Report #1
Submitted on 09 Feb 2024
Anonymous referee #2

Summary and recommendation:

In the revised paper, the authors have addressed most of the questions that I raised previously about the type of blowing snow events considered in this study and the different model parameterizations that are tested against the wind tunnel data. I made below a suite of comments for the authors to consider to improve the description and evaluation of the model parameterizations. This paper is a very valuable contribution to the literature, and it should be published in TC.

Thanks a lot for your very valuable comments and for reviewing our article again.

Line comments (with line numbers referring to the new version of the paper, without Track Change mode):

P2 L 37: the range of density for strongly wind affected surface snow should be revised. Maybe the range 250 – 400 kg/m3 is more appropriate for polar snow. See for example:
- Fausto, R. S., Box, J. E., Vandecrux, B., Van As, D., Steffen, K., MacFerrin, M. J., ... & Braithwaite, R. J. (2018). A snow density dataset for improving surface boundary conditions in Greenland ice sheet firn modeling. Frontiers in Earth Science, 6, 51.
- Domine, F., Lackner, G., Sarrazin, D., Poirier, M., & Belke-Brea, M. (2021). Meteorological, snow and soil data (2013–2019) from a herb tundra permafrost site at Bylot Island, Canadian high Arctic, for driving and testing snow and land surface models. Earth System Science Data, 13(9), 4331-4348.

We adapted the density range and added the references. Fausto et al. (2018).

P 6 L 155-157: the description of the snow densification by wind in the models should be revised. Indeed, the wind-driven compaction represents the compaction of snow that has been previously deposited at the snow surface. It can be active with and without concurrent snowfall as soon as the wind condition are sufficient to generate ground-based snow transport. Therefore, the sentence “a term describing wind-driven compaction during blowing snow events without precipitation” is not accurate and should be revised.

Thanks, this information was indeed misleading, therefore we rephrased this sentence:
L155 “In all three models, snow densification by wind can be active with and without concurrent snowfall and is initiated when wind speed exceeds certain thresholds to generate snow transport. Therefore, all models include terms describing ground-based densification of surface snow layers due to wind transport, and new snow density terms (rho_{ns}) that also depend on wind speed for describing an initial compaction of precipitation. “

P 7 L 175: Note that in Crocus the implementation of the Pahaut relationship assumes a minimal value of 50 kg/m3 for the snowfall density. This minimal value should certainly be taken into account by the authors when deriving the optimal parameters for the Pahaut relationship (P 16 L345-355).

We added:
L179: "… and the minimal initial density is 50 kg m^{2} (Pahaut, 1975)."

P 7 L 176: Royer et al. (2021) have proposed a modification of the Pahaut relationship to better represent the effect of wind on surface density in arctic environment. They double the value of c_rho. It could be interesting to test this alternative formulation in the paper and compare it with the fit proposed by the authors (P 16 L 345-355).

Thank you for this comment! We doubled the value c_rho according to Royer et al. (2021) as suggested which results in a significant increase of the absolute snow density but only in a small increase of the densification rate. The main discrepancy between the measurements and the models is, as you indicate in your next comment, the result of the estimated time scales involved.

P 15: on Figure 8 the authors derive densification rates from parameterizations of snowfall density that do not include any temporal component. These parameterizations only provide a value of snowfall density under given meteorological conditions. The author must explain how they have computed the densification rate from those parameterizations. This point was raised in my initial review and has not been properly addressed by the authors.

Thanks for asking again this important question. We seemed to have missed providing a direct answer to this question during the initial review! The answer was partially indirectly provided in Section 3.4.2, but your question exactly pinpoints the problem of involved time scales with recent parameterizations for the new snow density during precipitation events. Therefore, we added additional information in Section 3.4.2. New paragraph that also treats most of your comments below:

L366: "We conclude that the differences between the models and our measurements are mainly the result of the estimated time scale (Δt) used for the calculation of the densification rates (Fig. 8). The new snow densification parameterizations (Eq. 1-3) do not contain any temporal component at all, although the measurements they are based on involved some time scales. However, densification of new snow under wind during precipitation events not only depends on the wind speed, but also on an effective transport duration (τ_t) of individual precipitation particles, which is mainly governed by the precipitation intensity and particle cohesion as discussed below. We used a time scale of Δt = 0.5h for calculating the densification rates for our experiments and all three models (Fig. 8). This time scale is at least appropriate for the SNOWPACK model and our measurements. That the SNOWPACK model nevertheless predicts significantly lower densification rates might be the result of lower precipitation rates during their field measurements resulting in longer effective transport durations τ_t as discussed in the following Section (Fig. 9a). The discrepancy for the two other models (SnowTran-3D and CROCUS) is likely also the result of different time scales Δt involved in their measurements used for the model parameterization. Changing Δt from 0.5 h to 1 h for the SnowTran-3D model and to 0.1h for SNOWPACK and CROCUS already results in reasonable agreement of the models with our measurements, highlighting the strong dependency of the model on involved time scales. Additional discrepancies between the model descriptions and our measurements may arise from the fact that we did not consider additional compaction of surface snow layers due to wind when using the models (Fig. 8), because our RWT simulations are similar to the field measurements used to parameterize the wind speed dependent new snow density in the models. This highlights the problem of overlapping processes, where wind compaction during precipitation may be treated twice in the models: Once within the description of the wind speed dependent new snow density (Eq. 1-3), and once during additional wind compaction of surface layers. We conclude that a clearer separation in snowpack schemes may improve future model attempts of wind induced snow compaction, where the snowfall density only depends on temperature and humidity (to indirectly represent the variability in falling hydrometeors) and all the wind-related processes are treated by a well calibrated wind-compaction routine. Overall, the discrepancies between the models and our measurements can be attributed to poorly defined time scales, different precipitation intensities, different initial precipitation particles, particle cohesion, and local topography and climate conditions. This highlights the importance for more detailed physical descriptions of snow densification."

P 16: L 356-357: As mentioned above, the models (at least Crocus) are not separating the wind densification into precipitation and no-precipitation events. During a blowing snow event with concurrent snowfall, both parameterizations (wind-dependent snowfall density and wind compaction routine) will contribute to the increase in surface density. It could explain why the formulations of Pahaut and Zwart used in Crocus and SNOWPCK cannot predict the observed densifications rates. They certainly need to be combined with a wind compaction routine to fully represent wind densification during blowing snow events with concurrent snowfall. It would be interesting to explicitly mention this feature in this part of the analysis.

We agree and included a discussion of this in the new paragraph (previous comment):
L378: "Additional discrepancies between the model descriptions and our measurements may arise from the fact that we did not consider additional compaction of surface snow layers due to wind when using the models (Fig. 8), because our RWT simulations are similar to the field measurements used to parameterize the wind speed dependent new snow density in the models. This highlights the problem of overlapping processes, where wind compaction during precipitation may be treated twice in the models: Once within the description of the wind speed dependent new snow density (Eq. 1-3), and once during additional wind compaction of surface layers."

But, as mentioned in my first review, this generates a clear grey zone in between these two model components with parameterizations that may overlap and may even 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 humidity (to indirectly represent the variability in falling hydrometeors) and (ii) all the wind-related process are all treated by a well calibrated wind-compaction routine. The data collected in the SLF RWT will improve the wind-compaction routines implemented in the model.
Thanks for this comment! We added this information also in the new paragraph of Section 3.4.2:
L383: "We conclude that a clearer separation in snowpack schemes may improve future model attempts of wind induced snow compaction, where the snowfall density only depends on temperature and humidity (to indirectly represent the variability in falling hydrometeors) and all the wind-related processes are treated by a well calibrated wind-compaction routine."

Report #2
Submitted on 04 Mar 2024
Referee #1: Nikolas Aksamit, nikolas.aksamit@uit.no

The authors have been receptive to my previous concerns and have put in a notable effort to account for them in the updated manuscript. These efforts are appreciated.

Thanks for reviewing again our manuscript and highlighting potential improvements!

The primary concern in my original review was the effect of the wall impacts through the curved section of the ring tunnel. As the authors state "In the curved sections, a large portion of the snow particles are transported along the outer wall due to centrifugal forces. " They have largely addressed these concerns in lines 326-337 (section 3.4.1, not 3.2).

As it stands, I find their response still lacking any transparent quantifications, and a bit confusing in its description:

The authors first describe the snow motion as "sliding" along the wall, which is a very strange process for "large portion of the snow particles" to repeatedly undergo when simulating saltation. Should we now be thinking about coefficients of friction?

