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The Cryosphere Discussions

Interactive comment on "Seasonal controls on snow distribution and aerial ablation at the snow-patch and landscape scales, McMurdo Dry Valleys, Antarctica" *by* J. W. Eveland et al.

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The study shows that high-resolution imagery can be used to investigate snow accumulation patterns as well as aerial ablation patterns on different scales. The topic of the study is of high relevance for the cryospheric community and thus well suited for publication in the journal The Cryosphere. The data set presented in this study is very interesting as it allows an extensive analysis on factors controlling inter-annual variability of snow accumulation and ablation in Antarctica. Most of the conclusions drawn by the authors are, however, already well-known (e.g. that snow accumulates at the same locations each accumulation season), but have not been accordingly refer-

enced by the authors. Given the high potential of the data set presented I recommend extending the analysis on processes and factors controlling snow ablation and accumulation at the snow patch scale (which is stated as a main aim of the study but not sufficiently analysed and discussed in the following). This would include the analysis of micro-topographic effects such as curvature or sheltering. I am sure that the extended analysis would give the manuscript a more profound focus,

Main concerns and specific comments are summarized below.

General comments:

While controls on snow distribution (accumulation) and aerial ablation are well investigated on the landscape scale, driving processes and factors on the snow patch scale are not sufficiently analysed. As the snow patch scale is included in the title of the manuscript and the investigation of factors leading to late season patterns of snow-covered area (especially on the snow patch scale) is one of the main aim of the study, I would strongly recommend extending the analysis on the snow patch scale by including micro-topographic influences to the analysis such as curvature and exposure. Recent studies (Fujita et al., 2010 and Mott et al., 2012) have shown that micro-topography such as curvature drive processes leading to increased/decreased ablation of snow patches. Snow patches located at topographic depressions or concave topographic features show decreased ablation rates because of the formation of cold air pools and associated boundary layer decoupling. At the other hand, high wind velocities can lead to strong local advection of sensible heat later in the season when snow-free and snow-covered areas coexist (e.g. Liston, 1995; Granger et al., 2006; Mott et al., 2011 and 2012). This additional source of heat causes higher ablation rates at snow patches prone to wind (relevance of exposure). At some points of the manuscript, the authors are mentioning that some snow patches tend to survive longer than others (shown by differences in aerial ablation or by long-lasting snow patches at the end of the season). They are claiming to investigate seasonal controls – thus it is very important to focus on changing controlling factors as snow-coverage changes in the course of a melting

season. As the presented data set allows such analysis, I would strongly recommend doing so. The significance of the manuscript would certainly benefit from such an analysis as most of the other conclusions of the manuscript are already well-known.

The reference list is incomplete. Although not focussing on snow distribution in Antarctica, there are a number of studies investigating snow depth distribution (e.g. Winstral et al., 2008; Trujillo et al., 2009; Bernhardt et al., 2010; Deems et al., 2008; Schirmer et al., 2010; Mott et al., 2010; Lehning et al., 2012) or controls on snow ablation (e.g. Grünewald et al., 2010; Fujita et al., 2010: Mott et al., 2011, 2012;).The studies of Trujillo et al, 2009 and Schirmer et al., 2010 already showed the persistence in inter-annual snow depth distribution in alpine environments, which is also one of your main conclusion of the manuscript.

From my point of view the modelling part does not add any additional value to the manuscript. It does not become clear why ablation modelling has been performed. The authors should either give a new focus to the modelling part or skip it from the manuscript.

The manuscript is well written and well structured.

Specific comments:

Introduction:

Please set this study within the context of studies investigating snow depth distribution and snow ablation It seems that you are discussing three instead of two scales (landcape, plot and snow patch scales)

Methodology:

What are the length scales of the snow patches observed?

Please state the spatial resolution of the “high resolution” imagery when you are introducing your data set

P 3829, l3: What is “dynamics” referring to? Temporal or spatial dynamics

What makes a snow patch representative for your analysis?

It would be important for the reader to know the specific aim of the modelling part.

Results:

Fig 7: although topographical parameters as aspect and slope are analysed, they are not discussed in the text

P 2835, l 24: it would be very nice to see where these pockets of deeper and more persistent snow patches are located — please discuss micro-topographical factors in more detail (including effects on the local energy balance, please see general comments)

P 3836: “the rates of aerial ablation appear to taper near the end of the season” – this also indicates that some of the snow patches tend to survive longer than others and that these snow patches are very persistent. It would be interesting to discuss why!

P 3837: do you mean with “early season” the end of the accumulation season? Pease, be more precise here!

P 3837, l7: why does the gradient of snow-covered area with along-valley distance becomes much less pronounced near the end of the season? The manuscript would certainly benefit from a more process-oriented discussion of results.

P 3838-3839: I do not see the aim of the modelling part! Looking at the comparison of measurements and model results, the modelling does not really support the measurements, nor has the modelling part any explanatory power.

P 3839, l20: here it would be worth to discuss the change in the energy balance!

P 3840: l 20: *but shallow snow patches have been observed faster in the field”- do you mean aerial ablation here or the ablation rate? There is a big difference, because it is quite obvious that the aerial ablation is much higher for shallow snow patches, but

it would be more interesting if shallow snow patches show higher ablation rates as this would indicate some micro-topographic influences.

L23: “given the same topography and meteorology” – even if snow patches are located in a distance of a few hundreds meters, this does not necessarily mean that topographic and meteorological conditions are the same or similar. These local topographic differences are of great interest.

P 3841, l5: You give only one example of micro-topographic effects (exposure to wind). Given the high-resolution DEM available for your analysis I would strongly recommend to analyse those effects in more detail! Please also discuss effects as local advection of sensible heat as percentage of snow-coverage decreases and boundary layer decoupling which is highly connected to wind conditions and the location of the snow patch (local topography - curvature)

P 3841, l9-10: add “at landscape scale”

P 3841, l20: what about the influence of aspect and slope (Fig. 7)?

P 3841, l26: can you shortly explain the higher wind speed at the valley bottom?

P 3842 l5-8: that’s exactly the reason why you should extend your analysis!

P 3842 l 12: you should add a reference here

Fig. 12: this Figure is very confusing. Maybe it would help to split this figure into two figures showing only one year per figure . Fig. 14: I recommend skipping figure 14 as the figure has only little informative value.

Conclusions:

P 3843, l 15 -18: you should also add that snow patches not only accumulate in the same locations each year, but also that specific topographical characteristics lead to very long-lasting snow patches (Fujita et al., 2010; Mott et al., 2012) which has also an important effect on local ecology.

P 3844: the usage of season and seasonality are very confusing as they are used in different senses. L3: is elevation also controlling the accumulation of snow patches and not only the aerial ablation?

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