsistencies in the text and figures, such as the color code in Fig. 3 to be consistent throughout the manuscript. In summary, the major changes made are related to:
1) New Fig. 5 to describe the co-evolution of d18O and dD during and after snow introduction in more detail
2) the statistics of observed isotope changes in vapour and snow. We included a table (Table 2) to group the information and declutter the corresponding Sec 3.2
3) A short paragraph in the introduction to define temperature-gradient and isothermal snow metamorphism

General Comments

This is a carefully designed laboratory experiment, with sound methods and data analysis, and with some interesting conclusions, and should published after addressing minor comments listed below.

While this may be common knowledge a very brief description of isothermal versus temperature gradient snow metamorphism as relevant to this study is warranted in the introduction.
We followed the reviewer's recommendation and have included the following paragraph in the introduction: L.62: *"In this context, Walter et al., (2023) first introduced the term "airborne snow metamorphism" which summarises the multiple cycles of sublimation and vapour deposition on the suspended snow particle resulting in modifications of the snow particle size and shape during aeolian particle transport in analogy to metamorphism inside a stationary snowpack (Pinzer et al., 2012; Schleef et al., 2014b). The term snow metamorphism describes the recrystallization of snow grains*

in a snowpack that is driven by vapour pressure gradients (Colbeck, 1982). Snow metamorphism typically results in a decrease in SSA (growing of snow grains) and can be associated with density changes in a snowpack (Jafari et al., 2020; Kaempfer and Schneebeli, 2007). Based on the temperature regime of the snowpack a distinction can be made between isothermal and temperature-gradient metamorphism. The dominant physical processes creating the vapour pressure gradients are distinct: Under isothermal conditions, the microscale curvature effects (Kelvin equation) drive metamorphism (Colbeck, 1980), which are outweighed by macroscale temperature-gradient effects (Clausius-Clapeyron equation) in snowpacks with a temperature gradient (Marbouty, 1980). Temperature-gradient metamorphism typically results in higher recrystallization rates and thus faster SSA decay in snowpacks (Taillandier et al., 2007).

Thus, airborne snow metamorphism was proposed as a driving factor for PPP changes during aeolian transport of snow, yet the relative importance of the different processes involved and their combined effect on the snow microstructure is still unknown due to missing observations."

In particular to clarify the statement that a particle-air temperature gradient must exist to explain depositional particle growth and isotopic fractionation.
The alternative would be vapour fluxes (sublimation/deposition) across an individual particle but also between particles driven by the curvature (Kelvin) effect resulting in local water vapour pressure gradients and super(or sub)-saturation. These fluxes occur at thermal equilibrium and may also induce isotopic fractionation between solid and the remaining vapour phase. I may be convinced that the bulk isotopic composition of snow remains constant but some further discussion is warranted.

We fully agree with the reviewer that microscale curvature-driven pressure gradients exist which control local snow sublimation and vapor deposition that result in grain growth and grain rounding also under thermal equilibrium. In fact, we mention that analogy to our observations in L. 476: *"Notably, the evolution of sphere size distribution resembles the evolution observed during isothermal metamorphism (Legagneux and Domine, 2005; Flin et al., 2004)".* Yet, the time scales for such isothermal metamorphism are in the range of days while the observed airborne metamorphism happens within minutes to hours.
To clarify this important distinction, we have modified L. 559 which now reads: "*As discussed earlier, this condition of a particle-air temperature gradient that leads to supersaturation with respect to the particle surface is a requirement for the growth of particles through vapour deposition **on such short timescales** and the changes in $\delta^{18}O$ support this theory.*"
Regarding the necessity for a temperature-disequilibrium for isotopic fractionation, we also agree with the reviewer that isotopic fractionation, and thus potentially a noticeable change in snow isotopic composition can also happen under thermal equilibrium (driven by the Kelvin effect). The considerations to explain the changes

in snow $\Delta\delta$D (Fig. 6b) assume such equilibrium conditions. However, these considerations can not explain the observed $\Delta\delta^{18}$O. Thus, it was our intention to highlight in Discussion Section 4.2, that the specific change in bulk δ^{18}O that we observe in 4 experiments (negative $\Delta\delta^{18}$O) can ONLY be explained through kinetic fractionation under net supersaturation conditions (L. 558 and Fig. 6a). We then translate the necessary supersaturation value into a temperature difference between snow and air and obtain reasonable values that also agree with modeling estimates of Sigmund et al.. Based on these considerations we feel confident to postulate a snow-air temperature disequilibrium for larger snow particles. Smaller snow particles instead, may thermally equilibrate faster and then sublimate due to higher surface curvatures (convexities). Therefore, we argue that smaller particles disappear at the expense of the larger particles that grow resulting in the observed shift of the sphere size distribution shown in Fig. 2. The schematic in Fig. 7 is intended to summarize these processes. The corresponding text is: L. 491*:*
"The growing and rounding of the particles may thus be explained by airborne metamorphic growth with vapour being preferentially sublimated from convex sub-grain boundaries and entire small grains and preferentially deposited in concavities or on larger grains with lower curvature (Kelvin effect) in analogy to isothermal metamorphism in a snowpack (Colbeck, 1998, 2001; Wakai et al., 2005)."

