Dear Editor,

please find below the changes that were made based on the reviewer comments. Small additional changes were made to the manuscript for the benefit of consistency and readability. These changes are not included in this document but are highlighted in the marked-up manuscript version.

With best regards,
Benjamin Walter

RC1: 'Comment on tc-2023-112', Anonymous Referee #1, 15 Sep 2023

1) The authors present a very exciting and compelling experiment focused on the effect of wind snow surface microstructure. For how simple the question is, this is an incredibly hard problem to work on. We have limited tools at our disposal to make concise measurements of snow microstructure, and it is incredibly difficult to run such an experiment in the field. This group at SLF has succeeded at combining their expertise in snow microstructure and wind tunnel experiments to provide new insights into this intriguing aspect of snow metamorphism.

2) My only concern with this research is the significantly unphysical conditions under which snow is being transported. As it stands, I do not see a reason why the rate of change of any snow characteristics in their experiment should be related to any measurements of natural snow undergoing natural transport.

We added the following sentences in Section 5, L511:

"The focus of this study is on linking atmospheric and aeolian snow transport conditions during precipitation events to the snow microstructure of the ultimately deposited snow and to identify the relevant processes. While we show magnitudes of dependencies between different flow and snow parameters, processes and snow microstructures, natural conditions may be different in the field, depending on the snow type or flow conditions, while the latter is also rarely well developed and stationary for natural conditions."

3) It is unclear if the snow particles actually come in contact with the propeller driving their RWT. A schematic that shows this mechanism would be very helpful.

We added the following sentences in Section 2.2, L102:

"The wind turbine propeller is located beneath the lid in the curved section of the ring wind tunnel (Fig. 1a) covering approximately the top quarter of the wind tunnel cross-section. The propeller blades are 90 mm long. As the particle mass flux exponentially decreases with height as shown by Yu et al. (2023) for our RWT, only a negligible part of the snow particles will get into contact with the propeller. A schematic drawing of the RWT and additional figures can be found in Yu et al. (2023)."

4) More importantly, the authors acknowledge that a large portion of snow particles are transported along the outer wall due to centrifugal forces and, among other effects, this causes a measurable impact on density. This is well outside the realm of normal saltation and suspension. Given that v_x is so much larger than v_z, this impact force may be considerably higher than in nature. As well, repeat impacts caused by snow working its way around a corner may cause orders of magnitude more fragmentation.

We added the following sentences at the end of Section 3.4.1, L317:

"In the curved sections, particles are mainly sliding along the RWT outer wall, an effect that is certainly not favourable for simulating natural snow transport but inevitable for a compact closed circuit wind tunnel in a cold laboratory of limited dimensions. The centrifugal forces acting on snow particles in the curved section were estimated being two to three orders of magnitude smaller compared to the forces acting on the snow particles during surface impact while saltating. The maximum impact angles of snow particles impacting into the curved outer wall were calculated being around 25°-30° which are comparable to the impact angles α_{in} on the snow surface in the straight test section (Fig. 4b and d). However, a Stokes number < 0.1 indicates a good flow following behaviour of the snow particles when the air flow gets redirected in the curved section, resulting in smaller impact angles. Based on these results, we conclude that both, centrifugal forces, and particle impacting into the curved side walls have at most a similar but more likely a smaller effect on particle fragmentation than particles impacting on the surface during saltation. Based on these estimates, it can be assumed that particle fragmentation is dominated by the particle impacts on the snow surface in the saltation layer, and that our results are, to a certain degree, comparable to real natural snow transport situations."

Given these concerns, could you please address the question of transport around the curves (impact velocities, momentum balance, fragmentation rate, restitution coeff, how many more impacts per second? etc.), or modify the manuscript in such a way that the reader knows while you may have novel measurements of a physical process, this physical process has little relation to what one may expect to find in nature? As it stands, I think the quantitative information provided needs to be qualified or better justified.

