This manuscript introduces the measurements of the blowing snow with the Particle Tracking Velocimetry (PTV) and tries to investigate the particle motions near the snow surface. I appreciate very much for the author's effort to apply the PTV technique in the field and successful observations. The attitude should be highly evaluated.

Thank you.

However, as far as I read through the manuscript, I have got the impression that the data shown here is not always valuable for both the drifting and blowing snow research community, in particular, for the accurate modeling.

PTV recordings shown in Figure 2 probably involve the meaningful information, however, I am afraid the surface bed is far from flat and the height difference amounted to 5 mm. It gives the substantial effect on both particle speeds and the wind velocities shown in Figure 3.

Yes, we note the effect of surface "microtopography" on obtaining useable statistics is shown below the dashed line of influence. In the revised paper, we have introduced terrain-following coordinates (see response to Reviewer Comments #2). However, 5 mm rise over 130 mm equates to a little more than 2 degrees of slope. When we are considering blowing snow in alpine terrain, this is quite flat.

It is quite plausible that the reason why the wind speed showed the maximum at 2 to 7 mm

We do not see this in our data.

and why a zero wind velocity zone above the saltation layer exists is related to the bed surface undulation.

A modeled zero wind velocity zone above in figure 3e-f could indeed be related to surface roughness. The issue at hand in the discussion is how in natural conditions, the commonly used log-law will predict erroneous wind profiles. Thus the choice of timescales for mean values and assumptions required for predicting wind speed for blowing snow in complex terrain must be of higher concern.

Bagnold's focal point is hard to refer under such conditions. Further, the friction velocity of 0.08 m/s at Rec. #2 is extremely low; under such conditions the blowing snow, never breaks out and keeps going, even though we take into account the turbulence effect.

The wind data for Rec #2 has been compromised during the recording specific period, hence why the difference between 2m and 40cm values is so much larger than during other recordings. This has been highlighted in the revised text.

On the contrary, at Rec. #3, u* is extremely high. Under such conditions, I suppose the particle concentration near the surface increases largely and it makes hard to distinguish individual particle, that is, no precise particle tracking is available.

True, particle tracking does become difficult and this was mentioned in the discussion of figure 4 – P10 L24, P11 L4

In fact, I cannot agree with you more that the turbulences including ejection and sweep intermittent structures are key factors to initiate the snow particle motion, rather than the time averaged friction velocities, not only in the mountain area but on the flat snow surface.

However, when you would like to set your focus on these issues, you need more specific and detailed analysis based on the high frequency data. Although the quadrant analysis has been tried, the explanation of the outcome is superficial, more detailed analysis related to the particle motion are essential.

Thank you. We have expanded this analysis as well as the amount of data investigated in the revised paper.

Presumably 1m distance between the PTV and the anemometer makes the quantitative comparison difficult?

This distance has a considerably smaller effect in the atmospheric surface layer than in wind tunnels because the eddies driving the turbulent structures are much larger. This lateral distance was chosen to preclude any bluff-body influence of the camera/laser apparatus. Subsequent data has been included with a smaller distance between PTV and anemometer in a spanwise orientation. Please refer to the response to Reviewer #2 Comments.

Further, the descriptions in the discussion and conclusion parts are mostly qualitative and look nothing but a pile of well-known and predictable issues.

We have obtained many similar results to aeolian transport studies in much more controlled wind tunnel environments, this shows how the PTV approach in natural conditions can confirm certain results found in more controlled, but less natural wind tunnel environments. The scientific blowing snow community would benefit from the knowledge that such techniques may be used in nature, as the potential gains from employing these techniques outdoors are considerable, albeit with further quantitative results coming only after "well-known and predictable issues" are first addressed.

More firm conclusions based on the more quantitative analysis are essential. I strongly recommend, first of all, the authors to reexamine the obtained data again and uncover the hiding useful ones.

Data from two more nights of recording have been included in the analysis and discussion in the revised paper. Consideration of these data and further analysis of other data has led to stronger conclusions.

Further, an accumulation of more data will be preferable; in actual, saltation of 400 to 1000μm diameter graupel is a very exceptional case. I cannot believe that such large particles kept saltation under the friction velocity of 0.08 m/s at Rec. #2r.

Addressed above.