To do this I'd suggest to better illustrate the temporal co-evolution of the stable H2O isotopes in both snow and also water vapour. E.g. add a similar figure as Fig.3 showing d18O, d2H and d-exx in the vapour phase. Some of the behaviour seen in experiment No.9 (Fig.4) is puzzling, e.g. O18 in vapour and snow shows correlation, whereas 2H shows anti-correlation (significant?). Was this behaviour observed also in other experiments and is this related to the mentioned non-equilibrium conditions?
We agree that the co-evolution of snow and vapour in each individual experiment is interesting but we had to synthesize the results to be able to draw general conclusions and to maintain a reasonable manuscript length. Figure 4 was intended to provide a visualization of the co-evolution to the reader as a trade-off. We decided to follow your suggestion (that was also mentioned by reviewer 2, comment 15) and we now incorporate a figure (Fig 5) to additionally visualise the co-evolution (net change) of the vapour isotopic composition vs. the change in snow isotopes. We color-coded the markers with the average experiment temperature to allow a good comparison to Fig 3.

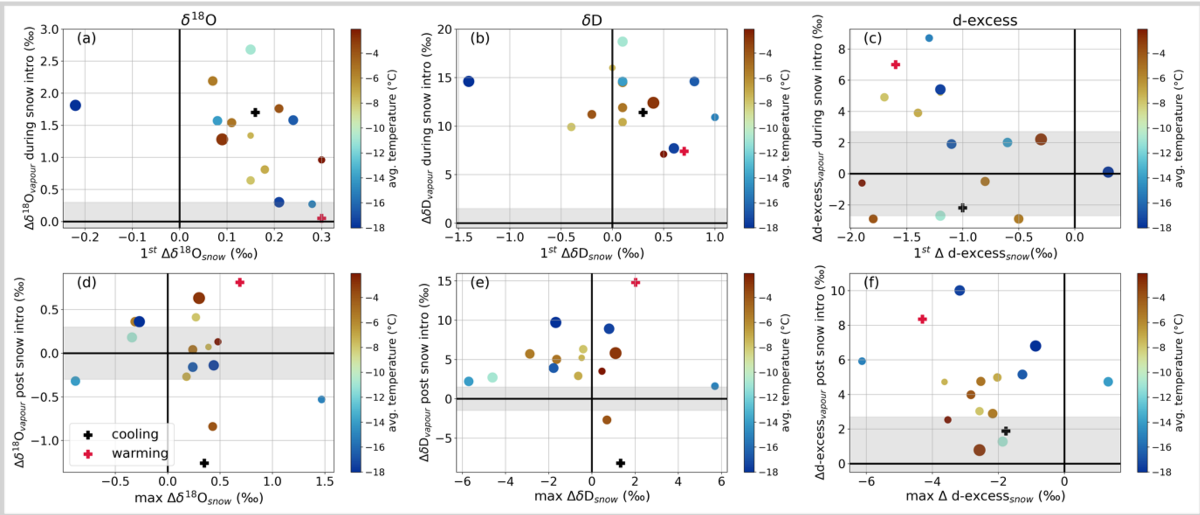

Figure 1 The co-evolution of changes in vapour and snow isotopes in all experiments with vapour isotope observations The change in the snow isotopes (x-axes) is plotted against the changes in the vapour isotopes (y-axes). The changes in vapour isotopes are calculated from the 3-min averaged data. The upper row (a, b, c) shows changes in vapour isotopes during the snow introduction plotted against the observed change in snow isotopes of the first airborne sample (see Fig. 3). The lower row (d, e, f) shows the subsequent change in vapour isotopes post snow introduction until the end of the experiment plotted against the maximum observed change in the snow isotopic composition. Shaded areas represent low variability in vapour isotopic composition (|Δδ18O| <0.3 ‰, |ΔδD| <1.5 ‰, (|Δd-excess| <2.7 ‰). Note that the colour code represents the average air temperature during the experiments and allows the comparison between upper and lower row and the results shown in Fig. 3. The cooling (black cross) and warming (red cross) experiments are exempt.

