In this paper, the authors describe a set of innovative data acquired by Particle trecking Velocimetry during blowing snow events. Even if Particle Trecking Velocimetry is a classic wind-tunnel method for studying drifting particles, this is the first investigation to measure outdoor snow particle flux and velocity. It is really challenging and interesting and is a potential source of new knowledge. Such experiments should be made known to the scientific community. But even if the paper is potentially interesting, it seems that it is not suited for publication in its current state. I have several suggestions for improvement if the authors would like to resubmit the manuscript.

- (1) The results are not described in sufficient details and conclusions are often not supported through the material presented. The authors must be aware of what information the data can provide and what they cannot provide. It is a persistent problem throughout the whole text. Sometimes, the new findings highlighted in the paper seem questionable.

We have expanded the discussion of results, added new data to back this discussion and included new results that are strongly backed by analysis of this data. We have clarified the findings from the data and removed speculative discussion or identified it more clearly in the discussion section. We are very confident in the veracity and certainty of our results and note that as these come from outdoor measurements, they are more likely to be representative of natural blowing snow than are wind tunnel simulations under less turbulent and more idealized two phase flow conditions.

P7-L10 : From figures 3a-g, it is not evident that the constant particle velocity gradient is limited to a height below 10 mm, at least for me.

After consideration, we agree with this comment and so have expanded our analysis with new data and now Figure 3 has been restructured using terrain following coordinates. Now, particle velocity profiles of 12 recordings (9 new) over three nights of recordings are presented encompassing 283 seconds and 513,000 frames. From these we have found that the horizontal velocity profiles of ascending particles start to diverge from linear plots above heights that vary from 8 to 15 mm.

Moreover it seems that the velocity gradient estimated by linear regression includes all measurements points? Is it the case?

In the new figure, the velocity gradient has been estimated using particles in the first 10 mm of the terrain following coordinate system.

How do the authors estimate that this 10 mm transition corresponds with the upper extent of the low-energy population?

As noted above we no longer conclude that there is a 10 mm threshold. Aeolian transport exhibits a continuous spectrum of particle velocities and so the gradient of motions from "low-energy" to "high-energy" does not contain a discrete step. Using the Ho et al. (2014) definition, derived for saltating sand in a wind tunnel, where the two populations are distinguishable if grains are either responsive or irresponsive to changes in 'wind strength,' we found that nearly all snow particles outdoors are responsive to 'wind strength'. This manuscript has been restructured to discuss this continuum of motion through examination of particle velocity distributions (as in Ho et al. 2012) and identifying the region of transition from a creep dominated particle population to higher energy grains. This highlights the important role of creep in forming the lower boundary condition and "source" for saltation.

P8-L14: The 10 mm threshold delimitating variances of blowing snow particles is not clear again. The decline in variances is not so pronounced. For sure it is difficult to directly estimate this value from the graph. Some orders of magnitude could be useful for the readers.

We agree. The variance argument has been eliminated and replaced by data in a new Figure 3.

P9-L4 : I did not understand how the results help to illuminate the shift in snow transport mechanics when transitioning to particles in the tail of particles velocity distributions. The authors are expected to provide more explicit demonstration.

This is now explicitly demonstrated as follows: This and other studies have shown that the density of flow decreases exponentially with height. The two particle velocity histograms (above and below 4mm) indicated at what velocities the majority of particles were moving in each height region. 4 mm was an arbitrary divide to separate near surface and above surface flow regions. Very close to the surface in the densest part of the flow, the flux was dominated by slow moving ("creep") particles, but also with a contribution of relatively fewer high-energy ("saltation") particles that has not been described before. Above the snow surface, there was a continuum of particle velocities that was dominated by high-energy particles. The increasing proportion of high-energy particles with distance from the surface is due to the need for a greater velocity to reach greater heights on a ballistic trajectory from the surface and the subjection to stronger winds with increasing distance above the surface.

We were illuminating the point that high energy particles are also counted at the <4mm level as well. Therefore typical saltation measurements that limit data above the creep layer are missing critical data, whereas, near-surface measurements include both populations. Additionally, there is an intrinsic coupling of low-energy ("creep") population with high-energy ("saltation") particles as they both occupy the same spatial location, and the creep layer feeds upper regions of saltation as mentioned in the new discussion of Figure 4.

