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.

First thanks a lot for the very detailed review of our manuscript. We included most of your suggestions which helped to significantly improve the quality of the article!

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 and elsewhere) that we only consider blowing snow events with concurrent snowfall. Our supply of fresh snow simulating precipitation leads to a constant source of highly fragile dendritic new snow crystals that are primarily transported by the wind. Our results (Fig. 9a) shows that initial densification rates are highest which we attribute to the effect that the most fragile dendrites are easily fragmented once added to the flow. We will revise the manuscript to better point out that we only consider precipitation events.

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.

Thanks for this important comment! We agree that we need to better clarify why we chose these model components. From both models (Lehning et al., 2002 and Liston et al., 2007) we used the parameterizations (EQ. 1 and 2 in our manuscript) that define a new snow density for precipitation events under the influence of strong winds as we simulated in our case in the RWT. Both models are parameterized based on field measurements of new snow densities with concurrent snowfall. Therefore, we do not consider the effect of snow densification due to wind in the absence of precipitation.

The current separation into wind affected precipitation and snow transport without precipitation in models is likely resulting from different time scales and snow types involved. 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 transport durations τ_t *as discussed in our Section 3.6.2. However, without precipitation, (loose) surface snow of likely different grain type (potentially more decomposed rounded particles) may be entrained and affect by wind for much longer time (or transport) durations.*

Regarding the transport duration: Fig. 9a shows the dependency of the densification rate for different experiment durations (τ_{exp}*) with different effective transport durations (*τ_t*) as estimated in Section 3.6.2. Basically, our experiment with* $\tau_{exp} = 2.5h$ *simulates a low precipitation rate situation resulting in an estimated mean effective transport duration of* $\tau_t \approx 5$ *min (Section 3.6.2) for individual snow particles, while the experiments with* $\tau_{exp} = 0.5h$ *simulate a high precipitation rate situation with* $\tau_t \approx 1$ *min.*

However, we agree that the current separation into precipitation and no-precipitation events results in a grey zone where processes may overlap, and that new temporally highly resolved models should 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 the effective transport duration τ_t *would likely be the way to go, although these parameters are very difficult to quantify or verify experimentally. This is probably the main reason why current snow models prefer using simple empirical correlations instead of physically based process descriptions.*

We will include the above discussion and your suggestions accordingly in the revised manuscript in Section 3.4.2. We also redefine the parameters in Eq. 1 and 2, where the snow density ρ_s *will be renamed as* ρ_{ns} *(new snow density), and clearly state here and elsewhere that we only consider new snow densification under the influence of wind. We will rewrite these equations as*

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

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

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?

Generally, as discussed in Section 3.6.2, the temporal component is extremely difficult to quantify experimentally, because the important timescale is the mean effective transport duration τ_t of the snow particles which defines the magnitude of fragmentation and airborne metamorphism (see Section 3.6.2). This time τ_t depends mainly on the particle size and shape, wind speed and the precipitation intensity, where the latter defines how fast additional precipitation particles will cover up the underlying deposited snow which then cannot be re-mobilized again.

The parameterization of the SNOWPACK model (Lehning et al. 2002) is based on field measurements at the Weisfluhjoch (WFJ) field site in Davos, Switzerland. The density of the topmost 3 cm of new snow were regularly sampled during different snowfall events, resulting in a maximum temporal resolution of one sample per 0.5 h depending on the precipitation intensity. This is comparable to the timescales of our experiments with $\tau_{exp} = 0.5 – 2.5$ h. We did not find any information on the timescales of the dataset used in SnowTran-3D (Liston et al., 2002) for the parameterization of Eq. 2 above, i.e. the parameters D_1-D_3. However, we assume a daily measured new snow density has been correlated to a 24h mean wind speed for days with precipitation, where intermittent snowfall and variable wind speeds may strongly affect the results. A significant contribution of densification without precipitation during these days may thus result in longer effective transport durations τ_t and thus higher densification rates as shown in Fig. 8 for the SnowTran-3D model, that could potentially explain the large model discrepancies. We will include this discussion in the revised version in Section 2.4.

Regarding the initial ice volume fraction, we wrote in L215: "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) …" we will add the information that these densities 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.

Thank you very much!

Specific Comments

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

The wind speed was measured at a height of 0.4 m. We will add this information accordingly in the abstract.

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 agree and will rearrange the abstract accordingly.

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.

We agree. Therefore, we will make a clear distinction as suggested and add more information on the particle types as discussed above (dendritic new snow during precipitation and typically more rounded grains in the absence of precipitation).

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 agree that this is a good idea, and we will add these references.

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 agree that we should first mention that we want to test different wind speeds, temperatures, and transport durations with these 12 experiments and that the experiment #13 aims at measuring particle impact characteristics with the high-speed camera and experiment #14 is a sensitivity experiment to estimate the magnitude of airborne snow metamorphism.

