Interactive comment on “Radar measurements of blowing snow off a mountain ridge” by Benjamin Walter et al.

Anonymous Referee #1

Received and published: 17 January 2020

This paper presents the use of a Micro Rain Radar (MRR) to investigate the dynamics of snow plumes forming at mountains ridges during blowing events. The MRR, pointing horizontally, was deployed at a mountain ridge above Davos in the Swiss Alps. MRR data were collected during two “pure” blowing snow events (without concurrent snowfall) and one snowfall event. Measurements provided information on the travel distance and the velocities of blowing snow in snow plumes. Snow accumulation in the lee of the ridge was also measured using drone-based photogrammetry.

The subject of this paper is very interesting for the snow community and presents novel measurements of blowing snow characteristics in mountainous terrain. To my knowledge, this is the first study that provides details measurements on the dynamics of snow plumes, which constitute the typical image of blowing snow events in alpine terrain.

First thanks a lot for the very detailed review of our manuscript. I included basically all of your suggestions which were clear and easy to understand and which helped to significantly improve the article!

So far, snow plumes have only been investigated from space (Moore, 2004) or from the air (Geerts et al., 2015).

These references are included now in the Introduction Section:

L66: “Space born images of a huge, about 15 to 20 km long snow plume at Mount Everest have been related to local wind and weather conditions by Moore (2004). Geerts et al. (2015) used airborne radar and lidar data to show that small fractured blowing snow ice crystals may enhance snow growth in clouds.”

Other measurements during blowing events (fluxes, particles size and speed : : :) are typically taken at point-scale (e.g. Naaim Bouvet et al., 2010; Nishimura et al, 2014; Aksamit and Pomeroy, 2016).

These references are now included in the Introduction Section:

L54: “… (e.g. Naaim Bouvet et al. 2010, Nishimura et al. 2014, Aksamit and Pomeroy 2016) … “

These data will be very useful to evaluate blowing snow model in alpine environments. Therefore, this paper should be published in The Cryosphere. However, prior to publication, the author should clarify several points that are listed below. They are followed by more technical comments.

Comments:

Abstract L 11-12: The author should mention that the number of cases studied in the paper is limited. So far, it is not clear in the abstract if the results concern one or several blowing events or even a full winter season.

Good point, thanks!

L14: “Three blowing snow events are investigated, two in the absence of precipitation and one with concurrent precipitation.”

Introduction: As mentioned above, this study brings novelty in the field of blowing snow studies in alpine terrain. However, so far, the introduction of the paper does not reflect enough this general context and lacks an overview of the existing measurement techniques (restricted to a few sentences from L 48 to L 52 in the current version of the paper). I recommend the authors to make the distinction between measurements that are collected during blowing events and measurements that are collected before and after blowing snow events. The first kind of measurements is generally made of point measurements using Snow Particle Counters (Nishimura et al., 2014; Guyomarch et al., 2019) as already mentioned in the current introduction but also using other devices such as high-speed cameras (e.g. Aksamit and Pomeroy, 2016).

With "before and after blowing snow" I guess you mean "before and after precipitation"? I think in our case it is not necessary to separate the existing literature to such two different cases. I think it is more relevant to separate between attempts of measuring and modelling spatiotemporally resolved redistribution of snow and studies based on local point measurements. Furthermore, this study focuses on blowing snow (snow particles in suspension), whereas past studies are often exclusively about drifting (saltating) snow close to the ground and therefore not necessarily relevant. Additional literature, including most of that suggested by the referees, was included in the Introduction Section with a focus on providing more details about the individual studies:

L54: "Naaim-Bouvet et al. (2010) used wind velocity and snow particle flux point measurements at a mountain pass to parameterize and validate a numerical model of drifting snow. Nishimura et al. (2014) measured snow particle velocities and mass fluxes using an SPC and found snow particles being about 1-2 m s^{-1} slower than the wind speed below a height of 1 m. Aksamit and Pomeroy (2016) introduced an outdoor application of particle tracking velocimetry (PTV) of near-surface blowing snow investigating the complex surface flow dynamics. Despite providing valuable knowledge on process understanding, none of those studies provides spatially resolved measurements on larger scales (> 10 m)."

L64:" First attempts of measuring blowing snow across a mountain ridge to estimate additional snow deposition on steep lee-slopes for the local avalanche warning in Davos were presented by Föhn (1980)."