Thanks for highlighting this. The word "sliding" is indeed misleading. We updated this section:

L318: "In the curved sections, the particles are transported within a few centimeters distance from the vertical RWT outer wall. The visually identified modes of transport were a mixture of bouncing (saltation), rolling, and sliding along the wall, thus similar to saltation at the horizontal snow surfaces at the straight sections. The particle transport along the curved side walls is inevitable for a compact closed circuit wind tunnel in a cold laboratory of limited dimensions."

The authors then state that the centrifugal forces were 2-3 orders of magnitude smaller compared to forces acting on the particle during impact. How the authors came to this conclusion is not supported by any calculations, or measurements. It is unclear to me where these numbers came from. Did they perform particle tracking around the bend? Just the beginning of the bend or in the middle as well?

Thanks for this comment. We did not perform any particle tracking in the curved sections. We agree that we should be more transparent on where these numbers come from and therefore added the following information:

L322: "The centrifugal forces acting on snow particles in the curved section were estimated being one to two orders of magnitude smaller compared to the forces acting on the snow particles during surface impact while saltating. The maximum centrifugal force was calculated as F_{c} = $m_{p}*v_{p}^2/r = 4.3$ μN for a large spherical snow particle of 0.5 mm diameter with a mass of m_{p} = 0.06mg, a maximum horizontal velocity of v_{p} = 6 ms^{-1} ($V_{0.4m}$ $\approx$ 7 $m s^{-1}$) and the RWT radius of the curved section of r = 0.5 m. Horizontal snow particle velocities in snow saltation layers can be approximated as being about 1-2 ms^{-1} lower than the mean horizontal wind speed (Nishimura et al., 2014). The maximum impact force can be calculated as F_{c} = ΔE_k/h = 360 μN, where ΔE_k is the kinetic energy difference before and after an impact of a similar snow particle of mass (m_{p} = 0.06mg) estimated from Fig. 5c as ΔE_k = 0.5*m_{p}*(V_{in}^2 – V_{out}^2) = 0.5 * m_{p} * 6 $m^2 s^{-2}$ at the same wind speed of $V_{0.4m}$ = 7 $m s^{-1}$. An unknown parameter in this estimate is the height h which defines the particle penetration distance into the snow surface. For small h equal to the particle diameter, the particle impact force is about two orders of magnitude larger than the centrifugal force according to the values above. For increasing penetration distances h (depending on the snow surface elastic or plastic deformation potential), the impact force decreases but is still one order of magnitude larger than the centrifugal force even for h equal 8 times the particle diameter. We conclude that centrifugal forces in the curved section are negligible compared to surface impact forces for our RWT experiments."

Next, the authors suggest the particles are undergoing some sort of bouncing along the wall and estimate the impact angles along the wall are similar to the straight sections, but we don't know where these values come from (numerical simulation? Measurements?). If there are indeed a lot of small hops around the corner, and not some sliding, could you argue the cumulative impact on the crystals is less than during the same transport time in the straight section?

Sorry for the confusion. We assessed the effect of the curvature on the particles based on the above estimation of centrifugal forces. Regarding impact angles in the curved section, we meant the first impact of the particles after the straight test section into the vertical, curved side wall, which is likely the maximum impact angle the particles will experience in the curved section. We did not do any simulation or particle tracking measurements in the curved sections. We modified this Section accordingly:

L336: "The impact angles of the snow particles first impact into the vertical, curved side walls after a straight section were calculated (based on geometrical considerations) to be within a range of 5°-25°. These angles are comparable to the impact angles α_{in} on the horizontal snow surface in the straight test section (Fig. 4b and d)."

Finally, a stokes number of <0.1 is subsequently provided, as well as the suggestion of "good flow following behavior of the snow particles when the air flow gets redirected in the curved section, resulting in smaller impact angles." Is this to suggest the stokes drag is countering the effect of the curve?

Thank you for this very important question which revealed an erroneous calculation of the Stokes number. Our calculation assumed a low Reynolds number flow which is not the case for our experiments. Therefore, no "good flow following behavior" of the snow particles can be assumed per se reducing impact angles and thus impact forces at the first impact in the curved section. Instead, the flow following behavior strongly depends on the particle size and shape, thus the drag coefficient. Huang et al. (2015) have shown that trajectories of smaller snow particles < 100 µm follow turbulent motions of the flow transitioning into suspension, whereas larger particles > 300 µm have a poor flow following behavior thus remaining in saltation. Similarly, in our case, impact angles of larger particles are assumed being less reduced than that of smaller particles.