There are a few grammatical things that could be improved:

L8- Cover wind speeds? *We changed this to: "vary wind speeds".*

L11-In the deposit? *We changed this to: "deposited snow".*

L21- Is rolling different from creep? *No: we call it now "rolling or creeping"*

L30-Chemical species? *We changed this to "Chemical substances".*

L64: Do you mean necessary or inevitable? *We changed this to "necessary".*

L194: To make contact to previous studies? *We changed this to: "To compare the quality of our particle transport phase to previous studies".*

Other comments

L56: At what height are these wind velocities? *We added the information that the wind speed was measured at a height of h = 1 m.*

L76-77: Very cool Thanks.

L80-82: Do the particles not come in contact with the propeller?

We added additional information in Section 2.2 (please see your comment 3 above).

Figure 2: How did you conclude the jump in RH was from snow particle sublimation? What's the RH of the cold room?

We added the following sentences at the end of Section 3.1, L183:

"The relative humidity of the cold room varies depending on how often people enter and leave the room during a day. In the morning, the RH is typically low at around 40% - 50%. Therefore, depending on the initial RH before an experiment, a more or less strong increase in RH is obtained due to sublimation of the suspended particles (Dai and Huang, 2014)."

L258-259: Very cool *Thanks.*

L267-268: Again, how can you decouple this from the effect of particles smashing into walls that are necessarily there in nature?

We added additional information on this at the end of Section 3.4.1 (please see your comment 4 above).

RC2: 'Comment on tc-2023-112', Anonymous Referee #2, 15 Sep 2023

Review of the paper "Wind tunnel experiments to quantify the effect of aeolian snow transport on the surface snow microstructure" by Walter et al. submitted to The Cryosphere.

This paper presents an innovative set of measurements to investigate the effect of wind-induced snow transport on the physical properties of surface snow (density, SSA). These measurements were collected in a ring-shaped wind tunnel (RWT) that reproduces the main characteristics of aeolian snow transport. The authors quantified the changes in density and SSA during events with different wind speed and air temperature. Their analysis confirmed the increase in surface snow density with increasing wind speed that has been observed in the field and highlighted a slight decrease in SSA with increasing wind speed. The author also compared the densification rates measured in the RWT with parameterizations used in snowpack schemes.

The subject of this paper is very interesting for the snow community and presents a set of original measurements to quantify the effects of wind on the physical properties of surface snow. So far, these quantifications have mainly been obtained from field measurements (mainly for surface snow density) that are influenced by other physical processes, making it challenging to disentangle the effect of the wind from the other processes. These measurements can serve to develop more-physically based parameterizations of the impact of wind on the physical properties of the snow cover in multi-layer snowpack schemes such as Crocus and SNOWPACK. Therefore, this paper should be published in The Cryosphere. However, prior to publication, the author must carefully define in which context they are working (blowing snow event with concurrent snowfall) and revise accordingly which existing parameterizations they are evaluating in the context of the study. These two general comments are followed by more specific and technical comments.

General comments

1. In this paper, the authors study the effects of aeolian snow transport on the properties of surface snow using a RWT where snow is continuously added to mimic snow precipitation until the end of the different experiments. In this respect, the authors are reproducing in their experiments what happened during blowing snow events with concurrent snowfall. In such conditions, most of the snow transported by the wind is made of precipitating snow particles that fall continuously on the snow surface and are then transported by the wind as illustrated nicely by Figure 3 in the submitted paper. Snow particles initially present at the snow surface may also be transported if the wind speed is sufficient. This situation differs from blowing snow events without snowfall when the transported snow is only made of snow initially present at the snow surface without the constant supply of new snow particles from snowfall. Even if the physical processes involved are the same in terms of particle impacts and ejections, we can expect different densification rates and changes in SSA due the influence of the constant supply of fresh dendritic snow during blowing snow events with concurrent snowfall. Therefore, I strongly recommend to the authors to define well the type of blowing events that they are simulating in the wind tunnel and to discuss how the constant supply of fresh snow influences the results presented in their study. Without such clear discussion, their conclusions may be applied erroneously to different situations that were not captured yet in the RWT experiments.