As the reviewer points out, the two isotope species ($\delta^{18}O$ and δD) do not necessarily show the same temporal evolution and trends. This is intriguing and explainable through the unique combination of vapour and snow isotopic composition and the resulting isotopic disequilibrium in our experiments. As we make snow from local tap water we create specific snow-vapor isotopic combinations that are not identical for the different experiments and not necessarily often observed in nature. However, the strong disequilibrium conditions allow us to draw conclusions, such as the existence of a snow-air temperature gradient, that would otherwise not be discernible. To better explain this to the reader we have modified L. 686ff which now reads: *"The experiments revealed that the isotopic signature of the wind-blown snow event in the wind tunnel was dependent on the combination of initial vapour and snow isotopic composition and the resulting disequilibrium. As we produced snow from tap water this disequilibrium was at times asymmetric between the two isotope species and more pronounced than generally assumed in nature. As the disequilibrium determines the isotopic evolution, it is not possible to unambiguously predict the expected changes in the snow isotope signal under wind influence without considering the water vapour isotope variability imposed by synoptic-scale atmospheric transport (Aemisegger et al., 2022; Bagheri Dastgerdi et al., 2021)."*

Detailed Comments

L145 - Mention here what temp was the wind tunnel set to?

Since the experiments were conducted at varying temperature regimes we decided to reference Table 1 at this point. The sentence now reads:

L158: "Care was taken to equilibrate the snow temperature to the target wind tunnel air temperature (Table 1) for 30–60 min before the start of the experiment."

Table 1: Clarify in the caption that DELTA T means change in mean wind tunnel T over the duration of the experiment
We changed that as recommended.

L182 - cm3
We assume the reviewer suggests adding information about the volume of the wind tunnel. We added it in L. 212: *"The wind tunnel has an air volume of ~ 0.5 m^3 and was sealed against snow loss with the help of insulation material."*

L337 - Be specific: significant enrichment by how many permil?
We added the statistics about the observed significant changes in a new table (Table 2.)

L395 - In order to illustrate the concurrent vapour isotopic composition change across all experiments I suggest a similar figure as Fig3. (see above)
We included a new figure and made that section more concise. Fig.5 shows the change in vapor isotopes during the snow introduction (upper row) and post snow introduction (lower row) plotted against the corresponding snow changes.

L431 - Shouldn't mechanic fragmentation lead to a SSA increase if it was the dominating process?
The reviewer raises a valid point. Depending on how much "new surface area" is created through the breaking of the crystal, the SSA value can increase if the volume stays the same. However, previous literature has claimed that mechanic fragmentation leads to lower SSA (Comola et al., 2017) which is conceptually wrong. We therefore included that statement but will adapt it to your suggestion now: L.457: *"Note here, that simple mechanic fragmentation of snow particles alone does conceptually lead to an increase in sample SSA and can thus not explain the decrease in SSA."*

L441 - Please explain "higher SSA decay rates for isothermal snowpack metamorphism", how much higher? Higher than T-gradient metamorphism? reference?
We realise that this sentence was poorly phrased and thus wrongly understood. It was supposed to reference the well known temperature-SSA decay rate dependency in isothermal and temperature-gradient conditions. We did not observe such temperature dependency in our windtunnel experiments. The sentence is rewritten and references are added to L. 464: *"The driving processes for snow metamorphism are vapour pressure gradients, which are largely governed by the (absolute) temperature regime in a stationary snowpack (Kaempfer and Schneebeli, 2007; Taillandier et al.,*

2007). In airborne snow metamorphism however, other processes such as turbulent mixing of air and vapour, particle-air temperature gradients (see Sec. 4.2) and variability in saturation conditions might be dominating vapour pressure variability and therefore masking the simple absolute temperature dependency that is expected in stationary snowpack metamorphism."

L448 - Are particles in the saltation layer subject to a different metamorphism regime than those in the suspension layer? Please expand & add any relevant reference
The reviewer raises an interesting question. However, with the current experimental set-up and the available observations we don't think we can provide an adequate answer that is based on more than speculations. Since we sampled snow from the whole height of the air column our observations integrate the saltation and suspension layer. Because of the generally close-to-saturation levels and the transport of snow particles in general proximity to the outer wall or the floor, we concluded that our wind tunnel experiments are probably more likely to represent saltation layer regimes (see Section 4.3). We are not aware of other studies that have performed research on the concept of airborne snow metamorphism in suspension or saltation layer and can thus not provide relevant references.

