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The Cryosphere Discussions

Interactive comment on "Understanding snow-transport processes shaping the mountain snow-cover" *by* R. Mott et al.

R. Mott et al.

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We thank the two anonymous referees and M. Pelto for their valuable contributions. In the following we answer all referee comments and short comments.

REFEREE # 1:

Comments:

Comment: 1. For readers unfamiliar with the region a very brief description of conditions during the study would be helpful. For example, what were: the range of temperatures; the seasonal extent of the snow; and the amount of precipitation? How snow-free is the summer?

Response: We agree with the referee that a brief description of the meteorological conditions during the study might be helpful for the reader and will implement a short paragraph: During the accumulation periods 2008/09 and 2009/10, the air temperature ranged between -24 °C and + 10 °C; the accumulation period 2008/09 lasted from November 20, 2008 until April 9, 2009, whereas the accumulation period 2009/10 started on the October 12, 2009 and ended on April 19, 2010. Complete melt of the seasonal snow is usually observed in June.

Comment: 2) The definition and use of "HSmax situation" is confusing and unclear. Is this the date each winter when average snow depth is maximum? Or is this the yearly maximum snow depth for each location or grid square? Since the concept is only used on page 870 and Fig. 8 perhaps it isn't needed. It would be simpler for Fig. 8 to refer to HS on 9 April 2009 or "the time of peak accumulation on 9 April 2009" as on 872-3.

Response: The term "HSmax situation" was intended to describe the time of peak accumulation of snow during an accumulation period, thus the time when the accumulation period ends and the ablation period starts. To avoid confusion we now only use "the time of peak accumulation" and will provide a definition in the text.

Comment: 3) The descriptor of "simple statistics" isn't necessary. It is more clear to state that you used the mean and standard deviations.

Response: We change "simple statistics" to "statistics".

Comment: 4) Perhaps "Observed and modelled" aren't needed in the titles of Sections 3.1 and 3.2.1.

Response: We remove "Observed and modelled" from the titles.

Comment: 5) No heights or elevations are given for the wind speeds discussed throughout the paper.

Response: The wind speed is always referring to the first level above ground, which is approximately at 0.8 m (mean value) above the ground (see 2.2). Due to the terrain-following (sigma) coordinate system the height of the first level (dz) varies between 0.49 m at ridges and 1.1 m at flatter terrain or depressions. We will specify this range in the text.

Add to the text: "In Fig4b, the modelled mean flow field for a characteristic NW storm with moderate wind velocities is illustrated for the first level above ground ... (in 3.1) Modelled wind field for the first level above ground..." (caption of figure 4b)

Comment: 6) 872-23: Do you mean that the met data was used to update Alpine3D every hour? Or was it only used to initialize the model?

Response: The met data was used to update the Alpine3d model every hour. We change this in the text. .... we updated Alpine3D with hourly...

Comment: 7) 874-1: What is "a snow-fall event without precipitation"?

Response: A mistake: remove without precipitation from the text.

Comment: 8) 871-4: You could refer to Fig. 3 here to confirm your claim.

Response: Will be implemented;

Comment: 9) 875-14,15: Are these positive offsets or absolute differences?

Response: The mean deviation was calculated from absolute differences.

Comment: 10) 875-23: You have chosen the term "bowl" to refer to a specific location, so it confusing here to refer to "two bowls".

Response: Now we only refer to the Bowl or the Inner Bowl;

Comment: 11) 877-8: Is "HS at the HSmax situation" just the same thing as saying "HSmax"?

Response: Yes it meant the same. To be more precise we replace "HS at the HSmax situation" by "HS at the time of peak accumulation" (see above).

Comment: 12) Figs. 1, 4, 5, 6, 8, 13: The x and y axes are not labeled.

Response: We add to the figure caption: The x and y- axes give Swiss coordinates in meters.

