RESPONSE TO REFEREE COMMENTS BY R. DADIC

Overview

The authors present an interesting dataset on snow extent in the Dry Valleys, Antarctica. Snow distribution seems to have important implications of the ecology of the Dry Valleys, which makes this manuscript relevant for the cryospheric community.

It is, however, not clear how the main conclusions of this paper (e.g. that snow patches form at the same locations every year) are different from existing literature on that topic (e.g. Erickson et al. (2005); Deems et al. (2008); Schirmer et al. (2011) and references therein). A more detailed study of the "microtopographic influences over snow depth and exposure", as mentioned in the abstract would greatly improve this paper and give it a new focus besides what is already known. Based on the high resolution of the data and the relatively high resolution of the digital elevation model, there is great opportunity to look at some topographical factors as for example curvature or "shelter and exposure" (Winstral et al., 2002; Schirmer et al., 2011) and discuss them in the context of Dry Valleys. My major comments are summarized in the below and followed by more detailed line comments.

> ***Response:*** ***The conclusion that snow patches generally form in the same locations each year is specific to the McMurdo Dry Valley landscape. This is something that has not been previously confirmed and is integral to understanding spatial processes associated with biogeochemical cycling and the structuring of microbial communities in the Dry Valleys. For researchers investigating the Dry Valleys, this is a very important conclusion. Landscape characteristics, climatic patterns, and snow distribution mechanisms are unique in the Dry Valleys, and are not reflected in other more general snow studies, typically in alpine environments. Additionally the spatial scale is much finer than what is typically investigated. While some of the presented data analysis techniques aggregate the high-resolution data to match the resolution of the 30 m DEM (methodology of landscape analyses described in Eveland et al, in press), the 1 km^2 plot analysis fully utilizes the 0.5 m resolution of the data. Typically studies do not investigate snow processes at this fine of a scale. A revised manuscript will clarify how this conclusion differs from previous publications, and such previous publications will also be referenced.***
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> ***It seems apparent that the use of phrase "microtopographic influences" is vague in this context. The authors intend this to refer to processes acting at a scale much finer than wind modeling can resolve through the use of the available DEM. One example is the range of topographies for the nivation hollows in which snow patches form (i.e. nivation hollows lending to deep snow patches vs. shallow snow patches). To investigate these differences, one would need a DEM with a resolution near that of the Worldview/QuickBird data set (sub-meter). However, the authors agree that modeling capable of investigating very fine scales would provide great insights. An expanded discussion of microtopographic effects, the scale at which these processes occur, and a review of relevant literature will be included in a revised manuscript.***

General comments

• Comment 1: The word "seasonal" and "seasonality" seems to be used incorrectly and is therefore misleading throughout this manuscript. The authors are actually not looking at seasonal variability, but rather at inter-annual variability. Seasonality usually has to do with differences between seasons and not with comparisons of the same season. This should be changed in the manuscript, because it is misleading.

> ***Response:*** ***The phrase "inter-annual variability" will be used in place of "seasonal variability" in a revised manuscript.***

• Comment 2: Looking at two summer seasons does not seem to be enough data to be talking about "inter-annual" variability, as is done in this manuscript. The authors might want to change the focus and rather discuss the spatial patterns in more detail.

> ***Response:*** ***The two ablation seasons presented are the first available data sets of imagery of sufficient spatial and temporal scales for analysis. Analyzing more seasons to come may provide additional insights to snow dynamics, but investigating the contrast between the presented data sets provides important results now. Previous studies in the Dry Valleys (referenced in the manuscript) have shown snowfall in the region to be highly variable, and the contrasting magnitudes of snow-covered area in the two ablation seasons presented exhibit such variability. The intent of the manuscript is to document this large inter-annual variability and not necessarily to look for temporal trends across a series of recent ablation seasons. Also, as mentioned, such a time series is not yet possible given the newness of the appropriate remote sensing imagery.***

• Comment 3: The different scales are a bit confusing. The "landscape scale" should be renamed "regional scale". The snow-patch scale (1 km2 ?) should be renamed "local scale". There also seem to be a third scale, which is also called snow-patch scale (and also has 15 different snow-patches, as does the 1 km2 scale (e.g. Fig. 3)) and it is now always clear, which snow-patch scale is being discussed. This should be clarified.

