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Interactive comment on "Near-Surface Snow Particle Dynamics from Particle Tracking Velocimetry and Turbulence Measurements during Alpine Blowing Snow Storms" *by* N. O. Aksamit and J. W. Pomeroy

Anonymous Referee #2

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General points

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

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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.

- (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.

- (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)

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

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

- (6) Papers supporting the reasoning should be properly referenced and used. Otherwise, it puts a doubt into readers' minds.

Example of specific points illustrating general points

- relating to the item 1

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. Moreover it seems that the velocity

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gradient estimated by linear regression includes all measurements points ? Is it the case ? How do the authors estimate that this 10 mm transition corresponds with the upper extent of the low-energy population ?

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.

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.

P10 : The authors have to explain in details how concurrent streamwise wind measurements show penetration of a turbulent sweep and why it is best to base our reasoning on streamwise wind measurements instead of Reynolds stress measurements.

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). 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.

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.

-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 conditions are quite usual on the site (80% of the 192 hourly periods studied in Helgason and Pomeroy, 2005). So, authors can't calculate the aerodynamic roughness which becomes a function of height. What are the Richardson numbers for these experiments ? Is there any other data under neutral stability over the course of the campaign ?

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

-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

-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.

-relating to the item 6

P3- L7: Ho et al., 2011 does not address grain velocity distribution functions

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). Ho et al., 2011 explaine 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. 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.

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

P11-L18/24: Sigiura 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.

P7-L19: What is the numerical value of the transition height obtained by Ho et al., 2014 (2 zf ?) ? 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. 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.

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