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The Cryosphere Discussions

Interactive comment on "Wind-packing of snow in Antarctica" *by* Christian Gabriel Sommer et al.

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Wind-packing of snow in Antarctica

The manuscript presents a novel data set and focuses on a fundamental open question in the wind redistribution of snow literature. A better understanding of the evolution of the snow surface during prolonged transport is of significant importance. However, the analysis in the presented manuscript could use some advancing and the conclusions are only loosely supported.

Page 1 Line 4: Misuse of the word 'topology.' You are only speaking of the topography, and at a scale of 10 cm. Topology is also not mentioned later in the actual manuscript. (https://en.wikipedia.org/wiki/Topology)

Page 2 Line 1: Clarify how the drifting snow events resembled your wind tunnel ex-

periments earlier in the manuscript. The similarity described in section 4 is lacking. It appears the only similarities were snowfall without wind, and a slow increase in average wind speed.

Page 2 Line 24-25: What sort of measurement error exists for these snow surface hardness measurements? This is of considerable concern as the snow surface is notoriously difficult to measure accurately with an SMP. This error quantification should be included in the subsequent analysis and correlations.

Page 3 Line 2: Why was the wind direction not measured with one of the 4 anemometers? If you insist on using dune orientation as a meteorological measurement, what is the response time of a dune reorienting? It is stated this was an old dune. How much can we trust this orientation for the current wind events.

Page 3 Line 10 (and throughout): At what height were the SPC measurements made? It is inaccurate to say there was no "drifting snow" if the measurements were made above any possible heights of transport. Additionally, please define "drifting snow." Does this mean saltation? If so, the measurement height of the SPC is critical.

Page 3 Line 13: "was about" not "were about"

Page 3 Line 24: It is not obvious in figure two that there are barchan dunes "everywhere." Please highlight the dunes (and distinguish from Zastrugi) or remove this sentence.

Page 4 Figure 1 (& Page 7 Line 6): There is considerable time lapsing after the "main event" during which there was "no drifting" and subsequent SMP measurements were made. The conclusions in the paper that it was the "main event" that contributed all of the hardening, and not the long period of "no drifting" between SMP's, is not justified unless this temporal span is thoroughly addressed.

Page 4 Figure 1: What is this measurement of windspeed? What length average? What instrument? What height? Which location? Reconsider units for displaying inten-

sity of snow mass transport. Kg m^{-3} s^{-1) would be more insightful.

Page 5 Figure 2: It is unclear why this figure was included as it does not add anything to the manuscript that is not already included in Figure 3.

Page 5 Line 2: Fix the sentence that begins with Zastrugi. The citation is improperly included and the sentence is not clear.

Page 5 Line 5: It is not clear from Figure 2A that MOST barchan dunes are shallow. There is one site-specific example.

Page 5 Line 10: This is another fundamental issue in the paper: all the SMPs were disregarded where there was old snow surface was exposed. However, this is very important information as it is a long time asymptotic-like behavior of what will happen with prolonged scouring and "drifting." If there was no change in SMP hardness at these points, show it. If the underlying snow surface hardened even more during prolonged drifting, that is important to know as well. If there was the unlikely softening of the old surface, that is important to know as it puts the other surface hardness measurement in context. If the hardness is uniform surrounding the dune, this could also be used as a very informative normalizing value. The old surface was no doubt evident in the SMP measurements, even when the fresh snow was accumulating, as the SMP gives a profile.

Page 6 Figure 3: Again, there needs to be some indication of the temporal evolution of the measurements. They span many days, and this information is and implies that the hardness comparison is at one time step.

Page 6 Line 3-6: See previous comment about discarding data.

Page 6 line 10-Page 7 Line 1: The current "distance to tail" measurement is imprecisely defined. Is this the Euclidean distance or simply the downwind distance? Either way, the crest is parabolic and thus the distance from the tail is not a measurement of proximity to the crest as implied in the previous sentence. A crest is identified in Figure

2. Use this as a line from which to measure up and downwind. It can then be marked where the tail is with a different colored circle in the new figure 4.

Page 7 Figure 4: If you are going to use the correlation, show the linear regression on the plot.

Page 7 Line 4: The p-value is small, but there is essentially no correlation. This analysis appears prematurely presented. Surely there is a stronger way to justify the connection. A clustering analysis could be very beneficial for this much scatter.

Page 7 Line 7-8: Again, the temporal variation in SMP measurements needs to be included in this analysis and on the figures. There is simply too much time elapsing with windspeeds above transport thresholds to lump all these measurements in together. Even more beneficial than what is presented would be the evolution of the hardness!

Page 7 Line 20: Define drifting for the "no drifting" group. Again, SPC height above saltation layers is important to keep in mind here.

Page 8 Line 10: Again, this correlation is a bit of a stretch, especially with the enormous time elapsed during measurements. If you remove the seven points in the top left corner you would get a positive correlation and negate your results! Explain why these points are so far away from the rest of the cluster.

Page 8 Line 13: Is there evidence the snowfall was homogeneous? A TLS scan to justify this? What about preferential deposition around the old dune?

Page 9 Figure 6: The significance of conclusions drawn from this scatter plot should be significantly reconsidered. A correlation coefficient of -0.26 is meaningless. And what to think of the measurement error of the SMP for surface hardness?

Page 9 Line 1-9: Please expand on the significance of including this paragraph. The wind tunnel comparison appears qualitative at best and at present states that windier conditions result in more wind crusts.

Page 9 line 11: The tail area is very wind exposed! Yet it is softer than the crest? This contradicts your conclusions of the importance of wind exposure.

Page 10 Line 10-12: The conclusions are a bit overstated or inaccurate. This is absolutely not the first time snow redistribution has been quantified in Antarctica, let alone by this institute. This is also not a measurement of "reorganization" as the original location of the snow measured after drifting is not know. The measured changes in associated hardness indicated that during wind transport of snow, there are hardness changes. The "invaluability" of these conclusions is not displayed in the manuscript.

Other comments: As particle size is available from your SPC, it would be very beneficial to see the connection between particle distributions changing over time and the increase in snow surface hardness. Undoubtedly, the smaller grains will impact the surface with less momentum, but will also pack into the surface to create crusts. What connection exists between surface hardness and size distributions in your data?

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