Comment: 13) Fig. 1: If a shadow plot must be used, the direction of lighting should be given. "Colored areas indicate" should be "Colored lines enclose". Most importantly, it is unclear if "lowest" and "highest" refers to elevation or to the position in the figure. The labels T1, T2, and T3 used in Table 1 should be added to the figure.

Response: Figure 1 is updated - T1, T2 and T3 are labelled in the figure according to Table 1. However, we do not think that there is additional benefit in adding the direction of lighting.

Comment: 14) Fig. 4: "was originally" should be "is".

Response: We will change "was originally" to "is";

Comment: 15) Figs. 5 and 6: Are six examples really needed here? It would also be reasonable not to present data if an avalanche has occurred.

Response: We think that multiple examples (also from two different years) are helpful for the reader to get an impression of the consistency of the snow depth distribution caused by the 'master' storms. Due to the avalanche occurred in P4_0910 we decided to remove this period from the analysis, as recommended by the referee. Therefore the following part will be removed from the text: "In periods with lower wind velocities, for example, P40910 (Fig. 6f), snow-transport processes could not be initiated to form the cornice-like drifts. Note that the erosion zones, which are visible in the map of measured dHS of P40910 (Fig. 5f, red colors), are due to avalanches which occurred during this short period."

Comment: 16) Fig. 6: Instead of "mask" could you say something like "Only data for the regions defined in Fig. 1 are shown"?

Response: We replaced the sentence by: Only data for grid points covered by measurements are shown.

Comment: 17) Figs. 11 and 12: Why are "P" notations used in Fig. 11 but dates are used in Fig. 12? The use of a line graph to present this data is misleading, since the points in time are not evenly spaced. Perhaps a bar graph would be more clear.

Response: We agree with the referee that a line graph might be misleading. Fig. 11 and Fig. 12 are updated - we now always use the same “P” notation and use points instead of a line.

Comment: Technical Corrections 1) 867-22: Should "on snow distribution" be "of snow distribution"?

Response: Yes, we changed this.

2) 869-14: Web-sites are usually given in the reference list.

Response: The web-site will be given in the reference list.

3) 869-14: Reverse yellow and red as "red and yellow stars in Fig. 1, respectively".

Response: Will be implemented;

4) 869-15 and throughout: Italics are used for non-English words. There is no need to italicize 'ridge' and 'bowl' throughout. (They could be capitalized such as "The Bowl".)

Response: We replaced bowl with Bowl (not italicized anymore).

5) 869-25; 879-4; and 879-7: TLS and HSmax are used before they are defined. "terrestrial laser scanner" should be "Terrestrial Laser Scanner (TLS)".

Response: They are now defined adequately;

6) 870-9,10,11,16: These four occurrences of "(Response et al., Year)" should be "Response et al. (Year)".

Response: Will be implemented;

7) 876-19: "Fig. 5" should be "(Figs. 5 and 6)".

Response: Will be implemented;

8) 877-5: "changes in snow depth"

Response: Will be implemented;

9) 877-26: "smooth", not "smoothen"

Response: Will be implemented;

10) 878-11: "became smooth", not "smoothed"

Response: Will be implemented;

11) 879-9: "_" should be "_".

Response: Will be implemented;

12) 879-7: Parenthesis should be used around "the bowl... (Fig. 1)", not a colon.

Response: Will be implemented;

13) 883-14: "this would be a..." should be a new sentence.

Response: Will be implemented;

14) Table 1: "Pearson", not "Pearsons".

Response: Will be implemented;

15) Figs. 3 and 7: The legend variables should be italicized and subscripted.

Response: The legend variables are now italicized and the font taken smaller.

16) Fig. 7: The wind speeds (4 and 7) are missing from the legend.

Response: We add initialized wind speeds to the legend

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17) Fig. 8: The titles at the top of panels a and b should be DEMS and DEMW.

Response: Will be implemented;

18) Fig. 10: DSM should be DEM.

Response: We will change this.