L465 - What about the vapour flux between particles, i.e. sublimation of small snow particles, which may eventually disappear, followed by deposition to larger particles. See comment above.
We agree that we should mention this process specifically at this point. We discuss it already in L 717-718 and the process is also included in the schematic Fig 7. Following the reviewer's suggestion we added to L. 491: "The growing and rounding of the particles may thus be explained by airborne metamorphic growth with vapour being preferentially sublimated from convex sub-grain boundaries **and entire small grains** and preferentially deposited in concavities **or on larger grains with lower curvature (Kelvin effect)** in analogy to isothermal metamorphism in a snowpack (Colbeck, 1998, 2001; Wakai et al., 2005)."

L528 - Except that particle-to-particle vapour flux can occur also at T-gradient = 0 and RH_ice = 1 due to Kelvin effect (equivalent to isothermal metamorphism I think)
This is in relation to the reviewer's general comment which we have answered in detail above. We want to emphasize that the observed $\Delta\delta^{18}O$ changes can NOT be explained with isotopic equilibrium fractionation (i.e. RH=1).

Fig.6 - yes, this is related to the curvature (Kelvin) effect
We added an explanation of the Kelvin/curvature effect in the introduction and also in response to your comment above to L. 491, so it is easier now for the reader to understand Fig. 7 (previously Fig 6).

Conclusions - list here and possibly in the abstract the order of magnitude of the observed isotope fractionation attributed to airborne snow metamorphism in permil, a result relevant for the interpretation of field data.

Thank you for this valuable feedback. We added this sentence to the Conclusion L. 724: "*The change in the snow isotope signal that we attribute to airborne snow metamorphism was dependent on the vapour-snow disequilibrium and ranged from: -0.88 ‰ – +1.47 ‰ in δ18O, -5.7 ‰ – +5.7 ‰ in δD and -6.1 ‰ – +1.3 ‰ in d-excess.*" And this sentence in the abstract: L. 24 "*Within transport times of 3 hours, we observed changes in the isotope signal of airborne snow of up to: +1.47 ‰ in δ18O, ± 5.7 ‰ in δD and -6.1 ‰ in d-excess.*"

References:

Aemisegger, F., Trachsel, J., Sadowski, Y., Eichler, A., Lehning, M., Avak, S., and Schneebeli, M.: Fingerprints of Frontal Passages and Post-Depositional Effects in the Stable Water Isotope Signal of Seasonal Alpine Snow, JGR Atmospheres, 127, e2022JD037469, https://doi.org/10.1029/2022JD037469, 2022.

Bagheri Dastgerdi, S., Behrens, M., Bonne, J.-L., Hörhold, M., Lohmann, G., Schlosser, E., and Werner, M.: Continuous monitoring of surface water vapour isotopic compositions at Neumayer Station III, East Antarctica, The Cryosphere, 15, 4745–4767, https://doi.org/10.5194/tc-15-4745-2021, 2021.

Colbeck, S. C.: An overview of seasonal snow metamorphism, Reviews of Geophysics, 20, 45–61, https://doi.org/10.1029/RG020i001p00045, 1982.

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Jafari, M., Gouttevin, I., Couttet, M., Wever, N., Michel, A., Sharma, V., Rossmann, L., Maass, N., Nicolaus, M., and Lehning, M.: The Impact of Diffusive Water Vapor Transport on Snow Profiles in Deep and Shallow Snow Covers and on Sea Ice, Front. Earth Sci., 8, 249, https://doi.org/10.3389/feart.2020.00249, 2020.

Kaempfer, T. U. and Schneebeli, M.: Observation of isothermal metamorphism of new snow and interpretation as a sintering process, J. Geophys. Res., 112, 2007JD009047, https://doi.org/10.1029/2007JD009047, 2007.

Pinzer, B. R., Schneebeli, M., and Kaempfer, T. U.: Vapor flux and recrystallization during dry snow metamorphism under a steady temperature gradient as observed by time-lapse micro-tomography, The Cryosphere, 6, 1141–1155, https://doi.org/10.5194/tc-6-1141-2012, 2012.

Schleef, S., Löwe, H., and Schneebeli, M.: Influence of stress, temperature and crystal morphology on isothermal densification and specific surface area decrease of new snow, The Cryosphere, 8, 1825–1838, https://doi.org/10.5194/tc-8-1825-2014, 2014.