The second kind of measurements usually correspond to distributed measurements such maps of snow accumulation and erosion derived from Airborne or Terrestrial Lidar Scanning or photogrammetry. This is mentioned at L 50 but without any references. These two kinds of measurements are complementary and the MRR used in this study brings a next step since it provides distributed measurements during blowing snow events.

We agree that references were missing here, therefore we added those two:

L62: "Schirmer, M., Wirz, V., Clifton, A., and Lehning, M.: Persistence in intra-annual snow depth distribution: 1. Measurements and topographic control. Water Resources Research, 47, W09516 (16 pp.). https://doi.org/10.1029/2010WR009426, 2011.

Picard, G., Arnaud, L., Caneill, R., Lefebvre, E., Lamare, M.: Observation of the process of snow accumulation on the Antarctic Plateau by time lapse laser scanning, The Cryosphere, 13, 1983–1999, https://doi.org/10.5194/tc-13-1983-2019, 2019"

Several other important studies were also missing, so we added the following:

L68: "Nishimura et al. 2019 recently applied fifteen SPCs and Ultra-Sonic Anemometers on a flat field to reveal the spatio-temporal structures of blowing snow near the surface and explore the interaction with the turbulent flow structures. Several studies simulated wind-affected snow redistribution and accumulation by relating atmospheric wind fields with resulting snow deposition patterns in mountainous terrain (Dadic et al. 2010, Winstral et al. 2013, Mott et al. 2014, Vionnet et al. 2017, Gerber et al. 2017, Wang and Huang 2017)."

I also recommend the author to mention existing studies on snow plumes (Moore, 2004; Geerts et al., 2015) and their main conclusions.
These two studies were added as mentioned above in L66.

In addition, the introduction is missing a paragraph on the MRR technology and its traditional use to retrieve characteristics of solid precipitation. It would be valuable for the reader to know if previous attempts have been made to study blowing snow with MRR (the authors mention such application in their conclusion L 357-358). So far, the term MRR is mentioned for the first time in Methods section.
Thanks for this important comment! We added more information:
L73: "Flow structures around utility-scale 2.5 MW wind turbine have previously been measured by Hong et al. (2014) using a field Particle Imaging Velocimetry (PIV) setup with snow precipitation as the tracer particles. Their results provide significant insights into the Reynolds number similarity issues presented in wind energy applications.
Radar is often used for snow avalanche detection (e.g. Vriend et al. 2013) and to capture avalanche flow structures and velocities. Kneifel et al. (2011) analyzed the potential of a low-power FM-CW K-band radar (Micro Rain Radar, MRR) for snowfall observation, a method that was further improved by Maahn and Kollias (2012)."
L447: "The MRR instrument was also recently tested by the CRYOS group at EPFL Lausanne, Switzerland, for measuring vertical blowing snow velocity profiles and its temporal variability in eastern Antarctica at the site S17 near the Japanese research station Syowa (yet unpublished work in progress), where blowing snow layers can reach a vertical extend of up to 200 m (Palm et al. 2017)."

Finally, the introduction in its present form mainly contains references to papers from the Davos and Lausanne group. There are no doubt that this group has published very valuable contributions in this field but a broader perspective would certainly improve the quality of the introduction.
We agree, so we introduced more than 25 additional (mainly non-Davos/Lausanne) studies in the Introduction Section to provide a more comprehensive overview of this research field. Please see comments above.

P 2 L 50: it is not clear here if the authors are referring here to measurements of blowing snow characteristics taken during blowing snow events or to measurements collected before and after blowing snow events. For example, when they mention radar technology, are they referring to a MRR to collect data during blowing snow events or to a ground penetrating radar to collect snow depth data before and after the event? Same for the LiDAR (see my previous comment).
We agree that this was a bit confusing because we used a LIDAR Laser Scanner and later we mention a cloud physics LIDAR. We changed this sentence to:
L61: "Spatially continuous measurements using remote sensing techniques like radar, for observing blowing snow, in combination with LIDAR (Light Detection and Ranging) or Photogrammetry measurements (e.g. Schirmer et al. 2011, Picard et al. 2019), to capture the spatio-temporal snow depth variability, may thus provide valuable information for improving our understanding and modeling of drifting and blowing snow and its spatial variability."

P 3 L 70-75: general references on the MRR technology and its application in meteorology are missing.