Based on this comment and other comments below, we added new information and revised the entire paragraph:

L335 “Besides the centrifugal forces along the curved side walls, the first impact of snow particles into the vertical, curved side walls after the straight sections introduce additional unnatural mechanical stress on the snow particles, potentially affecting fragmentation. The above introduced estimate of the impact force F_{i} onto the horizontal snow surface is based on impact characteristics determined from the particle tracking measurements, data that is not available for the first impacts at the curved sections. Therefore, we can only provide a discussion of potential differences that may in- or decrease the wall impact force relative to the snow surface impact force. The impact angles of the snow particles’ first impact into the side walls were calculated (based on geometrical considerations) to be within a range of 5°-25°. These angles are comparable to the observed impact angles α_{in} on the horizontal snow surface in the straight test section (Fig. 4b and d). The maximum particle impact velocities into the side wall can again be estimated being 1-2 ms^{-1} lower than the mean horizontal wind speed, thus about v_{p} = 5-6 ms^{-1} ($V_{0.4m}$ $\approx$ 7 $m s^{-1}$). These maximum impact velocities are comparable to the maximum impact velocities V_{in} on the horizontal snow surface (Fig. 5a). Geometric vector analysis revealed similar wall normal velocity components for the snow and the curved wall impacts. While the impact angles and velocities are similar, the hard wooden surface of the curved side walls likely increases the impact force relative to the snow surface. Contrarily, the smooth surface of the side walls is assumed to reduce the ejection angle and increase the ejection velocity compared to a snow surface impact, resulting in a decrease of the normalized dissipated impact energy (Fig. 5c) and impact force. The impact angle and the impact force may further be reduced by the particles’ ability to follow the flow. Smaller particles (< 100 μm) have a good flow following behavior (Huang et al., 2015) resulting in a reduction of the impact angles and thus forces. Vice versa, larger particles (> 300 μm) have a poor flow following behavior resulting in a minor reduction of the impact angle and force. An estimate of the particle size distribution for our experiments (Section 3.6.1, Fig. 12a) reveals that the majority of our snow particles are of a size smaller than 200 μm, indicating that our particles likely experienced a significant reduction of the impact angle and thus force relative to the impacts analysed based on purely geometrically calculated impact angles. We conclude that these difficult to quantify first particle impacts into the curved side walls after a straight test section introduce some uncertainty but result in similar or in the worst case slightly higher impact forces compared to snow surface impacts. Based on the above discussion, we assume that the mechanical stresses affecting the snow particles in the curved section are comparable to real natural snow transport situations. A more in-depth analysis of the wall-impact forces would require detailed simulations or particle tracking measurements, which is beyond the scope of this work.”

Huang, N. and Wang, Z.: A 3-D simulation of drifting snow in the turbulent boundary layer, The Cryosphere Discuss., 9, 301–331, https://doi.org/10.5194/tcd-9-301-2015, 2015.

Does the stokes number change at the curve?

We removed the discussion of the Stokes number due to the reasons discussed in your previous comment.

What are the streamwise/spanwise drag forces and can they account for the acceleration necessary to bend around the curve and reduce wall impact forces?

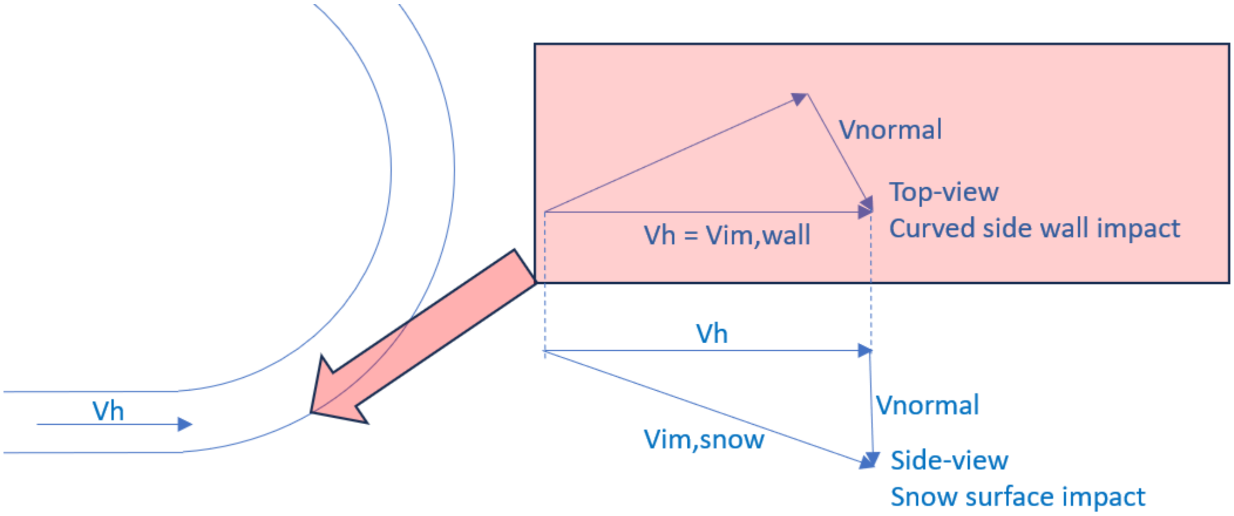
A reliable estimate of the initial impact forces at the curved side walls after the straight sections would require an in-depth study of the particle size/shape defining drag forces, of the flow field and particle trajectories, of the side wall surface roughness and hardness, ideally using CFD or LES simulations or particle imaging techniques. We argue that this would be way beyond the scope of our work and not necessary at this point as shown in the discussion above.