REFEREE # 2:

General:

1. There are clearly some problems with the full model simulation of the change in snow depth that should be more openly addressed in the discussion of results and conclusions. It is very strange that in a wind-blown landscape the model results without blowing snow transport were better than with in some zones and this could be more clearly discussed.

Response: The referee is right that in some parts, especially at the Bowl, the model runs considering the whole range of snow transport processes show the development of unrealistically huge deposition patterns behind the ridge. For ARPS, the critical slope is 45°. Due to insufficient geometrical resolution of pronounced ridges, the ARPS model overestimates the speed-up effects at most ridges. The non-linear function of the snow redistribution to the wind velocity then lead to a strong overestimation of the saltation fluxes and deposition rates in the lee of these ridges. This effect is now more clearly discussed and demonstrated by adding a cross-section figure with wind speeds on the first model level. Furthermore, the model resolution appears to be still insufficient to capture some very small-scale flow features (e.g. at small-scale channels and the upper part of the NE-slope), which highly influence dominant deposition patterns in the area. Also this is discussed now in the text.

2. The final concluding chapter is too long and wordy and could be condensed.

Response: We would like to keep the concluding discussion in its current form.

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Comments:

Comment: Please correct small typographical errors: for instance you can use north-west instead of North-West for wind direction and snowfall is one word, not snow-fall.

Response: Typographical errors are corrected;

Comment: p. 867 line 21 – "oversimplification": perhaps "simplification" could be used here as many of these models are designed for use at larger scales or where detailed topographic or snow information is lacking or not needed and are trying to avoid over-parameterization.

Response: changed oversimplification to simplification

Comment: P 867 – what about sublimation of blowing snow (e.g. Schmidt 1972; Pomeroy, 1989)? This has been shown to affect some alpine blowing snow transport in Scotland and Canada (Pomeroy, 1990; MacDonald et al. 2010)

Response: The authors are fully aware of the importance of sublimation during blowing snow events. The effect of sublimation on the Wannengrat testsite and the implementation of a drifting snow sublimation routine into the model frame of Alpine3D are addressed in a recent publication (Groot Zaaftink and Lehning, 2010) and ongoing work at SLF Davos. In our work, we only discuss drift events during or shortly after snow fall events. At least during the snow fall events sublimation is insignificant. Furthermore we only discuss deposition patterns and do not try to discuss the total mass balance. We will add a discussion on the missing sublimation to the conclusions.

Comment: P 868 line 7 – Increased horizontal wind flow scours snow and increased downward wind flow normal to the surface is not consistent with a boundary layer so this will need to be explained a bit more or differently. Can you put this accumulation area in terms of zones of convergence and divergence of wind fields?

Response: In three dimensions, wind fields are always (approximately) divergence free because the air is approximately incompressible. As soon as you have signifi-