We agree, so we added some more references in the methods Section (L97 and 148):

- *Peters, G., Fischer, B. and Andersson, T.: Rain observations with a vertically looking Micro Rain Radar (MRR), Boreal Env. Res. 7: 353ñ362. ISSN 1239-6095, 2002.*
- *Peters, G., B. Fischer, H. Münster, M. Clemens, and Wagner A.: Profiles of raindrop size distributions as retrieved by Microrain Radars, J. Appl. Meteorol., 44, 1930–1949, 2005.*
- *Tridon, F., Van Baelen, J., Pointin, Y.: Aliasing in Micro Rain Radar data due to strong vertical winds, Geophysical Research Letters, Volume 38, Issue 2, CiteID L02804, 2011.*

P 4 Figure 1: a map of the area would be useful to better understand the experimental setting and the location of the MRR with respect to the surrounding topography. Figure 2 is not sufficient and only shows the immediate surrounding of the MRR location. The authors could also show on this map the location of the transect presented in Fig 1b.

We agree. We added a map of the surrounding topography to Fig. 2 so the reader gets an idea on the mountain ridges in the close vicinity. We also added a line in Fig. 2a indicating the location of the transect from Fig. 1 b.

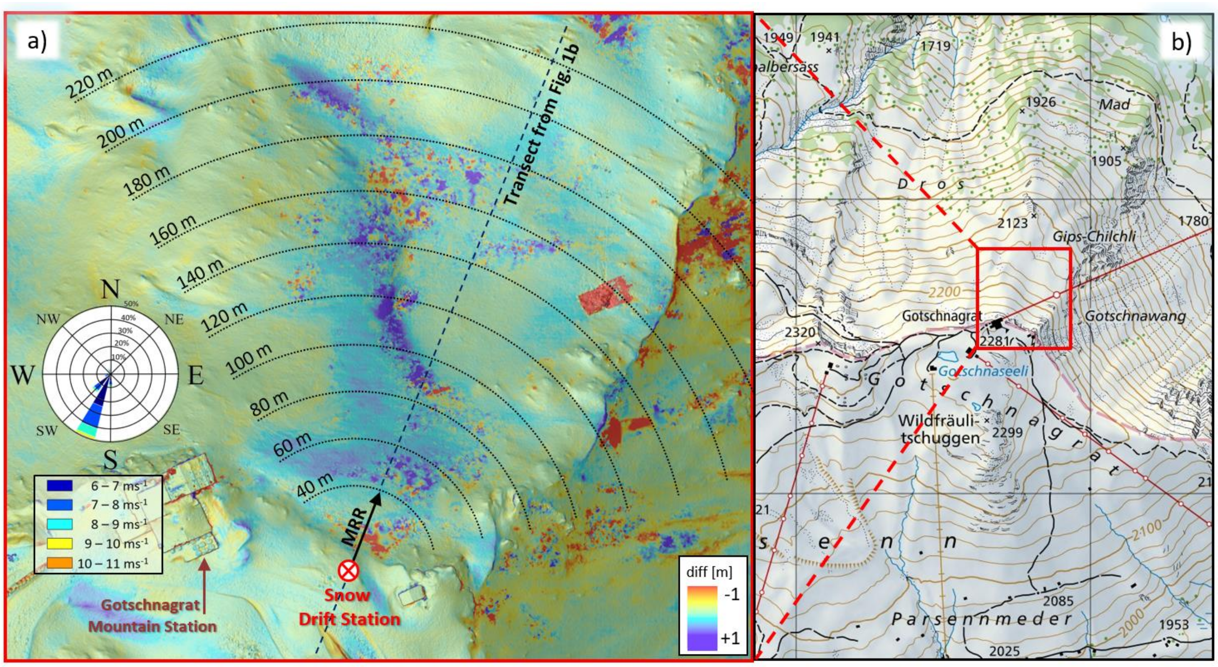

Figure 2: Aerial view of the study domain close to the Gotschnagrat Mountain Station: a) Colours indicate the difference in snow height (diff) between 2019-03-29 and 2019-03-12 determined from two photogrammetry drone flights, showing areas of up to 1 m of snow accumulation north of the Snow Drift Station. The horizontally aligned MRR instrument is mounted at an azimuth angle of 22° at a height of about 1 m above ground. A wind rose indicates the wind speed and direction of all major wind events with a wind speed > 6 m s^{-1} and thus potentially blowing snow effective for the period 2019-03-12, 1200 UTC+1 to 2019-03-21, 1200 UTC+1. b) Surrounding topography of the study site.

P 4 L 97-110: this paragraph is confusing since it is the first time that the authors mention that several evaluation periods were considered in this study. The authors should re-organize this section and describe earlier the different evaluation periods. This is currently done at the end of the Methods section (P5 L 128-133).

Thanks. The different evaluation periods are now explained earlier in the Methods Section (L118).