Walter, B., Weigel, H., Wahl, S., and Löwe, H.: Wind tunnel experiments to quantify the effect of aeolian snow transport on the surface snow microstructure, Snow/Snow Physics, https://doi.org/10.5194/tc-2023-112, 2023.

https://doi.org/10.5194/egusphere-2024-745-RC2

Dear authors and editors,

thank you for the opportunity to review this interesting study. The work presents a laboratory experiment exploring snow processes during aeolian transport using a ring-shaped wind tunnel. The experiment leverages two technologies, microCT scanning to determine the change in physical properties of the snow grain during transport, and stable water isotope analysis of both the transported snow and the air water vapor to study the physical processes in phase change, i.e. sublimation and deposition during transport. They find that grain snow SSA decreases and grain size increases, and snow particles experience longer time of wind transport. The isotope data suggest both net sublimation in the early stage of the experiment, and vapor deposition on the transported snow particles.

The work is an innovative way to study the changes in snow during wind transport, which is difficult to do in-situ in field conditions. The work used a unique combination of microCT scanning and stable water isotope measurements, allowing novel insights to micro scale processes. I think the experiment is carefully planned and executed, and the findings have implications for both snow physics research, and research using stable water isotopes as tracers of paleoclimate or hydrology in snow-influenced regions. I recommend the work to be published, after addressing my comments below.

We thank the reviewer for the time that was spent on this review which helped to improve this manuscript. We detailed our changes in reply to the comments below (answers in green). In addition to edits based on the reviewers' comments, we updated a few inconsistencies in the text and figures, such as the color code in Fig. 3 to be consistent throughout the manuscript. In summary the major changes made are related to:
1) New Fig. 5 to describe the co-evolution of d18O and dD during and after snow introduction in more detail and an adjustment of the vapour isotope change results section 3.2.2
2) the statistics of observed isotope changes in vapour and snow. We included a table (Table 2) to group the information and declutter section 3.2
3) A short paragraph in the introduction to define temperature-gradient and isothermal snow metamorphism

L32: Cite some large and small scale wind drift modeling studies.
L. 31: We added the following citations: (Agosta et al., 2019; Groot Zwaaftink et al., 2011; Lenaerts et al., 2012).

L55: start new paragraph
We followed the reviewer's suggestion.

L85: also melt and freeze
We added melt and freeze fractionation, the sentence now reads: L. 98: "*Due to their predictable partitioning, i.e. fractionation into the vapour, liquid and ice phase during phase changes (i.e. water vapour deposition and sublimation or freeze and melt), stable*

water isotopes are powerful tracers (Ala-aho et al., 2021; Beria et al., 2018; Galewsky et al., 2016), which allow to identify and quantify the impact of metamorphic processes on the PPP (Ebner et al., 2017; Harris Stuart et al., 2023). Ideally, stable water isotope measurements comprise all involved phases (in dry snow regions this means solid and gas) to fully constrain the isotopic fractionation during phase change processes."

L89: suggest to cite papers that use water isotopes as tracers in snow studies.
We followed the reviewer's suggestion and added these citations instead of the Mook-fundamentals citation: (Ala-aho et al., 2021; Beria et al., 2018; Ebner, 2012; Galewsky et al., 2016; Harris Stuart et al., 2023) to the sentence.

L104: in my understanding the d18O_ice < d18O_vapor in any deposition process, not only supersaturated, fractionation factor in Eq (2) take values >1.
The reviewer is wrong in this regard. Generally, d18O_ice > d18O_vap (remember that delta values are negative numbers given in ‰) which is the case when alpha_net > 1 with alpha_net=R_{snow}/R_{vapour}. The equilibrium fractionation factor will always be alpha_eq > 1 due to vapour pressure differences for the different isotope species. However, the kinetic fractionation factor is <1 (the higher the supersaturation, the smaller alpha_k). Given that the relative importance of equilibrium and non-equilibrium fractionation is determined by the supersaturation, there is a critical supersaturation value above which, alpha_net in eq. (2) can become <1 which then leads to d18O_ice < d18O_vap. See Mook, (2000) Sec. 3.3. p. 27 or (Jouzel and Merlivat, 1984) Fig 9.

L135: changes in what?
We changed the sentence structure to avoid confusion. It now reads L. 147: *"The experiments aimed to simulate wind-blown snow transport in the ring wind tunnel for long transport times while monitoring changes in snow properties and environmental variables to identify the corresponding governing mechanisms."*

L140: tab -> tap?
That was a typo, yes. We corrected it.

L140-141: Don't get this – how it was not in operation, but was producing new snow minimum four days before the experiment (L144)?
We added more information to this sentence in the hope that it is clear now. L. 153: *"The snowmaker was situated in the same cold-laboratory but the snowmaker and the wind tunnel were not operated simultaneously to limit heat and moisture sources during the experiment hours."*

L143: did you analyze the influence of storage time? And how to determine mixing success? If not, suggest to remove the start of this sentence.
We think the reviewer refers to L 148 and we removed that first half of the sentence as the standard deviation measurements are indeed the relevant information to define mixing success.

