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

Anonymous Referee #1

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

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). 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. I also recommend the author to mention existing studies on snow plumes (Moore, 2004;

Geerts et al., 2015) and their main conclusions. 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. 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.

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 the 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).

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

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.

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). 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 meteorolog-

ical conditions during the blowing events or the occurrence of concurrent snowfall (for evaluation period 3)?

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.

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.

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.

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.

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.

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.

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. 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.

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

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. 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. 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? 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 mode detailed evaluation of the two blowing snow events.

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.

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. The authors should also mention in their conclusion the potential for innovative model evaluation.

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.

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.

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.

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.

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

P 13 L 304: should it be “< 0.05 for period one”?

P 14 L 329-330: the dismantling date for the MRR and the SDS should be given in the Methods section.

References (used in this review and not present in the initial manuscript)

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