Different sets of MRR parameters settings were used for each evaluation period. The authors should explain the reasons for these different values. Did it depend on the meteorological conditions during the blowing events or the occurrence of concurrent snowfall (for evaluation period 3)?

Very important comment, thanks a lot! It was to test different (and finding the ideal) setting(s) and not depending on the meteorological conditions. Furthermore, we think a recommendation for parameter settings would be good for those who want to perform similar experiments. We added more information here:

L125: "Different MRR parameter settings were tested during the RACLETS campaign to find the best setting for detecting blowing snow off mountain ridges. The most important parameters were those defining the distance and velocity resolution."

L160: "Providing a recommendation for an ideal MRR parameter combination is difficult, as it depends on the expected size and velocity of the blowing snow events. Based on the results of this study we recommend to start with a number of (N = 32) short (δr = 10 m) range gates resulting in a high distance resolution, a typically sufficient maximum measurement distance of 320 m and in a high Nyquist frequency of $v_{ny} = 48$ m s^{-1} ($v_{act} = \pm 24$ m s^{-1}). A maximum possible value of m = 256 for the number of lines in spectrum results then in a high velocity resolution of δv = 0.19 m s^{-1}. An averaging time of $T_i = 5$s seems to result in a sufficient temporal resolution without producing too much data while still capturing the major flow variability."

P 7 L 159-165: it is not clear why the authors included this paragraph at the beginning of the Results section, especially since results on snow height distribution are presented later in Section 3.4.

Good point. We moved this paragraph to the beginning of Section 3.5.

P7 P164-166: the wind rose on Fig 2. does not bring very valuable information since it does not correspond to the period of snow depth change shown on the map. Instead, I recommend the authors to add on Fig 2 the wind rose for the full period from 12 to 21 March (date when the sonic anemometer was removed) or the wind rose only combining all major wind events during this period (as currently shown on Fig 10). In addition, it would be interesting if the author could provide at the beginning of the Results section a figure showing the two wind roses for evaluation periods one and two. This would give the reader a general overview of the wind conditions during these two events.

Good point. We added the wind rose from Fig. 10 to Fig. 2a as you suggested and removed Fig. 10. The wind directions for EP1, two and three are shown in (now) Fig. 5c,7c and 11b, so it is not necessary to additionally show the wind roses.

P 7 P 167-173: it is also not clear why the authors included this paragraph at the beginning of the Results section. A table or a figure does not support the information provided here. Since this paper constitutes the first application of a MRR to blowing snow studies in alpine terrain, I think that it would interesting to show the differences in radar reflectivity for blowing snow events with and without concurrent snowfall.

The most basic data you get from a radar is the radar reflectivity in [dB]. We wanted to begin the Results Section with some basic radar results. We agree that it was a bit curious in the previous version of the manuscript, therefore we made some changes and included a new figure (now Fig. 3), as you suggested, showing the radar reflectivity for a pure blowing snow event and one during precipitation. Thanks for this good idea!

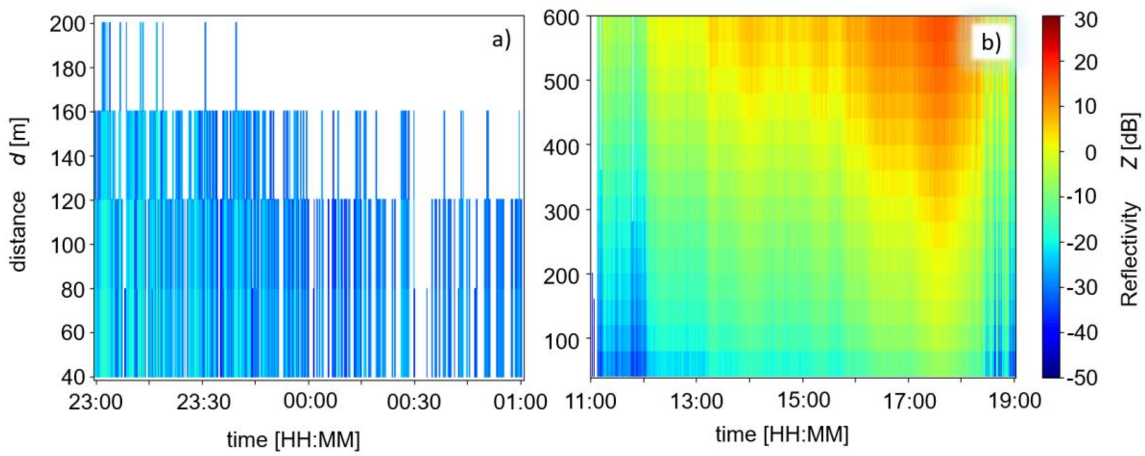

Figure 3: MRR reflectivity for a) part of EP2 (2019-03-06 – 07) for pure blowing snow events and b) EP3 (2019-03-14) for blowing snow with concurrent snow precipitation.