L170: out of interest: what was the mass balance of your 600g of added snow? x g sampled, y g deposited, the residual z g sublimated? Maybe not very important for your results and findings, but this would give an idea where did the snow in the air column end up, and allow better imagining the experimental setup?
The reviewer proposes a very important metric which is essential for modeling efforts.

We tried to monitor the mass balance components as closely as possible but it became apparent that the uncertainties in the accumulated snow mass was too high to establish a reliable mass balance. These uncertainties were due to snow getting stuck in cracks and screw boreholes etc which we had to clean with vacuum cleaners and brushes. In future experimental set-ups our goal will be to find a way to minimize the uncertainties in the accumulated snow mass with the hope that we will be able to perform mass balance calculations and sublimation modeling.

Table 1: please specify the variables, not only units for the DeltaH2O column. Also, what does DeltaT mean?
We included the humidity variables in the table header and added an explanation of DeltaT to the table caption.

section 2.2.1: a carefully though of experimental equipment and setup: trying hard to think of points of criticism but cannot find any😊
Thank you!

L228: what is the difference specifically between drifting and blowing snow in your experiment?
Essentially in our experiment, we can not differentiate between drifting and blowing snow as we take one integrating sample from the whole air column. In general, as we argue in L. 627ff, the wind tunnel might be more representative of saltation layer conditions.

L371-375: cannot locate this data in your plots
As this is not an essential part of the experiment but rather a way to check our assumptions we decided that we don't show these data. We clarified this in the sentence L. 385: *"The snow samples taken from the accumulated snow (not shown) show a clear difference between the samples from the inner part and the surface layer of the accumulated snow."*

L395-406: this difficult to follow. First you stare that most experiments follow example in Fig. 4. On L402 you talk about subsequent evolution, subsequent to snow addition? to which of the categories does the example in Fig.4 belong to, for example? Not sure what is the best way to summarize this data, perhaps in a table, but the current way is difficult for me to digest.
This point was also raised by reviewer 1 (specific comment 5). Thus, we included a new figure (Fig 5) that shows the net vapour isotope change as a way to visualize the co-evolution between snow and vapour isotopic composition. The upper row demonstrates the change in vapour isotopic composition during snow introduction, and the lower row the change in vapour isotopes between end of snow introduction until the end of the experiment. We also simplified the whole section, and included Table 2 with the relevant statistics of the isotope changes and we are confident that the updated version together with the new figure will be better understandable to the reader.