In addition to clearing up the physical processes in the above narrative you have provided, can you explicitly calculate what the impact forces against the wall are?

Calculating reliable estimates of impact forces would require PIV or LES characterization of the impact/ejection angles and velocities that depend on the surface hardness and roughness.

Why is the amount of force exerted on the particle by the wall during the first impact after the straight section as the particle enters the curve not notable (e.g. when the streamwise velocity, not the vertical velocity, is a potentially major contributor to the particle-wall momentum balance)?

As discussed in the new/revised paragraph, we do not argue anymore that it is not notable.



According to the above illustration, a particle with a horizontal velocity Vh is assumed for both the snow surface and wall impact. The impact velocity Vim,wall is actually lower for the curved wall impact compared to the snow surface impact (Vim,snow). The wall normal velocity Vnormal is in this case 10% lower for the curved wall impact compared to the snow surface impact. However, this analysis considers a perfectly horizontally flying particle for the wall impact. If a vertical component is added to the curved wall impact, the hypothenuse in the red box will be something in between Vh and Vim,snow, while the angle will only marginally change, and the resulting wall normal impact velocity will become similar to the snow surface impact. Thanks to the small impact angles in the curved sections of max. 25° that are very similar to the snow surface impacts, that fragmentation is not enhanced but similar at these first impacts into the side walls.

We added one sentence to account for this analysis:

L345:" Geometric vector analysis revealed similar wall normal velocity components for the snow and the curved wall impacts."

Presumably an upper bound for this impact can be calculated by using the radius/curvature of the wall to get a maximal first impact angle (that's what we did, resulting in the 5°-25° impact angles) and the maximum particle speed (to get an impact force, we need to know the energy dissipated at the impact which is not measured, and which depends on ejection angle/velocity), and compare that to vertical velocities in the straight section? My concern here is that the horizontal speed is still likely much higher than the vertical and small impact angles may not be sufficient to account for that.

Please see illustration and comments above. The particle impact velocities and impact angles are similar in both cases.

Are the curved wall impact forces 2-3 times smaller than in the straight section because of the curve of the wall and the actual angle of the surface tangent to the curve?

Sorry for the confusion. We found 1-2 (previously 2-3) orders of magnitude smaller centrifugal forces along the entire curved section, not impact forces at the first impact. The range of impact angels (5°-25°) of the first impact was indeed calculated from the surface tangent at the curved section.

Is this where the 25-30 degree (maximum!) impact angle comes from, or is that once the particle is further along in the tunnel?

Yes, please see answers to your comments above. We change to provide the whole range of 5°-25° to highlight that most impact angles are well below 25°.

I don't doubt that the experiment has measured some interesting processes that may be evident in nature, but I still would like the authors to make an effort be quantitatively transparent and rigorous when they argue these curved walls have a negligible effect.

We totally agree and therefore added corrections and more information to be as transparent as possible. Thanks for making us digging deeper into the effects of the curved sections of our RWT, which certainly was necessary and helped to improve the quality and discussion of our results.

Thank you again for allowing me to review this novel and exciting piece of research!

Thank you very much for your valuable time and comments that certainly helped to improve the quality of the article!