P 8 L 179: the title of this section is not appropriate since this section does not focus only on the MRR radial velocity. This section constitutes more a zoom on a specific event.

We agree. We renamed this Section to: "Radial Velocity and Turbulence Intensity: Exemplary cases"

P 8 L 190-194: the MRR turbulence intensity should be defined in the Methods section at the same time as the Doppler velocity and the spectrum width.

This definition has been moved to the methods Section.

P 10 L 227-230: the comparison between the MRR radial velocity and the horizontal velocity measured by the sonic anemometer is only carried out for the first evaluation period. Why did the author not consider the second period as well? Is it due to the different values of range gate length between the two periods?

P 10 L 246 – P 11 L 254: The analysis of the momentum flux revealed the presence of a low-level jet close to the ground during the first evaluation period. The presence of such hump in the lowest meters has been previously reported in measurement of the wind profile at crest location (Fohn, 1980). Did the author find negative values of the momentum flux during the second evaluation period? Overall, it would be interesting to systematically carry out the same analysis for the two pure blowing snow events in Section 3.2.

We wanted to show only the details for evaluation period one (EP1) as an exemplary case (now Fig. 5), to avoid too many details and too many plots. However, we agree that the details for the second evaluation period are also relevant and interesting, therefore we included another figure (now Fig. 7) as well as additional information:

L 321: "Very similar results were found for EP2 (Fig. 7). Longer transport distances (Fig. 7a) were typically obtained as a result of the higher wind velocities (Fig. 7b). The wind direction (Fig. 7c) was typically quite stable although there were two periods (2100 – 2200 UTC+1 and 2300 - 2330 UTC+1) where the wind direction varied significantly. The momentum flux (Fig. 7d) was negative in about 50% of the time, indicating a higher presence of a low-level jets close to the ground compared to EP1."