> ***Response:*** ***The language used to refer to the multiple spatial scales will be clarified in a revised manuscript.***

• Comment 4: It is surprising that the authors discuss topographical influence without mentioning the curvature, despite emphasizing how important those features are (sheltered spots). I recommend that they should try to correlate the locations of the snow-patches to curvature or "shelter and exposure", which would be a large contribution to this manuscript.

> ***Response:*** ***Two types of topographic influences are discussed in the manuscript: broad spatial patterns of ablation due to elevation, slope, and aspect (at the scale of kilometers); and microtopographic influences on individual snow patches operating at a much smaller scale (tens of meters or smaller). The authors agree that modeling wind effects at this fine scale would provide a great contribution, but limitations in data does not allow for such analysis. Unfortunately, there is a large discrepancy in the spatial resolution between the remote sensing imagery (0.5 m) and the best available digital elevation model (30 m). It is important to keep in mind that this study is investigating snow dynamics at a much finer scale than typical alpine studies.***

• Comment 5: The citation of relevant literature in this manuscript is poor. The authors seem to be unaware of the large body of literature that has been done outside of the Dry Valleys on snow distribution, on how snow distribution effects on vegetation, and on the ablation of patchy snow. For a start, the authors should consider following publications and references therein: Pomeroy and Gray (1995); Liston (1995); Liston and Sturm (2002); Winstral et al. (2002); Essery and Pomeroy (2004); Erickson et al. (2005); Deems et al. (2008); Gruenewald et al. (2010); Schirmer et al. (2011); Mott et al. (2012).

> ***Response:** The authors agree that the listed references are relevant in terms of discussing this study in the context of general snow distribution studies, rather than just focusing on snow studies within the Dry Valleys. These references along with a brief discussion of their relevance to the scope of this paper will be included in a revised manuscript.*

• Comment 6: It is not clear what the reason was for the ablation modeling, which is a bit lost in the manuscript. It seems that the conclusions that the authors get from the modeling is that it "shows the same temporal trends" is quite obvious, because the snow cover as well as SWE will decrease during the summer season. I am not sure what additional information is gained from the model. The authors might want to consider either taking the modeling out of the paper or discussing it in more detail, so it is actually supportive for their findings.

> ***Response:** This study is set in the context of snow patches being an important ecological control over biogeochemical cycling and the distribution of microbial communities. Snow patches may act as a control in two ways: 1) snow cover acts as a thermal insulator to the underlying communities (snow-covered area is important in this respect), 2) Appreciable amounts of melt from snow patches is a source of moisture for underlying communities (snow water equivalent is important in this respect). This study relies heavily on using remote sensing imagery to infer snow dynamics of the region, but this methodology only provides information on snow-covered area. It is important, therefore, to show that landscape patterns of snow-covered area are also true for snow water equivalent. This modeling merely serves as a check that snow-covered area is reflective of snow water equivalent at the landscape scale (We do not believe this is true at the snow-patch scale, however, due to microtopographic influences). Note that the modeling of snow water equivalent does not entirely match observations of snow-covered area, which is discussed in the relevant section.*

• Comment 7: The manuscript needs generally more references to Figures, particularly in the Results and Discussion sections.

> ***Response:** In a revised manuscript, the Results and Discussions Sections will be reviewed to add references to figures wherever it can clarify the explanation of results.*

Line comments

• P3824, L7: "aeolian redistribution" is usually referred to as "wind transport" in snow literature. Change "... topographic lees along valley bottoms" to "topographic lees and along valley bottoms..."

Response: ***The phrase "aeolian redistribution" will be changed to "wind transport" in a revised manuscript. The phrase "... topographic lees along valley bottoms" will not be changed to "topographic lees and along valley bottoms...", because this will change the meaning of the sentence (i.e. the topographic lees are located along the valley bottoms and snow is deposited in such lees).***

• P3824, L10: Change "spatial and temporal dynamics" to "spatial and temporal distribution".

Response:" Spatial and temporal dynamics" refers not only to the distributions, but also the mechanisms that control ablation.

• P3825, L10: delete "very".

Response: ***Accepted revision. The revised manuscript will reflect this revision.***

• P3825, L11–26: There is quite a bit of repetition in these two paragraphs, and they should be rewritten in a more concise way. E.g. 1) argument of thermal insulation on L18 and on L26, 2) influence of meltwater on biology on L13, L23 and L28.

Response: ***These lines will be revised to reduce any redundancy.***

• 3826, L16: example for "inappropriate" use of "seasonally" in "...understanding the temporal dynamics of ablation seasonally..." It should either be "...understanding the temporal dynamics of ablation during the ablation season..." or "...understanding the difference in temporal evolution of ablation between two different ablation seasons...".