P10 : The authors have to explain in details how concurrent streamwise wind measurements show penetration of a turbulent sweep.

Figure 4 has now been adapted to show time periods of significant ejection and sweep using Quadrant Analysis with a hole size of 1. There is an ejection (triangles) that precedes the main sweep (dots) event at 8 seconds. Sweeps by definition indicate positive fluctuations in streamwise wind speed with negative fluctuations in the vertical. As the sweep first became evident at 2 m and then at 0.4 m height, this motion clearly penetrated from above.

The discussion in the manuscript discusses these issues at length now.

Why is it best to base our reasoning on streamwise wind measurements instead of Reynolds stress measurements?

This is an excellent question. This empirical finding from our data agrees with conclusions from other field and wind tunnel sand studies (Bauer et al., 1998; Sterk et al., 1998; Schonfeldt and von Lowis 2003, Leenders 2005). Strong ejections (high Reynolds stress) events at 2-4 second, and >12 seconds did not result in blowing snow transport. Instead it was the positive fluctuations in streamwise wind speed that resulted in flux. The nature of turbulence near the surface has an influence of transport initiation and a simple Reynolds stress cannot fully describe this, whilst near surface wind speeds can better reflect sweeps and ejection events. Further analysis of this phenomenon is of interest but would be a very extensive addition to this paper and so we now call for it in the conclusions and suggest it for future research.

P10-L4 (and figure 4): New threshold values (4 mm and 8 mm) are given in this paragraph. Without any additional measurements, I see absolutely no reason why the authors change their tune. The authors previously compared the 10 mm threshold with values obtained by Ho et al., 2014, which correspond to the limit between saltation and reptation (particles are divided in two populations on the basis whether or not they rise high enough to be affected by the flow strength).

These three arbitrary heights are <u>not thresholds</u>, they were chosen to illuminate the subtle differences in particle transport and the continuum of motion as grains begin motion and begin bouncing to greater heights as wind speeds increase. They do not indicate hard thresholds of "creep" versus "saltation" regimes.

This will be reflected more clearly in the text to avoid such misinterpretations. In the revised text we have adopted the Ho et al. (2012) probability distribution explanation of "long tails" corresponding to the population of high energy grains. Plots of Gaussian and log-normal PDF's of lift-off velocities are included in the revisions.

On which basis (other than a qualitative approach for one blowing snow event) do the authors decide that 4 mm is the limit between creep and saltation? It is really confusing.

This arbitrary height is *<u>not a threshold</u>*, *it was chosen to illuminate the subtle differences in particle transport and the continuum of motion as grains begin motion and begin bouncing to greater heights as wind speeds increase. To clarify, we have revised the text to more clearly show a transition region between the "creep" dominated and "full saltation" dominated flow regions. This was already mentioned several times in the text. The emphasis is that the creep layer contributes substantially to blowing snow flux and is the source of saltation.*

P9-L16 : How do the authors consider that particles are in creep? by the position of the particles (i.e a particle seen below 4 mm is considered as being in creep ?) If at a given time a particle is at this position, it doesn't mean that later it will not be able to rise high enough to be affected by the flow strength.

As explained above, we do not use a height threshold, but examine particle speed and consider a continuum of motions between classical creep and saltation. The histogram in figure 3 was designed for this purpose: low-energy particles near the surface are more likely to be "creep". (page 9 second paragraph). The difference line on P9 – L16 identifies precisely the low-energy particles present near the surface, whereas the high-energy particles that are present at both heights are not counted. This is emphasized clearly in the revised paper.

(2) It is surprising that the results are not discussed taking into consideration key measurement uncertainties (related to the position of surface bed and to the distance between PTV measurements and ultrasonic anemometer). This way, analysis could be enhanced.

Natural blowing snow as found outdoors has a naturally uneven bed – in contrast to its common representation in wind tunnel studies. Uncertainty with respect to the position of the bed is briefly addressed with respect to the line indicating "influence of microtopography" in figure (2, 3). The issue of surface influence is now considered quantitatively by using a terrain-following coordinate system.

Measurement uncertainties with respect to the distance between PTV and ultrasonic wind measurements can be considered negligible for time-averaged quantities as they are occurring at similar heights. Assuming Taylor's frozen turbulence hypothesis, and utilizing the mean wind speed during recordings, a lag time of approximately 1/3 of a second could be expected, much shorter than the duration of any given recording. For instantaneous measurement comparisons, these lag times are discussed in that section.