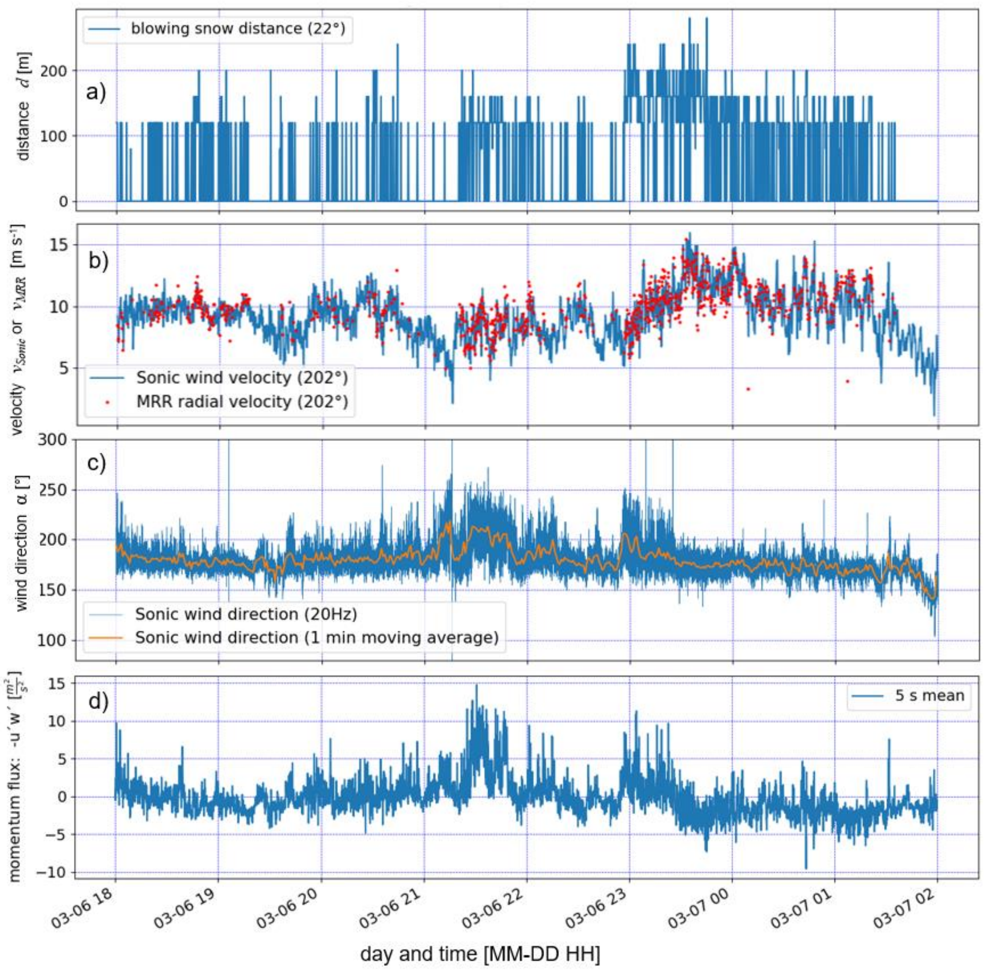

Figure 7: a) Temporal evolution of the horizontal transport distance of all blowing snow events of EP2 (2019-03-06 1800 UTC+1 – 2019-03-07 0200 UTC+1). b) Wind velocity parallel to the MRR direction (202°) measured with a Sonic compared to the close range (40 - 80m) blowing snow radial velocities measured with the MRR. c) Wind direction (mainly 180° - 220°) and d) momentum flux –u'w' calculated using the Sonic data.

For the sake of completeness, we also included the momentum flux for the third period in Fig. 11c.

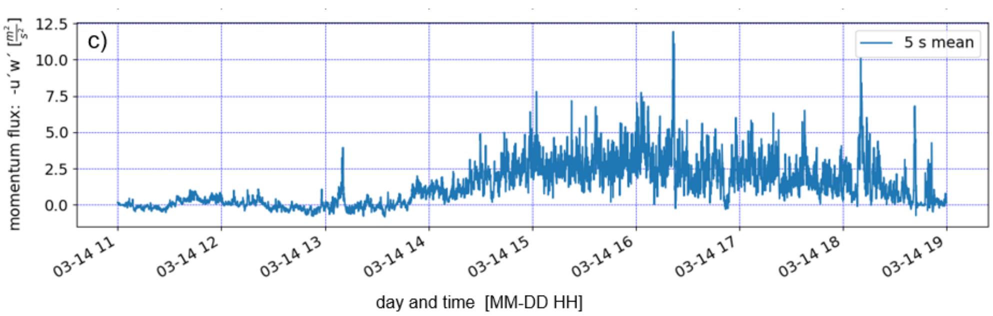

Figure 11: Precipitation event of the EP3 on 2019-03-14 with strong wind from the south resulting in blowing snow and preferential deposition north of the Snow Drift Station as shown in Fig. 2. a) Sonic wind velocity and MRR radial velocity, b) wind direction and c) momentum flux –u'w' calculated using the Sonic data (similar as in Fig. 5 and 7).

P12 L 274-207. The authors mention that the average wind speed was larger during the second episode, explaining the larger transport distances. To better understand these differences of transport distance, it would be interesting to show the distributions of wind speed during the two blowing snow events and not only the average values. In a sense, Figure 7 could provide this information but the author should separate the data for the 2 blowing events.

We agree, therefor we added (now) Fig. 7 (see above).

At L 279, the authors mention that the snow surface conditions and its erodibility may have been different between the two episodes. This suggests that the relationship between the transport distance and the wind speed varied between the two episodes. Separating the data on Fig. 7 would help answering this question.

Very good idea! Thanks! We changed this figure (now Fig. 9) as you suggested. The linear fits clearly show an increase of the "threshold velocity" for EP2. More information on this is added:

L352: "To estimate a threshold wind velocity (e.g. Li and Pomeroy 1997) and thus erodibility of the snow surface for particle entrainment and transport across the ridge, boxplots of the sonic anemometer wind velocity as a function of the transport distance are provided in Fig. 8. The median wind velocity increases by about 2 m s^{-1} for a transport distances increasing from d = 40 - 200 m for EP1 and about 5 m s^{-1} (d = 80 – 280 m) for EP2. An extrapolation of the wind velocity to d = 0 provides an estimate of a threshold velocity of 7.5 m s^{-1} for EP1 and 8.8 m s^{-1} for EP2, a result that is in overall good agreement with other studies (e.g. Li and Pomeroy 1997). Note: The wind velocity threshold definition for particle transport used in this study at a height of the sonic anemometer (1.5 m) is similar to that used in Li and Pomeroy (1997), who defined a threshold wind speed at 10 m above ground and is different to the traditional definition of a threshold friction velocity for particle entrainment and saltation (e.g. Schmidt 1980, Guyomarc'h and Mérindol 1998, Clifton et al. 2006, Walter et al. 2012). The fact that the estimated threshold for EP2 (Fig. 9b) is 1.3 m s^{-1} higher than for EP1 (Fig. 9a) supports our previous hypothesis of different snow surface conditions with a reduced erodibility for EP2."