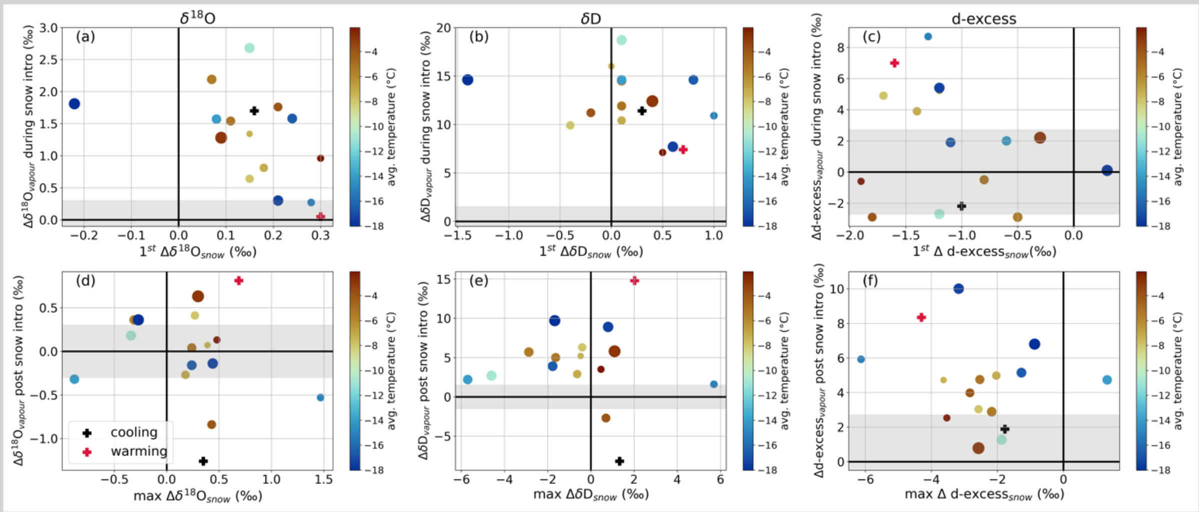

Figure 1 The co-evolution of changes in vapour and snow isotopes in all experiments with vapour isotope observations
The change in the snow isotopes (x-axes) is plotted against the changes in the vapour isotopes (y-axes). The changes in vapour isotopes are calculated from the 3-min averaged data. The upper row (a, b, c) shows changes in vapour isotopes during the snow introduction plotted against the observed change in snow isotopes of the first airborne sample (see Fig. 3). The lower row (d, e, f) shows the subsequent change in vapour isotopes post snow introduction until the end of the experiment plotted against the maximum observed change in the snow isotopic composition. Shaded areas represent low variability in vapour isotopic composition (|Δδ18O| <0.3 ‰, |ΔδD| <1.5 ‰, |Δd-excess| <2.7 ‰). Note that the colour code represents the average air temperature during the experiments and allows the comparison between upper and lower row and the results shown in Fig. 3. The cooling (black cross) and warming (red cross) experiments are exempt.

L404: does the reversed evolution pertain only to 18O?

From the new Fig. 5 it becomes clear that the reversed evolution is more often visible in d18O vs it only happened once for dD. This has to do with the disequilibrium between vapour and snow isotopic composition and we elaborate on this in L. 686: “*As we produced snow from tap water this disequilibrium was at times more pronounced than generally assumed in nature. As the disequilibrium defines the isotopic evolution, it is not possible to unambiguously predict the expected changes in the snow isotope signal under wind influence without considering water vapour isotope variability which is driven by synoptic-scale atmospheric variability (Aemisegger et al., 2022; Bagheri Dastgerdi et al., 2021).*”

Chapter 3.2: I was expecting also the snow samples and their temporal evolution in Fig.4 to be described in the this paragraph.

As suggested, we added two short sentences describing the change in snow isotopes that can be seen in Fig. 4: L. 407: *“The snow sampling events every 15–30 min can be identified as short-lived dips of 1–2 min in the wind speed (Fig. 4d). In this experiment, the first airborne snow sample (1st $\Delta\delta^{*}_{snow}$) shows a 0.18 ‰ enrichment in δ18O (Fig. 4f) and a -1.4 ‰ decrease in d-excess (Fig. 4j) whereas the δD value (Fig. 4h) does not change significantly (+0.1 ‰). The maximum observed change (max $\Delta\delta^{*}_{snow}$) for this experiment is a 0.18 ‰ enrichment in δ18O, a -0.6 ‰ depletion in δD and a -2.0 ‰ decrease in d-excess. The vapour isotopic composition in this experiment showed enrichment in δ18O (0.81 ‰), δD (10.4 ‰) and an increase in d-excess (3.9 ‰) during the snow introduction. After the snow introduction had ended and until the end of the experiment the δ18O signal showed a reverse evolution (-0.27 ‰) while both δD (2.9 ‰) and d-excess (5.0 ‰) continued to increase.”*

L459: start a new paragraph to give rhythm to the section?

We started a new paragraph

L476: What's your view: would the full sublimation of small particles conceptually lead to isotope fractionation in the suspended snow?

This is indeed an interesting question to think about. It would certainly not lead to isotopic *fractionation* in the snow as fractionation requires that there is a leftover compound that collects the remaining isotopes. However, one could discuss whether removing the smallest particles would lead to a change in the bulk isotopic composition if we assumed that the smallest particles have an initially, fundamentally different isotopic composition than the bigger ones. The snowmaker snow is fairly homogenous and we would assume that the smallest particles probably stem from abrasion or breaking of larger particles. Thus, I would not assume that the bulk snow isotopic composition changes much if only the smallest grains would get sublimated and the larger ones stayed inert. In nature, however, I would expect that snow particles of different sizes (formed at different altitude and/or different temperature) probably have a different isotopic composition and a selective removal of only the smallest sizes might change the bulk isotopic composition. But this is only speculation and a question that is difficult to test, because it would require analysing the snow's isotopic composition in bins of different particle sizes. We are not aware of a method to efficiently separate snow of different particle sizes and the usefulness (beyond curiosity satisfaction) is questionable since snow in nature is always a mix of different particle sizes. But a fun thought experiment!

L564: replace "in other words" by this is demonstrated by … or similar. Because you haven't really show the evidence for the statement.

We agree. We replaced "in other words" with "specifically". The sentence now reads:
L. 595: *"Specifically, the initial disequilibrium prior to snow introduction (i.e. vapour produced by fractionating sublimation) is a better predictor for the changes…"*

L582?: can you propose a way to conceptualize the processes you have found into a modeling context, where isotope values in the snowpack are important, such as isotope-enabled climate of hydrological models?