We agree that we need to better highlight (especially in the abstract) that we only consider blowing snow events with concurrent snowfall. We revised the manuscript accordingly:

First sentence of the abstract L1: *"The evolution of the surface snow microstructure under the influence of wind during precipitation events is hardly understood but crucial for polar and alpine snowpacks."*

Middle of the abstract L8: *"We used a single snow type (dendritic fresh snow) for simulating precipitation, …"*

Section 1, L67*: "It is the aim of our study to propose an experimental setup to systematically investigate how wind affects the evolution of the surface snow density and SSA during precipitation events as functions of the wind speed, air temperature and transport duration."*

Already existing information in Subsection 2.2 L96: *"… , temporally equally distributed over the entire experiment duration* τ_{exp} *(Table 1), to mimic snow precipitation until the end of the experiment."*

2. The authors have evaluated two parameterizations used in snowpack schemes for snow densification that account for the effects of wind. This part of the paper is very valuable for snowpack schemes, but it should be strongly revised to reflect well how the effects of wind on surface snow properties are included in snowpack schemes and then to make sure that the correct parameterizations are tested in this study.

The effect of wind in snowpack schemes such as Crocus, SNOWPACK and SNOWMODEL consist in a two-step process: (i) the falling snow density generally includes a dependency on wind speed (Pahaut, 1975; Lehning et al., 2002; Liston et al., 2007). It is computed at each model time step based on the meteorological forcing (wind speed, air temperature, …) and new snow is added at the top of the snowpack, (ii) the models then account for wind-driven compaction by including a wind compaction term when calculating compaction in the near surface snow layers (Brun et al., 1997; Liston et al., 2007; Amory et al., 2021; Wever et al., 2022). The wind compaction rate depends on the intensity of aeolian snow transport. This second component allows the models to simulate the increase in surface density during blowing snow events without snowfall (cf my first general comment). So far, in their paper, the authors are evaluating two parameterizations for the density of new snow and are ignoring the second component of wind-driven compaction in numerical snow models. I recommend them to better justify why they are only evaluating the first component. There is a clear grey zone in between these two model components with parameterizations that may overlap and may treat twice the same physical process. Ultimately, we could imagine a clearer separation in snowpack schemes where (i) the snowfall density only depends on temperature and density (to indirectly represent the variability in falling hydrometeors) and (ii) all the wind-related process are all treated by a wind-compaction routine. Under this assumption, the measurements presented in this paper would serve to adjust such wind-compaction routines.

We added the following information in Section 2.4, L155: *"In all three models, snow densification by wind is separated in a new snow density term for describing snow compaction during precipitation events, and in a term describing wind-driven compaction during blowing snow events without precipitation. This separation is likely resulting from different time scales and snow types involved in these different processes. During precipitation events, the typically highly dendritic new snow will quite quickly (depending also on the wind speed) cover the underlying (new) snow which then can't be entrained anymore by the wind, resulting in rather short effective transport durations. However, without precipitation, loose surface snow of likely different grain types (potentially more decomposed rounded particles) may be entrained and affect by wind for much longer time (or transport) durations. As we simulate precipitation events in our RWT, only the equations for the new snow density with concurrent snowfall are considered in our study."*

We added the following information in Section 3.4.2, L356: "*The current separation into precipitation and no-precipitation events in the three models as discussed in Section 2.4 results in a grey zone where processes may overlap. New, temporally highly resolved models may aim for more physically based descriptions of these processes. Therefore, a particle shape-based parameterization of the density and SSA as proposed in CROCUS (Vionnet et al., 2012) based on dendricity and sphericity in combination with an effective particle transport duration* τ_t *would likely be beneficial to simultaneously cover precipitation and no-precipitation events in future modelling attempts. However, these parameters are very difficult to measure and quantify experimentally which is probably the reason why current snow models prefer using simple empirical correlations instead of physically based process descriptions."*

We redefined the parameters in Eq. 1 and 2, where the snow density ρ_s is now renamed as ρ_{ns} (new snow density):

$$\rho_{ns} = 10^{\beta_0+\beta_1 T_a+\beta_2 \arcsin(\sqrt{RH})+\beta_3 log_{10}(V)} \qquad \textit{Eq. 1}$$

$$\rho_{ns} = 50 + 1.7(T_{wb} - 258.16) + D_1 + D_2\,[1 - e^{-D_3(V-5)}] \qquad \textit{Eq. 2}$$

