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

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General comments:

The manuscript presents snow hardness measurements performed in East Antarctica conjointly analyzed with meteorological measurements and snow depth changes to address the wind redistribution process during a drifting snow event and associated snow hardening. This is of valuable interest since only few is known about wind redistribution of snow and associated processes and models would certainly benefit from such observations to develop and evaluate parameterizations in this field. However, even though the manuscript is concise (the format is more that of a brief communication rather than a full-length article) and generally well written, the proposed analysis is incomplete and some of the main conclusions still need to be supported by sounder arguments before publication. In particular, the negative correlation between the windexposure parameter and hardness change relies only on a few data points (7 out of 68), making (together with the different environmental conditions, low correlation coefficients and disparity in the numbers used for comparison) the analogy made with the wind tunnel experiments not really convincing.

Specific comments:

1. P1, L16: surface mass balance (not mass balance).

2. P1, L16-17: I understand what you mean but strictly speaking, this sentence seems to describe wind hardening as a deposition process, while its role in terms of surface mass balance is more to prevent further erosion of snow after deposition. Could you reformulate to avoid confusion?

3. P1, L17: This sounds a bit restrictive, as for instance sintering through thermal metamorphism, the occurrence of melting and refreezing or the occurrence of rainfall can also prevent remobilization of snow.

4. P1 L17-19: What is the measurement height of the SPC? How far from the surveyed area is the blowing snow station? This could be of critical importance when interpreting the SPC data (including particle size distribution) since drifting snow is a highly spatially variable process related to highly spatially variable surface snow properties (as shown in Fig. 3).

Note: for a matter of uniformity, use either blowing or drifting snow (drifting snow seems more appropriate) to refer to the measurements as well as to the process itself in the whole manuscript, and gives a brief definition of drifting snow (saltation, height of interest etc..).

5. P2, L25: Is there any mean to quantify the various uncertainties (evoked in the conclusion; see P10, L4) related to the hardness measurements? When possible, you could for instance group the measurements acquired in close locations at the same time to compute a mean value and a standard deviation.

6. P3, L4: Is this direction consistent with the wind directions measured by the Young wind vanes during the main drifting snow event, or with the sastrugi orientation (if measured)?

7. P3, L10: Change “reaching” for “exceeding”.

8. P3, L16: The event seems to involve negligible mass fluxes. You could remove the sentence. As you say later in your description, there is also a small drifting snow event of very low magnitude in early January, but this time qualified as “almost no drifting snow”. Please clarify why you consider the first one and neglect the other, or use similar terms to describe them. Again the height of the SPC could help to interpret the magnitude of snowdrift events, as the drifting snow concentration decreases exponentially with height.

9. P3, L22: or simply this is the hardness of the underlying old snow surface, without being necessarily linked to any deposition event. Irrespective of their “age”, drifting and thus unbounded snow grains need to be packed once deposited before exhibiting significant hardness. Hardening also results from changes in the structure of snow with time and temperature. This is something generally not discussed enough in the paper. You should also show and include an analysis of the temperature time serie when discussing the change in hardness over time.

10. P3, L23: Surely this is a huge increase when compared to such a small transport amount. The comparison here is not necessary.

11. P3, L24: Do you mean than the barchan dune formed covers most of the sampling area? I only see one barchan dune on Fig. 2. Is the area covered by the TLS large enough to support that other barchans formed “everywhere”?

12. P4, Fig. 1: change “were” to “where” in the caption.

13. P5, L3: When? As there is only one drifting snow event strong enough to cause erosion of snow, it seems that the dune and the sastrugi formed during the same event.

14. P5, L5-6: Are there field reports mentioning numerous barchans dunes scattered over the whole study area? I agree that the bedform on Fig. 2 resembles a barchan dune, but this term refers to a specific morphology that is not clearly encountered on the other deposition areas evidenced by the TLS scans, at least from Fig. 2 alone.

15. P6, L6: As switching back and forth between Figs 1 and 3 is needed to follow your analysis, the use of identical symbols (triangles, circles, squares) that do not contain the same information in both figures can be confusing. Please use different symbols.

16. P8, L10: Figure 6 mainly shows highly scattered data (a determination coefficient of 7% has no signification). Your negative correlation (which serves however as one the main conclusions of the study) relies on only 7 points (top left corner) out of 68 points. Why do these 7 points locate out of the cluster? Do they correspond to a particular location on the dune?

17. P8, L13: If the atmospheric conditions cannot be compared directly (see P9, L9) and the measurement conditions are quite dissimilar (see P9, L14), thus the observed event is not "a close approximation" of your wind tunnel experiments.

18. P9, L11: This is only poorly supported by Fig. 4, and is somewhat confusing since upwind parts of roughness elements are supposed to be more exposed to wind than downwind parts, thus more subject to wind hardening.

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