P 12 L 282-283: the extrapolation of the median wind velocity to obtain a threshold velocity is rather hazardous.

We agree, therefore we renamed it now as an "estimation of the threshold".

Indeed, the definition of the threshold velocity differs from the traditional definition of the threshold velocity for the onset of snow transport in saltation (e.g. Schmidt, 1980; Guyomarc'h and Merindol, 1998 Clifton et al., 2006). The authors should better comment on the definition of the threshold velocity and its difference with previous studies.

Thanks for this good comment. We included more information as shown in the comment above (L352).

P 13-14: Section 3.4 presents the results on snow depth changes during the period from 12 to 29 March. This period does not correspond to the two pure blowing snow events studied in the previous sections. The authors should improve the description of the linkage between the snow depth changes and the blowing snow characteristics derived from the MRR in Sect. 3.1 to 3.3.

Please see comment below: L415 cc.

Indeed, so far, the MRR data in Sect. 3.4 are only used to show that the agreement is good between the MRR radial velocity and the sonic anemometer wind velocity. This was already shown in Fig 4 and 5.
In the previous figures it was without precipitation, the idea was to show that the agreement also holds during precipitation. Furthermore, you suggested to show the identical data for all three events to provide a complete picture. Therefore, we decided to keep that figure and also included the momentum flux for period three (now Fig. 11c).
For example, can the author discuss similarities or differences between the transport distance from the MRR and the pattern of snow deposition in the lee of ridge?
We agree and added more information:
L 415: "Similar transport distances for the blowing snow events with concurrent precipitation (EP3) as for those without (EP1 and EP2) are assumed, based on the similarity of the wind direction and wind speed. Therefore, the increased accumulation north of the ridge up to distances of 200 m (Fig. 2a) are very likely the result of the two blowing snow events with concurrent precipitation between the two drone flights. Although the wind velocity for EP3 (Fig. 11a) are slightly smaller than for EP1 and EP2, probably resulting in smaller transport distances than shown in Fig. 5a and 7a, the snow gets likely being transported further closer to the ground outside the field of view of the MRR before it is finally deposited, which might explain increased accumulation for distances of up to d = 220 m (Fig. 2a). Although the local topography and the near ground wind velocities north of the ridge also influenced the small scale (meters) snow height distribution on the ground, the main conclusion is that an overall good agreement is found between the blowing snow direction, wind velocities, blowing snow distances and the larger scale (several tens of meters) snow accumulation pattern."

Overall, the author should better justify why showing the snow depth changes bring constructive information to this study. So far, I cannot find it and would recommend to the authors to remove this section from the paper and to focus on a more detailed evaluation of the two blowing snow events.
We think it is very valuable to include the snow depth variability to close the loop from Wind->blowing snow->snow redistribution->accumulation pattern. We agree that we did not properly discuss the value of including the snow height distribution. Therefore, we added more information:
L 442: "The presented snow height distributions together with the characterization of the blowing snow events provides a valuable data basis for validating coupled numerical weather and snowpack simulations."

P 15 L 360-364: the potential of LiDAR is not clearly defined here. Are the authors referring to Airborne Laser Scanner for measure before and after blowing snow events or vertically- (or horizontally-) pointing cloud physics Lidar for measurements during blowing snow events.
Thanks for this important comment. We meant the latter one:
L452: "Also exploring the potential of horizontally pointing cloud physics LIDAR (e.g. Mona et al. 2012) in detecting the spatio-temporal …"

P 15: Section 4: Errors and uncertainties associated with the MRR data are not discussed in the text. It would be a very valuable addition since this paper constitutes the first investigation of the dynamics of snow plumes with a MRR and we can expect more studies to come in the future.
Errors of aliasing and ground clutter are described in the Methods Section (L150). More information on the uncertainty of the mean doppler velocity and the spectrum width is provided in the Methods Section:

L153: "Furthermore, it is difficult to quantify an uncertainty on the mean Doppler velocity v_{MRR} that is a moment of a distribution, the Doppler spectrum. The measure of the Doppler velocity itself is relatively precise, i.e. depends on the precision of the clock in the radar. It is more uncertain to which extent the mean Doppler velocity is representative of the movement of the particles within a range gate. However, the main wind direction was typically well aligned with the MRR view direction and the velocity fluctuations induced by turbulence is assumed being normally distributed around the mean so that the mean Doppler velocity v_{MRR} well represents the mean wind or particle velocity within a range gate."