3. One of my concerns with the current evaluation shown on Figure 8 is that the authors derive densification rates from parameterizations of snowfall density that does not include any temporal component. These parameterizations only provide a value of snowfall density under given meteorological conditions, but they do not specify which time is needed to reach this value (especially for the wind contribution). It would be very valuable to go back to the original datasets that were used to derive these parameterizations and to better understand the temporal aspect. For example, if these values were derived from measurements of snow taken on snow board, are they representative of 1, 3, 6 or 12, 24 hours snow accumulation? Maybe, it could help to understand the large differences in snow density resulting from these parameterizations. In addition, in Fig. 8, which value is taken when computing the initial ice volume fraction for the parameterizations?

We added the following information in Section 3.4.2, L335: "*The density of the topmost 3 cm of new snow were regularly sampled during different snowfall events, resulting in a temporal resolution of one sample per 0.5-1 h depending on the precipitation intensity. This is comparable to the timescale of our experiments with* τ_{exp} *= 0.5 h (Fig. 8)."*

We added the following information in Section 3.4.2, L338: *"We did not find any information on the timescales and sampling of the data set used for the SnowTran-3D parameterization (Liston et al., 2002). However, the higher densification rates predicted by the SnowTran-3D model (Fig. 8) may result from different time scales, atmospheric conditions, or additional transport in the absence of precipitation involved in the measurements they used for their parameterization. The parameters in Eq. (3) for the CROCUS model originate from a study carried out by Pahaut (1976) at Col de Porte (1325m altitude, French Alps). Unfortunately, no information on these measurements could be found."*

We added the following information in Section 3.3, L248: *"The initial new snow densities representing snow deposited without wind ($V_{0.4m} = 0$ m s−1) measured inside the Snowmaker box were ranging in between 45-80 kg m−3 (Fig. 6a-b) and define the initial ice-volume fraction Φ_{i0} for the calculation of the densification rates in the following figures."*

Overall, there is no doubt that the data collected in this study will inform the development of improved compaction routines due to aeolian snow transport.

Specific Comments

P1 L9: Add at which height were taken the wind speed measurements?

We added the following information in the abstract, L8: "…vary wind speeds at a height of 0.4 m from 3 m s-1 to 7 m s-1 …"

P1 L10-12: as mentioned above, the main contribution of this paper is to provide a set of very rich measurements to better understand the impact of the wind on the properties of surface snow. I recommend the authors to highlight first in the abstract the main conclusions derived from these measurements before mentioning the comparison with existing parameterizations.

We rearranged the abstract accordingly, L10: "*The measured airborne impact trajectories confirm the consistency of our coefficient of restitution with large scale saltation, rendering the setup suitable to realistically study interactions between airborne and deposited snow. Increasing wind speeds were found to result in intensified densification and stronger SSA decreases. The most drastic snow density and SSA changes of deposited snow are observed close to the melting point. Our measured densification rates as a function of wind speed show clear deviations from existing statistical models but can be re-parameterized through our data."*

P1 L 18-22: at this stage, the introduction lacks clarity. I recommend the authors to make a better distinction between blowing snow events with and without concurrent snowfall and to describe the types of particles that are transported in these two situations.

Please see the answer to your comment 3 above.

P2 L 45-46: the author can refer here to Royer et al. (2021), Wever et al (2022) and Amory et al (2021) to illustrate how the parameterizations of the increase of surface snow density due to wind can be adjusted to better represent the properties of surface snow in the Arctic and in Antarctica.

We added this information in the Introduction Section 1, L47: *"However, Royer et al. (2021), Amory et al. (2021), and Wever et al. (2022) illustrate how the parameterizations of the increase of surface snow density due to wind can be adjusted in these models to better represent the properties of surface snow in the Arctic and in Antarctica."*

P3 L 85-90: Section 2.2 explains the detail of the different experiments. Before jumping straight into the detailed description of the experiments, it would be good for the reader to give an overview of what is tested with these 12 experiments.