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

The air temperature and relative humidity are measured at a height of 15 cm. We will add this information in Section 2.3.

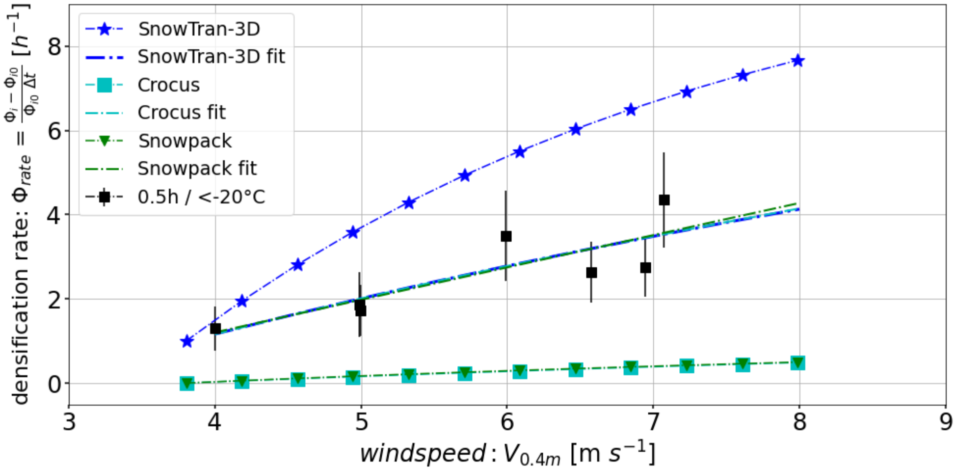
P 6 L 137: Eq. 1 is not described in Lehning et al. (2002). It seems that the ZWART equation has been developed later. Can the authors add a reference? The equation is also described in this supplementary material (Section 4 of https://tc.copernicus.org/articles/17/519/2023/tc-17-519-2023-supplement.pdf).

Great! Thanks! We used the Lehning et al. (2002) reference as a general reference for the SNOWPACK model, as the ZWART parameterization was never published officially. We will use this very recent reference now.

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.

Thanks a lot for this important comment! We hope we convinced you in the above discussion on transport durations and snow types that we should only consider parameterizations with concurrent snowfalls. We somehow missed the parameterization introduced in Vionnet et al. (2012) (Eq. 1) and tested it now as well:

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



We obtain the following parameters $a_\rho = 43$ kg m^{-3}, $b_\rho = 9$ kg m^{-3} K^{-1} and $c_\rho = 35$ kg $m^{-7/2}$ $s^{-1/2}$ 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 low 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). We will introduce the Crocus parameterization in Section 2.4 as well as the results and discussion in Section 3.4.2.

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 agree that "natural conditions" is misleading. What we mean is "well developed boundary layer flow conditions" as it can be achieved in a linear wind tunnel as used by Sugiura et al. (2000). We will change this accordingly.

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

Great question! 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). We will add this information in Section 3.2.

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 did this previously. However, because of the limited data available (only 3 measurement points, Fig. 9a), a simple two-parameter function is required. A two-parameter exponential function results in a bad representation (almost linear in this range) of the data. Based on the definition of the densification rate, a 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 with the statement we provided: "… simply to represent the data points in the experimental range. On physical grounds, the densification rate should tend to zero for very long times." We would also add: "The good fit of the reciprocal function indicates that the time (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.

This is a good point and we will mention this in the revised version.

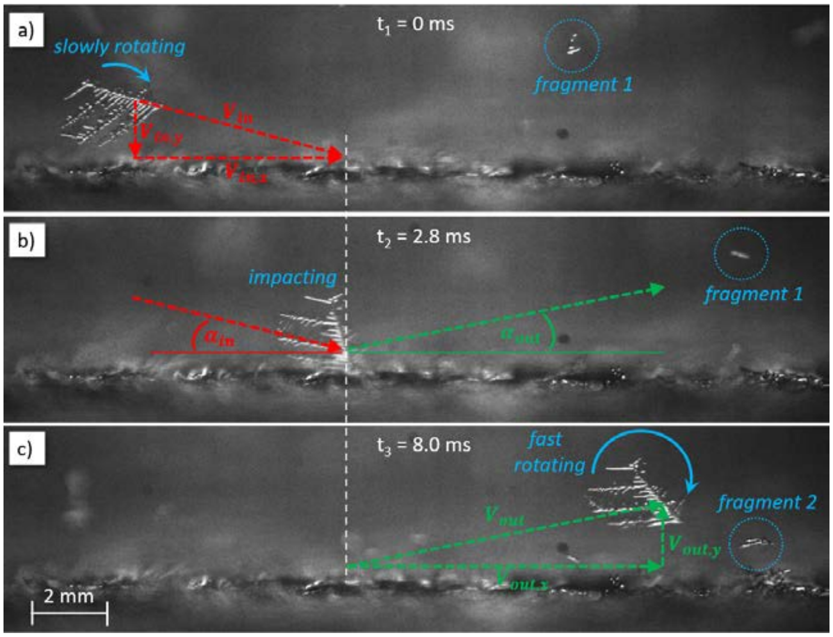
P 20 L 419: would it be possible to write Eq (7) in terms of SSA rate as the previous equations?