The authors should also mention in their conclusion the potential for innovative model evaluation.
Please see comment above. L442.

Technical Comments

Abstract L 18-19: the definition of threshold wind speed used here is questionable and a value of the threshold velocity with two decimal value may not be relevant for the abstract.
Thanks! This has been changed in L 19:
"In a first order approximation, the travel distance increases linearly with the wind velocity, allowing for an estimate of a threshold wind velocity for snow particle entrainment and transport of 7.5 – 8.8 m s^{-1}, most likely depending on the prevailing snow cover properties."

P 1 l 30: the references to Gerber et al (2018) and Sharma et al (2019) are not fully appropriate here. Indeed, the paper by Gerber et al (2018) does not study blowing and drifting snow and the paper by Sharma et al (2019) focuses on snow bedforms, which are typically below the slope scale.
This was a bit confusing as these references were referring to "slope scale" and not meant to refer to drifting and blowing snow. Slope scale can be few meters to hundreds of meters (Review Mott et al. 2018). Therefore, both papers should be OK in our opinion. Nevertheless, we added some more references:
L33: "Schön, P., Prokop, A., Vionnet, V., Guyomarc'h, G., Naaim-Bouvet, F., and Heiser, M.: Improving a terrain-based parameter for the assessment of snow depths with TLS data in the Col du Lac Blanc area. Cold Regions Sci. Technol. 114, 15–26. doi: 10.1016/j.coldregions.2015.02.005, 2015.
Shook, K., and Gray, D. M.: Small-scale spatial structure of shallow snow covers. Hydrol. Process. 10, 1283–1292, 1996."

P 2 L 46: the paper by Gerber et al (2018) only concerns modelling and observations of snowfall in alpine terrain. It would be valuable to add references to other studies that also consider drifting and blowing snow. See Mott et al. (2018) for a list of relevant references.
Thanks, we added two more references:
L49: "Guyomarc'h and Mérindol 1998, Naaim-Bouvet et al. 2010"

P 3 L 66-67: it would be interesting here to provide the link to the Envidat webpage that host the data collected during the campaign.
Good point! We added a reference containing the link.
L 93: "The data collected during the campaign including that used in this study can be found at Raclets (2019)."
RACLETS: Envidat data repository, https://www.envidat.ch/group/raclets-field-campaign, 2019.

Table 1: the date for event 3 in the table differ from the date given in the text (L 129).
We changed this.

P 13 L 304: should it be "< 0.05 for period one"?
We changed this.

P 14 L 329-330: the dismantling date for the MRR and the SDS should be given in the Methods section.
We added this to the Methods Section:
L123: "On 2019-03-21, the MRR and the instruments of the SDS were dismantled."

References (used in this review and not present in the initial manuscript)
All of the suggested references below were included in the manuscript.

Aksamit, N. O., & Pomeroy, J. W. (2016). Near-surface snow particle dynamics from particle tracking velocimetry and turbulence measurements during alpine blowing snow storms. The Cryosphere, 10(6), 3043-3062.

Föhn, P. M. (1980). Snow transport over mountain crests. Journal of Glaciology, 26(94), 469-480.

Geerts, B., Pokharel, B., & Kristovich, D. A. (2015). Blowing snow as a natural glaciogenic cloud seeding mechanism. Monthly Weather Review, 143(12), 5017-5033.

Guyomarc'h, G., & Mérindol, L. (1998). Validation of an application for forecasting blowing snow. Annals of Glaciology, 26, 138-143.

Guyomarc'h, G., Bellot, H., Vionnet, V., Naaim-Bouvet, F., Déliot, Y., Fontaine, F., ... & Naaim, M. (2019). A meteorological and blowing snow data set (2000–2016) from a high-elevation alpine site (Col du Lac Blanc, France, 2720 m asl). Earth System Science Data, 11(1), 57-69.
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Naaim-Bouvet, F., Bellot, H., & Naaim, M. (2010). Back analysis of drifting-snow measurements over an instrumented mountainous site. Annals of Glaciology, 51(54), 207-217.

Schmidt, R. A. (1980). Threshold wind-speeds and elastic impact in snow transport. Journal of Glaciology, 26(94), 453-467.