The reviewer is asking the follow up question to this manuscript and we can share that we are continuing to work along this idea to find a way to incorporate our findings in a numerical isotope-enabled model. However, an adequate answer to this question would go beyond this work's scope, but we hope to answer the reviewer's comment in a subsequent manuscript.

L613: start new paragraph

We have started a new paragraph

L659: can you find any field studies that would have observed similar (or any) change in the isotope values of wind-transported snow? Or can you propose an experiment that could study this in the field conditions?

To our knowledge there is no published water isotope dataset that was targeted towards wind-blown snow yet. On the contrary, many studies excluded intense wind periods for their sampling campaigns to limit possible drivers of post-depositional isotope change (Harris Stuart et al., 2023; Wahl et al., 2022). However, a recent study of surface snow at Concordia station in Antarctica (Ollivier et al., 2024) discusses wind as potential driver for post-depositional changes in snow isotopic composition during polar night conditions.

Reviewer 3 (comment 1) was skeptical about the possibility to differentiate between fresh and wind-blown snow based on the d-excess values since the d-excess variability in fresh, precipitated snow is very high in itself. In an effort to combine both comments we changed the paragraph to L. 690: *"However, the results suggest that a strong d-excess decrease can be linked to airborne metamorphism. This should be kept in mind when observations of snow d-excess values are used as hydrological tracers. Further field studies in windy and dry locations such as the katabatic wind zones on the Antarctic Ice Sheet could support this idea."*

L684: You do not have data for this? the conclusions are indirect from processes.
This is correct which is why we used the vague term "indicated" rather than "demonstrated" or "showed". We think that this is an important interpretation of the experimental data and will therefore keep this sentence in the conclusions.

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The review of manuscript "Identifying airborne snow metamorphism with stable water isotopes" by Dr. Sonja Wahl and colleagues.

The manuscript presents and discusses the results of laboratory experiments that simulate blowing snow events. The authors show that the blowing snow particles are modified as a result of "airborne snow metamorphism". The isotopic composition of the snow particles and of the surrounding water vapor is changing as well (due to sublimation and re-sublimation fluxes), although the sign and value of the isotopic transformations differ from one experiment to another.

This study shows that the snow drift before the newly precipitated snow is finally deposited onto the snow surface, is an important part of "post-depositional" processes that alter the initial isotopic content of the precipitation. Thus, this work is an important step towards a deeper understanding the whole complexity of the post-depositional snow evolution, which is crucial for the interpretation of the deep ice core isotopic signal.

We thank the reviewer for this very positive review and have noted our replies to the comments below (answers in green). In addition to edits based on the reviewers' comments, we updated a few inconsistencies in the text and figures, such as the color code in Fig. 3 to be consistent throughout the manuscript. In summary the changes made are related to:
1) New Fig. 5 to describe the co-evolution of d18O and dD during and after snow introduction in more detail and an adjustment of the vapour isotope change results section 3.2.2
2) the statistics of observed isotope changes in vapour and snow. We included a table (Table 2) to group the information and declutter the corresponding text.
3) A short paragraph in the introduction to define temperature-gradient and isothermal snow metamorphism

I have only minor correction to the manuscript:

Lines 658-659 ("Thus, it could be possible to use the snow isotopic composition to differentiate between wind-blown snow and precipitated snow") – firstly, I am not sure why one could need to make such differentiation. Secondly, freshly precipitated snow stays "fresh" not for long time, it is involved to the post-depositional processes immediately after deposition, so its isotopic signature would be modified quickly. Thirdly, in precipitation there is a huge variability of d18O and dxs, as seen from the observation (see data from Concordia station, as an example).

The usefulness for such a distinction tool might not be relevant in the paleoclimate context but rather interesting for deposition patterns in highly-complex terrain in relation to avalanche formation for example. Furthermore, it supports previous studies emphasizing that the attribution of the variability in isotope signals in ice cores to source conditions is not straightforward. However, we agree with the reviewer that our experiments do not reveal a unique isotopic fingerprint that can be used to identify wind-blown snow unambiguously. Thus it might only be useful in conjunction with physical properties parameters as an additional indication for wind-blown snow. We have changed the sentence to: L. 690: *"However, the results suggest that a strong d-excess decrease can be linked to airborne metamorphism. This should be kept in mind when observations of snow d-excess values are used as hydrological tracers."*

Line 215 – ml min^{-1} (put a space between ml and min). The same in line 223.
We added the space in both locations.

Figure 3 – does the grey background in the upper row have any particular meaning? If not, it's better to delete it.
We adapted the figure as suggested by the reviewer and removed the grey background.

Line 481 – do you need the word "explained" here? Suggest to delete it.
As suggested we deleted the additional (explained) from the Section caption.