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cant topography and therefore significant vertical velocities, the concept of horizontal divergence as used in large scale meteorology becomes meaningless. To clarify the discussion, we now calculated the surface-normal wind velocity (instead of the vertical component of the wind velocity), which will become zero as the ground is approached. As also implemented in the drift model the surface-normal wind velocity vectors are a measure of increased or decreased local erosion and deposition from suspension and preferential deposition of precipitation (Lehning et al.. 2008). The text in 3.2.3 is reworded in respect to the surface-normal wind velocity: "The relative importance of these transport processes in different areas is related to the local wind field as a function of the local topography. In Fig. 13, the modelled dHSprec for P10809 (a) the spatial distribution of the three-dimensional wind velocity (b) and the surface-normal wind velocity (c) are shown for one wind field (initialized wind velocity was 5 m/s). The flow field simulations (Fig. 4b, Fig. 13b) showed that, during synoptically controlled NW flow, the Bowl was characterized by a high wind velocity gradient on the leeside of the ridge. In the Inner Bowl, modelled wind velocities were very low with a very homogeneous distribution of wind velocity (Fig. 13b). The surface-normal wind velocities (Fig. 13c) are highly dependent on the curvature of the small-scale topography. The negative surface-normal wind velocities at the channels and the eastern part of the Bowl indicate downdraft zones (Fig. 13c). Zones of modelled decreased wind velocities and downdrafts experience stronger preferential deposition of precipitation due to higher deposition velocities of snow. Therefore preferential deposition of precipitation is the dominant snow deposition mechanism in the Bowl (Fig. 12e, 13a). This leads to a more homogeneous lee-slope loading (Lehning et al. (2008)) and corresponds with the low variance in snow depth changes observed in the Bowl (Fig. 12b, f). For the NW slope, the numerical analysis suggests that the flow around the Wannengrat and the associated local speedup caused an enhanced flow (Fig. 13b) on the NE slope. The positive surface-normal wind velocities (Fig. 13c) indicate updrafts on the NE slope. This led to high wind speeds on the windward sides and decreased wind speeds on the leeward sides of the cross-slope ridges. This flow field resulted in pronounced erosion on the windward sides and enhanced deposition on the leeward sides of the cross-slope ridges. Thus a high spatial variability of the wind field (Fig. 13b) caused considerable redistribution of snow, resulting in a high variance of dHS on the NE slope (12b, d) and the formation of huge drifts."

Comment: P 868 line 15. No, Fang and Pomeroy (2009) used 6 m spacing.

Response: We will mention in the text: beside the study of Fang and Pomeroy (2009), Mott and Lehning (2010) and Gauer (2001) previous studies . . .

Comment: P 868 line 16 – Why insufficient? Suggest word this as Have used “low density”: which make their promising results difficult to validate.

Response: We will change insufficient dense to low-densitiy;

Comment: P 868 line 25 to p 869 line 5 – recast this summary as a series of objectives for the paper or hypotheses that will be tested in the paper. You should not be describing your field site in the introduction.

Response: We move all parts of the introduction regarding the field site to methods parts. We recast the summary as a series of objectives:

"In this paper we address four main points concerning the understanding of snow-transport process shaping the snow-cover on a high-alpine wind-blown landscape. First, we discuss and show observed and modelled flow patterns. Second, we discuss the spatial patterns of deposited and redistributed snow after several snowfall and storm events based on high resolution measurements and a physical-based model description. Third, we investigate the influence of the changing topography during the accumulation period on the development of drifts observed in the investigation area. Finally, we identify the spatial characteristics of deposition patterns using statistics and relate them to the main snow-transport mechanisms and their dominance in specific areas as controlled by the local flow field."

Comment: P 869 line 10 – this is a vague description of the instrumentation. A table should be included with instruments and key variables measured at the various types of stations. Terms like SensorScope mean nothing without this. It sounds like you did not measure wind direction?? Perhaps this is not correct but if so it is unfortunate for a wind flow study.

Response: Of course, as also written in the text, all meteorological stations are measuring wind. Now, a detailed table is added which shows all sensors and variables measured by the mobile and permanent stations.

Comment: P 869 line 25 – define TLS.

Response: We will implement this;

Comment: P 870 line 20 – RMS error or mean error would be more useful statistics here.

Response: These results refer to the work of Schirmer et al. (submitted to Water Resources Research) and are therefore not in the scope of this work.

Comment: P 872 line 15 – what is the order of calculation for preferential deposition and wind transport processes? Is sublimation calculated?

Response: Sublimation is not calculated (see above). We think that a detailed discussion of the snow-transport modes within the methods part would not improve the paper considerably since the calculation and implementation of the different snow transport modes in the Alpine3D model is already discussed in much detail in Lehning et al., 2008 and Clifton and Lehning 2008. We therefore give references to the respective articles within the text.

Comment: P 872 line 25 – water vapour pressure (limited by saturation) should be conserved rather than relative humidity when extrapolating from meteorological stations. Why would you assume that RH is constant? Where does the water vapour go as temperature changes???