The February 3 and March 3 laser measurements were made at closer distances (0.5 and 0.33 m, respectively) to the CSATs, perpendicular to the direction of wind flow. The effect of this orientation on comparing turbulence to transport measurements is mentioned in the revised text.

- (3) The time series need to be extended, as –apparently- there are much more data available. However, it would be useful to know how many other cases (if any) could have been selected and why they were not presented here. I would encourage the authors to present more of the valuable data. If not, the research paper must be considered as a Brief communication (http://www.the-cryosphere.net/about/manuscript_types.html)

We only wanted to include data for which we had the highest confidence. Other videos from the 2014-2015 field campaign were not presented because there were either not sufficient particles in transport to give meaningful average profiles, or the particle flows were too dense and obscured illumination. We have now added new data from the 2016 field campaign – there is now more than six times the length of the original data available for analysis and has allowed more certain generalization of the results to inform the conclusions.

-relating to the item 3

Neutral stability did not occur during the field campaign as it can be seen on table 1 (the Reynolds stress is not constant with the height). It is a pity because neutral con- ditions are quite usual on the site (80% of the

192 hourly periods studied in Helgason and Pomeroy, 2005).

The Helgason and Pomeroy (2005) Kananaskis valley bottom site is 14 km away and 600 m lower than this high alpine valley site and so stability is not really comparable between them. This distinction is made clearer in the revised paper. But it was very useful that you pointed this out. Correcting a numerical error now shows, using both Monin-Obuhkov and the bulk/gradient Richardson number approaches, that there are indeed near-neutral (slightly stable) conditions on the nights of Mar 23, Feb 3 and Mar 3. All three nights are now part of the analysis and discussion and are further detailed in the revisions.

So, authors can't calculate the aerodynamic roughness which becomes a function of height.

Exactly, this is one complication we wished to highlight for use of log-law models that drive blowing snow in complex terrain. The discrepancy in roughness length measurements is evident in Table 1. There are somewhat more consistent values of z_0 *on Feb 3 and Mar 3 for some of the recordings, now included in the new Table 1.*

What are the Richardson numbers for these experiments? Is there any other data under neutral stability over the course of the campaign?

Plots of the Richardson numbers for the near-neutral conditions on Mar 23, Feb 3, and Mar 3 are supplied as well as the Min, Max and Mean values during the periods of active blowing snow recording (highlighted blocks in the time series). Slightly stable conditions occurred on all nights, and this correction is included in the revised paper. Monin-Obukhov length also indicates slightly stable conditions during all recording periods.

For recording 2 there is a strong difference between u* estimated by eddy covariance method at a height of 200 cm and a height of 40 cm. Is it possible for the blowing particles to disturb the measurements?

Yes, it appears that was the case. While the noise did not greatly affect the mean wind speed (there is reasonable agreement between 40 cm and 200 cm averages), there was sufficient noise in the signal to obscure the turbulence measurements. This is noted in the revised paper.

What are the drifting snow fluxes measured during recording-only period and during the 15 minutes surrounding each recording.

Equivalent diameters of individual blowing snow particles were measured in each frame, allowing a spherical estimate of the blowing snow volume in the 2mm wide plane of illumination. This is similar to the sand and snow studies of Creysells et al. (2009), Guo et al. (2013), and Paterna et al. (2016) among others. The time series of volume fraction was then multiplied by the density of ice by the average particle velocity for each time step to obtain a mass flux of blowing snow Q_s *in* $kg\ m^{-2}s^{-1}$*. Averaging these values over the duration of a recording gives a mean blowing snow flux rate for given wind and snow conditions. Values of* Q_s *are now included in the wind characteristics Table.*

There are quite unusual results which need to be commented (for example high value of roughness which can be smaller when estimating by flux-profile estimation techniques suggesting that the mean wind profile was in equilibrium with the snow surface, however the turbulence was not (Helgason and Pomeroy, 2005).

We do not attempt to estimate roughness from the wind speed profile given the violation of fully developed log-linear law assumptions. This is quite different from H and P's situation where time-averaged log-linear profiles appeared to be valid. However the high roughness lengths due to high turbulence appear to be characteristics of both sites due to their mountain location. This is commented on further in the revised paper.