We added the following information in Section 2.2, L89: *"We performed a total of 14 RWT experiments for testing the effects of wind speed (experiment 1-7), transport duration (experiments 8-9) and temperature (experiments 10-12) on the surface snow microstructure (Table 1). Particle impact characteristics were measured with a high-speed camera during experiment 13, while experiment 14 served as a sensitivity study to test the effect of sublimation and vapor re-deposition on airborne particles.*

P 5 P 111-112: at which heights are measured the air temperature and relative humidity in the RWT.

We added the following information in Section 2.3, L125: *"The air temperature and relative humidity are measured at a height of 0.15 m, while the wind speed is measured at a height of 0.4 m."*

P 6 L 137: Eq. 1 is not described in Lehning et al. (2002). It seems that the ZWART equation has been developed later. Can the authors add a reference? The equation is also described in this supplementary material (Section 4 of https://tc.copernicus.org/articles/17/519/2023/tc-17-519-2023-supplement.pdf).

We added the following reference in Section 2.4, L163: *"Zwart, C. (2007) Significance of new-snow properties for snow-cover development, Master thesis, WSL Institute for Snow and Avalanche Research SLF, Davos, Switzerland."*

P 6 L 146-148: If the authors manage to correctly justify why they are evaluating parameterization of falling snow density, the parameterization of Pahaut (1975) implemented in Crocus (Vionnet et al., 2012) could be tested as well. Indeed, it only depends on temperature and wind speed.

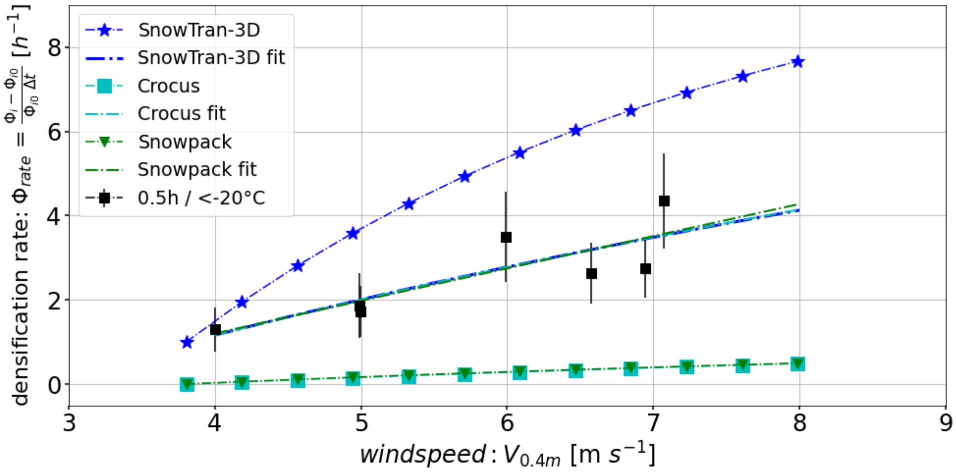
We introduced the parameterization from Vionnet et al. (2012) (Eq. 1) in Section 2.4 and tested it now as well in Section 3.4.1 and 3.4.2:

Section 2.4, L173: *"Another empirical description for the new snow density affected by wind during precipitation events is implemented in the CROCUS snow model (Brun et al., 1997; Vionnet et al., 2012) which is also expressed as a function of the wind speed V and the air temperature* T_a*:*

$$\rho_{ns} = a_\rho + b_\rho\left(T_a - T_{fus}\right) + c_\rho\sqrt{V} \qquad \textit{Eq. 3}$$

In Eq. 3, T_{fus} is the temperature of the melting point for water, a_ρ = 109 kg m^{-3}, b_ρ = 6 kg m^{-3} K^{-1} and c_ρ = 26 kg $m^{-7/2}$ $s^{-1/2}$ s−1/2 are constants."