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Response: Dynamical effects have little influence on our small area with very limited altitudinal changes. Therefore, constant RH appears to be justified and not much different from preserving mixing ratio or partial pressure.

Comment: P 877 line 13 – the effect of avalanche is substantial. Can you perform an empirical correction to accommodate this effect?

Response: It is right that the effect of the avalanche is substantial. We decide to remove this period from the analysis, as suggested by referee 1.

Comment: P 880 line 4 to p. 881 table 1 discussion – you should note and openly discuss the very poor performance of the snow transport calculations in determining changes in snow depth at some sites and the reasons that your model seems to suggest preferential deposition is the major redistribution process when you know well that you have substantial snow transport from your wind speed observations and from surface features like sastrugi. For instance, having a lower correlation between modelled and measured depth than between wind speed and measured depth suggests that another modelling approach might be required to develop reliable snow depth maps over the whole mountain landscape???

Response: We do not suggest that preferential deposition is the major redistribution process (also because it is not a redistribution process). We suggest that there is a dominance of different snow transport processes (redistribution and poor deposition) on different slopes. We indeed observed very high wind speeds, especially in areas where we identified saltation and suspension to be the main processes driving the snow depth distribution. Nevertheless, as also the non-existence of sastrugis and wind measurements suggest, we state that there are some regions in our test site which are highly influenced by the preferential deposition of precipitation and less influenced by strong redistribution processes (e.g. the Bowl). The observed stronger correlation for the modelled wind velocity distribution compared to the modelled snow depths can be attributed to the non-linear response of redistribution processes to the wind speed

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and the steady state assumption of the drift model which then causes a large overestimation of drift as a function of an overestimated local wind speed (see discussion above "General"). In the case of transects crossing the ridges, the correlation values are very sensitive to the exact position of the crowns. The position of the crown strongly depends on the changing topography and is thus very difficult to model.

We will add a more detailed discussion regarding the shortcomings of the model (see above) to the text.

Comment: P 883 line 11 – define LES

Response: We change LES to Large-Eddy Simulation

Response to M. Pelto:

Comment: 869-14: Most importantly there is not a detailed listing of the meteorologic variables captured at the SensorScope and WAN locations. A table would perform this task well. In particular what is the height at which wind is recorded and over what time interval is the wind speed and direction averaged?

Response: Now, a detailed table is added which shows all sensors and variables measured by the mobile and permanent stations. The time interval as well as the height of wind speed measurements above bare ground will be added to the text.

Comment: 873-1: Was WAN 7 chosen for the snow depth because of its flat and wind protected location?

Response: Yes, will be added to the text.

Comment: 873-3: Why was WAN 2 chosen for this analysis?

Response: The station Wan 2 has been chosen for the classification schema, because of its wind-exposed location. This will be added to the text.

Comment: 867-20: Why the tendency of the 2008/09 events in Figure 5 to have much

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more pronounced wind erosion on the margins of the NE slopes?

Response: The pronounced wind erosion in the upper parts of the NE slope (margins of the NE slope) is caused by higher wind speeds during these periods. High wind speeds combined with strong updrafts on the slope lead to an increase in erosion with increasing wind speed.

Comment: 878-7: It is mentioned that cornice development is continuous during the study. Does the examination of the cornices extend through the entire accumulation season? There are some cornices I have observed that reach an aerodynamic shape at some point in the accumulation season and grow no further. Is this what is observed at 878-22?

Response: We did not observe that the cornices reached an aerodynamic shape. The cornices were still growing during all measured NW storm events, for example during P8 at the end of the accumulation period.

Comment: 883-7: Given the goal of developing coherent scaling parameters relating wind speed to snow deposition, what resolution of snow depth measurements alone would be needed to use as input to determine the wind field?

Response: We use snow depth measurements with a grid resolution of 1m (the original measurements have a higher resolution depending on the distance of the measurement device from the slope).

Interactive comment on The Cryosphere Discuss., 4, 865, 2010.