- (4) The text could be more concise and focused. Similar points are discussed in several places of the text.

-relating to the item 4

P7-L10/20 : Paragraph about Ascending particles
P7-L21/P8-L8 : Paragraph about Descending particles
P8-L9/P8-L14: Paragraph about Ascending particles. It is a little bit confusing for the reader-

Thank you for this excellent suggestion. These sections have been reorganized.

- (5) Moreover the nature of discussion should be more quantitative than qualitative.
relating to the item 5

Figure 5 by itself is not an evidence that tumblons eroded many smaller crystals from the surface or shattered themselves and immediately became saltating grains, depending on impact velocity. Where are the measurements to show the effect of impact velocity? Moreover impacting particles may travel transverse to the plane of light and may not be included on the second image. Conclusions must be based on a statistical approach.

Tumblons have not been identified before in two-phase flow and their initial description here is necessarily mainly qualitative. The point of their inclusion in the paper is to distinguish the types of motion found in natural, outdoor blowing snow from that found in sand or found in wind tunnels. Figure 5 has been removed and the reference has been changed to supplemental videos, with overlain PTV vector fields, of tumblons that both shatter upon impact and tumble along the surface with. To better quantify them, we now compare the impact velocity of the shattering tumblon and that which remained whole.
Shattering Tumblon: $< u, v > \approx < 2.46, -.43 > ms^{-1}$
Rolling tumblon: $< u, v > \approx < 0.6, .1 > ms^{-1}$ *(depending on what is considered center of mass).*

- (6) Papers supporting the reasoning should be properly referenced and used. Otherwise, it puts a doubt into readers' minds.
-relating to the item 6
P3- L7: Ho et al., 2011 does not address grain velocity distribution functions

Thank you - corrected.

P8-L27: Ho et al., 2012 deals with Particle velocity distribution in saltation transport. So when speaking about number density, the authors have to use the right reference (It is probably Ho et al., 2011).

Thank you - corrected.

Ho et al., 2011 explained that the particle volume fraction decreases with height at a given exponential rate in saltation layer. If the authors want to compare their results with Ho et al., 2011, they have to limit the analysis to the first centimeter and to draw the result in the same manner as Ho et al., 2011 (figure 8) with an inset including the characteristic decay length.

This has been done for 12 videos over the nights of Mar 23 2015, and Feb 3 and Mar 3 2016. Because our study is concerned with highly intermittent conditions, we profiled average particle number flux (number of particles in transport multiplied by average particle velocity at that height). With 100% tracking, in equilibrium wind tunnel conditions, this value is theoretically equal at every time step. As our time series include periods of developing and no flux, a temporal average of particle fraction would not be comparable to the results of Ho et al. (2011). Instead, we limit our attention to only periods when transport was occurring. Notably, this means the characteristic decay lengths are not "apples to apples" comparable with Ho et al. 2011, but the fraction of total flux at each height in our profiles is representative of a plot that could be generated by Ho et al. (2011) data. Analysis of changes between our decay lengths in the style of Ho et al. (2011) is possible for our data set has been conducted.

This is discussed in more detail in the revised paper.

Moreover the authors base their analysis on the fractional particle number flux whereas Ho et al., 2011 base their analysis on the particle volume fraction. If both results are compared, the authors have to take into account the volume of particles which can vary according to the wind speed.

The methods of obtaining the two data sets, and the values under comparison are now more clearly distinguished. The difference between mass flux (or volume fraction) and number flux has been mentioned in the text with respect to the ability of the tracking algorithm to capture the flux: "As noted elsewhere (Creyssels et al., 2009), particle-tracking algorithms in the densest regions of saltation are still problematic, and thus these are conservative underestimates of near-surface flux." –page 9.

This is discussed in more detail in the revised paper.

P2-L29: Schmidt (1980) instead of Schmidt (1986)

Thank you - corrected.

P11-L18/24: Sugiura and Maeno, 2000 made a distinction between horizontal restitution coefficient and vertical coefficient restitution, which are different from the restitution coefficient calculated from the authors. Moreover the calculation method completely differs. The authors should make it clear.