We thought about this. However, the different time scale used in this additional sensitivity experiment (the mean effective "transport duration" τ_t*) 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 the experiment duration* 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?



Good point! We added time stamps and a scale.

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 becomes too compressed so that the individual measurements cannot be clearly identified anymore. We would rather keep it as it is and put a note there that the reader should be aware of the different y-axis scales.

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?

Thanks for this important comment! The error bars for the micro-CT measurements in Fig. 6b,d are one standard deviation of the vertical density and SSA variability of the 3-6 cm high snow samples with 20-40 data points. We will add this information in Section 3.3

Tables

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

Good point! We will include this in the caption!

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

Good point! We added a column in the table below:

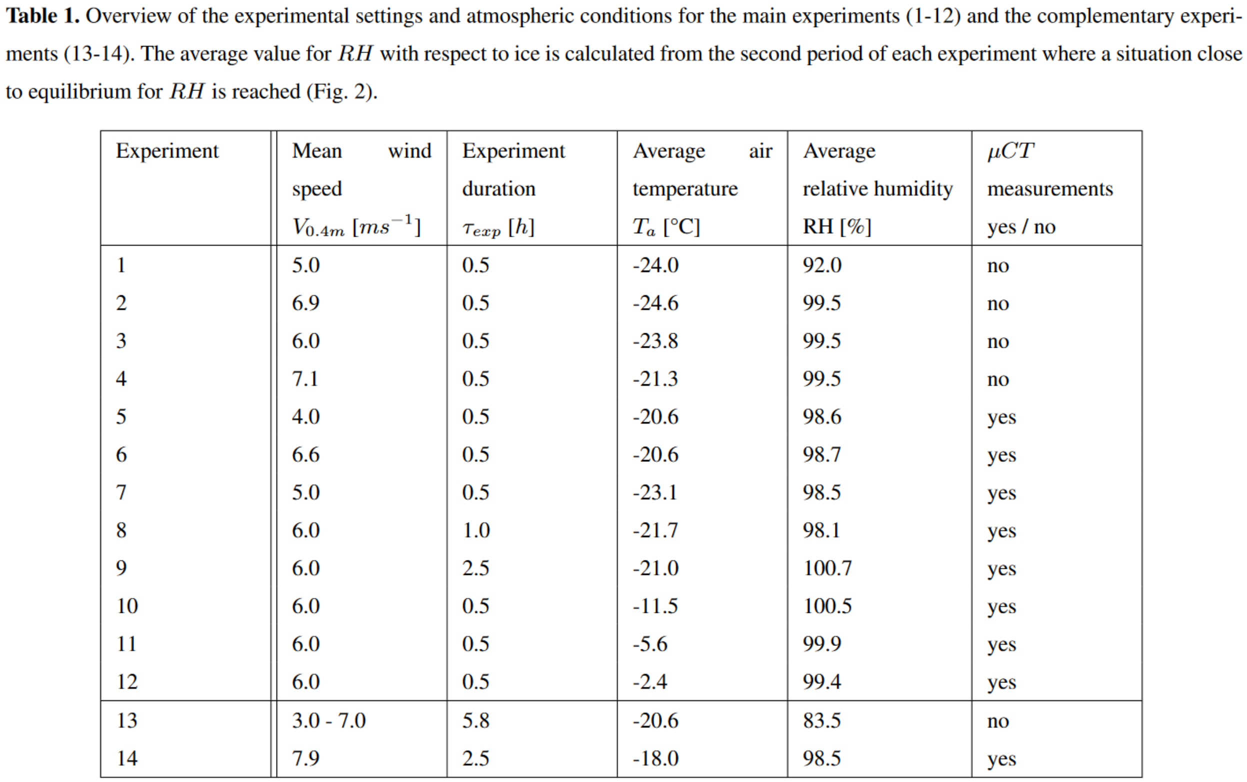


Table 1. Overview of the experimental settings and atmospheric conditions for the main experiments (1-12) and the complementary experiments (13-14). The average value for RH with respect to ice is calculated from the second period of each experiment where a situation close to equilibrium for RH is reached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

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.

Thanks! We add these references at the specific locations.