New Fig. 8 including the CROCUS model results:



Section 3.4.1, L301: "*Furthermore, Fig. 8 includes model predictions and model fits to our RWT data using the theoretical descriptions from the SNOWPACK, SnowTran-3D and CROCUS models (Eq. 1-3).*"

Section 3.4.2, L348: "*The following parameters a_ρ = 43 kg m^{-3}, b_ρ = 9 kg m^{-3} K^{-1} and c_ρ = 35 kg $m^{-7/2}$ $s^{-1/2}$ were obtained for the CROCUS model when fitting Eq. 3 to our data. Minimum air temperatures of T_a = -10°C (CROCUS) and T_a = -15°C (SnowTran-3D) had to be used for these model fits instead of the actual air temperature T_a = -24°C measured during the experiments 1-7 (Table 1), because the models do not result in realistic values for lower air temperatures. However, Fig. 9b in our manuscript shows that the densification rate tends to be temperature independent below approximately T_a < -6°. Therefore, the fit parameters for the CROCUS model are only considered to be valid for $V_{0.4m}$ > 3.8 m s^{-1} in Eq. 3, which corresponds to a wind speed of 5 m s^{-1} at a height of 2m with an aerodynamic roughness length of z_0 = 0.24 mm for fresh snow as determined by Gromke et al. (2011).* "

P 7 L 168-170: In this paragraph, the authors measure if the particle impact characteristics in the RWT are consistent with natural conditions. They compare their results with the measurements from Sugiura et al . (2000). However, these measurements were also collected in a wind tunnel. Can the authors elaborate on the definition of natural conditions?

We replaced "natural conditions" with "well-developed boundary layer flow" in Section 3.2, L200: "*We thus argue here that the boundary layer flow may not necessarily be perfectly homogeneous, stationary, and well-developed, as long as the particle impact characteristics are consistent with those of a well-developed boundary layer flow as studied by Sugiura et al. (2000)*"

P 10 L 198-200: could the authors test the statistical significance of the regression lines shown on Fig. 5a, 5b and 5c?

We added the following information in Section 3.2, L233: "The p-values for the statistical significance are $p = 0.011$ (strong evidence, Fig. 5a), $p = 0.079$ (weak evidence or trend, Fig. 5b), and $p = 0.0067$ (strong evidence, Fig. 5c)."

P 15 L 305-308: it would make sense to propose a fit that respects the physical grounds and tends to zero for very long times.

We tested such a fit previously. However, because of the limited data available (only 3 measurement points, Fig. 9a), a simple one or two-parameter function is required. Simply removing the parameter B1 from Eq. 5 (previously Eq. 4) results in a poor fit. A two-parameter exponential function also results in a bad representation (almost linear in this range) of the data. Based on the definition of the densification rate, the reciprocal function seemed to be most feasible resulting in a reasonable fit to the data in the measurement range. Therefore, we would rather keep it as simple as it is and added and modified the following sentences in Section 3.4.3, L380: *"… with $A_1 = 1.30$ and $B_1 = 0.80\ h^{-1}$, simply to represent the three data points in the experimental range with a two-parameter fit. On physical grounds, the densification rate should tend to zero instead for very long times, which would require more data points to obtain a reasonable fit. However, the good fit of the reciprocal function (Eq. 5) indicates that the time τ_{exp} (experiment duration) governs the decrease of the densification rate and not the change in ice volume fraction."*

P 18 L 370-371: it would be interesting to mention that the effect of ambient relative humidity should be tested as well due to its large impact on blowing snow sublimation.

We added this information in Section 3.5.2, L447: *"Whether the proposed parameterization of Eq. 6 is valid for different wind speeds $V_{0.4m}$, experiment durations τ_{exp} and relative humidity RH must be tested in future studies."*

P 20 L 419: would it be possible to write Eq (7 -> now 8) in terms of SSA rate as the previous equations?

We thought about this. However, the different time scale used in this additional sensitivity experiment, the mean effective "transport duration" τ_t instead of the experiment duration τ_{exp}, we want to avoid the definition of an additional SSA rate that can easily be confused with the ones from Fig. 10 and 11 which are based on τ_{exp}.

Technical Comments

Figures

Figure 3: can the authors add on the three photos the corresponding time stamps as well as a vertical and horizontal scale?