We have now noted that we used a different method, and that because of the density of flow a statistical method was necessary: A particle by particle restitution was not possible because the tracking software used often re-identified a rebounded particle as distinct from an incoming particle. Therefore a bulk statistical approach was used, what was the average incoming versus the average outgoing velocity. Looking in a region >1 particle diameter above the surface, we hoped to measure a rebound coefficient for truly rebounding particles, and not including reptating grains, which would artificially lower the coefficient. In the revised paper, we have lifted the region to 7.5 mm ($\pm$ 2.5 mm) *in terrain following coordinates above the surface.*

Additionally, because we are dealing with a natural surface topography, not a smooth bed as found in wind tunnel studies, parsing out horizontal and vertical restitution coefficients is not beneficial as the impact angle depends largely on the location of impact. A more general approach of E_xy does not require distinguishing the location of impact and provides information on kinetic redistribution and grain-bed impact elasticity useful for momentum balance concerns.

P7-L19: What is the numerical value of the transition height obtained by Ho et al., 2014 (2zf) ? As far I can see from Figure 3 the Bagnold focus point zf is around 8 mm. Considering uncertainties in relation to the choice of 10 mm threshold both values are close together.

The Bagnold focus point is unavailable for us to identify, as the wind profile did not remotely follow a log-linear scale as would be found for a prairie sites (Pomeroy and Gray, 1990). This is expected given the non-steady state conditions. Therefore, a log-linear profile with a linear offset (addition of U_f term) does not fit either. Attempts to fit it with and without the Bagnold focal point produced focal point values ranging from 11 mm to nearly 6 m!

What are the Shield numbers of the snow particles in the experiments? Ho et al., 2014 remain that the results have been obtained in a finite range of Shields number from 0.04 to 0.2.

Shields numbers ranged from 0.006 to 0.2. This is now indicated in the wind characteristics table.

Bauer, B. ., J. Yi, S. Namikas, and D. Sherman (1998), Event detection and conditional averaging in unsteady aeolian systems, *J. Arid Environ.*, *39*, 345–375.

Creyssels, M., P. Dupont, a. O. El Moctar, A. Valance, I. Cantat, J. T. Jenkins, J. M. Pasini, and K. R. Rasmussen (2009), Saltating particles in a turbulent boundary layer: experiment and theory, *J. Fluid Mech.*, *625*, 47–74, doi:10.1017/S0022112008005491.

Guo, L., and N. Huang (2013), Wind tunnel studies on the vertical emission of sand grains from surface, in *AIP Proceedings 1542*, vol. 1087, pp. 1087–1089.

Helgason, W., and J. Pomeroy (2005), Uncertainties in estimating turbulent fluxes to melting snow in a mountain clearing, in *Proc. 62nd Eastern Snow Conf*, pp. 129–142.

Ho, T. D., A. Valance, P. Dupont, and A. Ould El Moctar (2011), Scaling laws in aeolian sand transport, *Phys. Rev. Lett.*, *106*(9), 4–7, doi:10.1103/PhysRevLett.106.094501

Ho, T. D., P. Dupont, A. Ould El Moctar, and A. Valance (2012), Particle velocity distribution in saltation transport, *Phys. Rev. E - Stat. Nonlinear, Soft Matter Phys.*, *85*(5), 1–5, doi:10.1103/PhysRevE.85.052301.

Ho, T. D., A. Valance, P. Dupont, and A. Ould El Moctar (2014), Aeolian sand transport: Length and height distributions of saltation trajectories, *Aeolian Res.*, *12*, 65–74, doi:10.1016/j.aeolia.2013.11.004.

Paterna, E., P. Crivelli, and M. Lehning (2016), Decoupling of mass flux and turbulent wind fluctuations in drifting snow, *Geophys. Res. Lett.*, 1–7, doi:10.1002/2016GL068171.

Pomeroy, J., and D. Gray (1990), Saltation of snow, *Water Resour. Res.*, *26*(7), 1583–1594.

Schönfeldt, H.-J., and S. von Löwis (2003), Turbulence-driven saltation in the atmospheric surface layer, *Meteorol. Zeitschrift*, *12*(5), 257–268, doi:10.1127/0941-2948/2003/0012-0257.

Sterk, G., a. F. G. Jacobs, and J. H. Van Boxel (1998), The effect of turbulent flow structures on saltation sand transport in the atmospheric boundary layer, *Earth Surf. Process. Landforms*, *23*(10), 877–887, doi:10.1002/(SICI)1096-9837(199810)23:10<877::AID-ESP905>3.0.CO;2-R.