We added time stamps and a millimetre scale to Fig. 3:

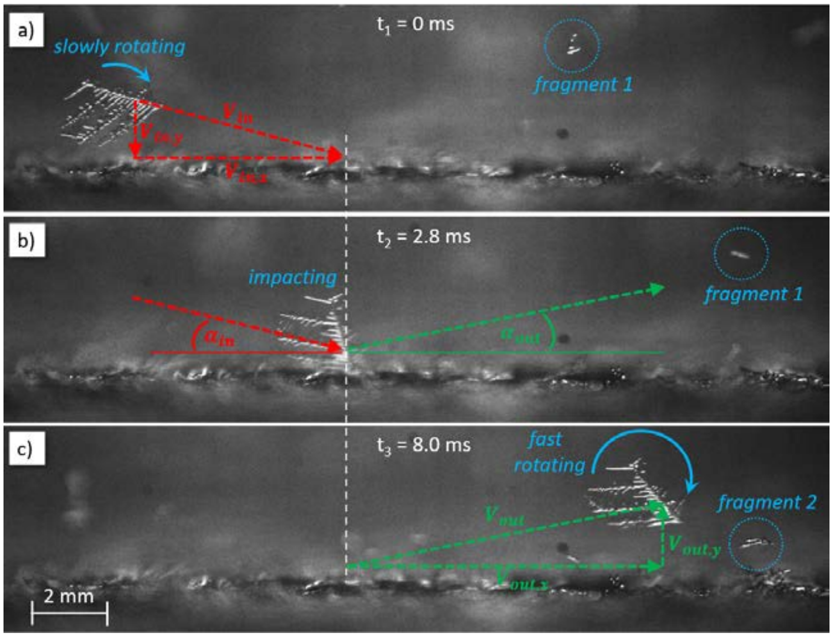


Figure 6: it would be interesting to have the same range of values for the y-axis of Fig 6c and 6d. Otherwise, it seems stronger SSA decreased are measured with the mico-CT.

We had this initially, however, the micro-CT data (Fig. 6d) becomes too compressed, so that the individual measurements and the SSA reduction cannot be clearly identified anymore. We keep it as it is and put a note in the description of Fig. 6 that the reader should be aware of the different y-axis scales: *"A different y-scaling is used in d) relative to c) for a better visualization of the SSA changes for the μCT measurements."*

Figure 6: If one micro_CT measurement has been collected for each experiment, what does represent the error bars shown on Fig 6b and d?

We added the following sentence in Section 3.3, L267: *"The error bars for the micro-CT measurements in Fig. 6b and 6d are derived from the standard deviation of the vertical density and SSA variability of the 3-6 cm high snow samples with 20-40 data points.*

Tables

Table 1: mention in the caption if relative humidity is measured with respect to ice.

We added this information in the caption of Table 1: *"The average value for RH measured with respect to ice is calculated from the second period of each experiment where a situation close to equilibrium for RH is reached (Fig. 2)."*

Table 1: it would be interesting to know on this table for which experiments micro-CT measurements have been carried out.

We added this information in Table 1:

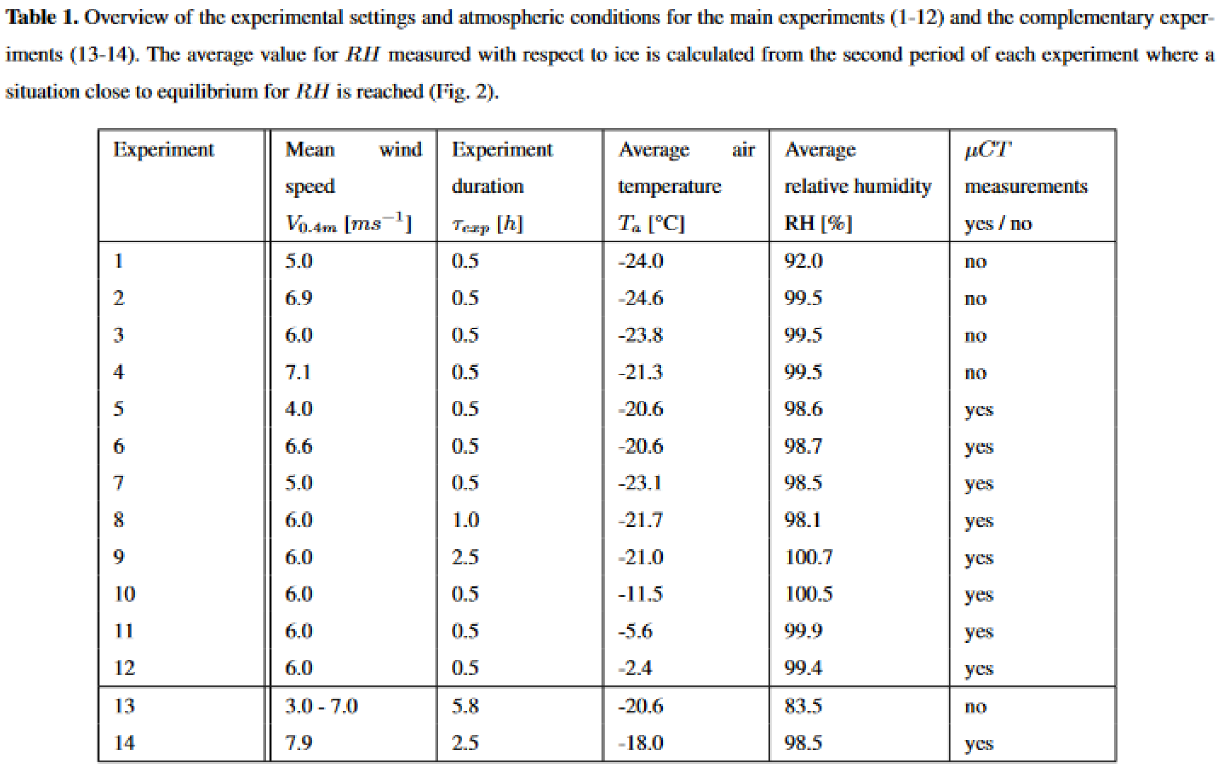


Table 1. Overview of the experimental settings and atmospheric conditions for the main experiments (1-12) and the complementary experiments (13-14). The average value for RH measured with respect to ice is calculated from the second period of each experiment where a situation close to equilibrium for RH is reached (Fig. 2).

Experiment	Mean wind speed $V_{0.4m}$ $[ms^{-1}]$	Experiment duration τ_{exp} $[h]$	Average air temperature T_a [°C]	Average relative humidity RH [%]	μCT measurements yes / no
1	5.0	0.5	-24.0	92.0	no
2	6.9	0.5	-24.6	99.5	no
3	6.0	0.5	-23.8	99.5	no
4	7.1	0.5	-21.3	99.5	no
5	4.0	0.5	-20.6	98.6	yes
6	6.6	0.5	-20.6	98.7	yes
7	5.0	0.5	-23.1	98.5	yes
8	6.0	1.0	-21.7	98.1	yes
9	6.0	2.5	-21.0	100.7	yes
10	6.0	0.5	-11.5	100.5	yes
11	6.0	0.5	-5.6	99.9	yes
12	6.0	0.5	-2.4	99.4	yes
13	3.0 - 7.0	5.8	-20.6	83.5	no
14	7.9	2.5	-18.0	98.5	yes

References (used in this review and not present in the initial manuscript)

We added these references to the bibliography:

Amory, C., Kittel, C., Le Toumelin, L., Agosta, C., Delhasse, A., Favier, V., & Fettweis, X. (2021). Performance of MAR (v3. 11) in simulating the drifting-snow climate and surface mass balance of Adélie Land, East Antarctica. *Geoscientific Model Development*, *14*(6), 3487-3510.

Pahaut, E.: La métamorphose des cristaux de neige (Snow crystal metamorphosis), Monographies de la Météorologie Nationale, Vol. 96, Météo France, 1975.

Royer, A., Picard, G., Vargel, C., Langlois, A., Gouttevin, I., & Dumont, M. (2021). Improved simulation of arctic circumpolar land area snow properties and soil temperatures. Frontiers in Earth Science, 9, 685140.

Wever, N., Keenan, E., Amory, C., Lehning, M., Sigmund, A., Huwald, H., & Lenaerts, J. T. (2023). Observations and simulations of new snow density in the drifting snow-dominated environment of Antarctica. *Journal of Glaciology*, *69*(276), 823-840.