Response: ***Accepted revision. The revised manuscript will reflect this revision. The revised line will read: "Additionally, understanding temporal dynamics during the ablation season and the factors controlling ablation is also necessary."***

• P3827, L10: I assume that "nivation hollows" can also be described as "concave areas", in which case they would be easily identified using digital elevation models. Including such an analysis would be a useful addition for this manuscript.

Response: ***Nivation hollows or concave areas (also referred to as topographic lees in the manuscript) in which snow patches form are on the order of tens of meters in size (see P3826, L9-12). The digital elevation model (DEM) used in the analysis has a spatial resolution of 30 meters. Therefore, the DEM does not have the spatial resolution sufficient to resolve the nivation hollows. A DEM with sufficient spatial resolution was not available. These limitations will be discussed further in the Methodology section of the revised manuscript.***

• P3827, L17–19: "With respect, ..., underlying topography": This sentence is lacking proper references that have discussed snow distribution with regard to topography before 2012. See Comment 5 in "General comments" for more details and possible citations.

Response: ***This sentence was intended to refer only to those studies specific to the Dry Valley region. This will be clarified in a revised manuscript. However, general studies on snow distribution will also be referenced here.***

• P3827, L19–21: "This implies, ..., landscape scale": This sentence is not clear and conflicts with the sentence that follows. Please rewrite.

> ***Response:*** ***The phrase "not preferentially distributed with respect to particular topographic parameters" in this sentence refers to snow being distributed somewhat ubiquitously across the landscape. The next sentence refers to shapes of distribution patterns (i.e. SCA as a function of elevation, slope, and aspect). However, these "early season" distribution patterns are not uniform because they reflect the underlying topography. In other words there is more snow at certain elevations because that is a broader elevation contour. So, snow is distributed somewhat ubiquitously at the onset of the ablation season, but the distribution is not uniform with respect to topography because the underlying topography is not uniform. These sentences will be clarified in a revised manuscript.***

• P3827, L26–28: There are many studies that link elevation to ablation that should be cited here.

> ***Response:*** ***Additional studies will be cited here in a revised manuscript.***

• P3828, L14: A short summary of the method should be given here, additional to the reference.

> ***Response:*** ***The rationale for delineating the areas of interest will be briefly discussed in a revised manuscript.***

• P3829, L3: Change "dynamics" to "temporal evolution of the extent of individual snow patches".

> ***Response:*** ***Accepted revision. The revised manuscript will reflect this revision.***

• P3829, L17: Change "snow" to "temporal and spatial evolution of snow patches".

> ***Response:*** ***Accepted revision. The revised manuscript will reflect this revision.***

• P3829, L18: What does "explicit basis" mean? You might want to consider removing it.

> ***Response:*** ***Accepted revision. The revised manuscript will reflect this revision.***

• P3829, L27: Please quantify "fine enough".

> ***Response:*** ***The phrase "fine enough scale" refers to plot sizes large enough to contain enough snow patches (on the order of dozens) to statistically determine the degree of overlap between seasons. This will be clarified in a revised manuscript.***

• P3830, L5: "greater accumulation" should be replaced with "greater spatial snow cover", because there is no information on how big the accumulation is (due to lack of snow depth/ density information).

> ***Response:*** ***Good point. The revised manuscript will reflect this revision.***

• P3830, L6: "and the dates ..." is a repetition from L2 and can be removed.

> ***Response:*** ***Accepted revision. The revised manuscript will reflect this revision.***

• P3831: See also Comment 3 in "General comments": the third scale is somewhat confusing. The plot area sometimes referred as the "snow-patch scale", as in Figure 3. But there seems to be the scale that talks about 15 different snow patches (L15), which is also somehow the snow-patch scale. This should be clarified. Also how were those latter snow patches chosen (L15) to be representative of the region? It would seem to be quite hard to pick a "representative" patch.

> ***Response:*** ***The 1 km^2 plots were selected to investigate the "snow-patch" scale (i.e. snow patches within the plots), but the resolution of this scale is not necessarily 1 km^2. The 15 individual snow patches were also used to investigate the "snow-patch" scale. The "snow-patch" scale is intended to refer to individual snow patches or a collection of several snow patches at a length scale of tens of meters or less. It is a good point that this is not explicitly defined in the manuscript. A revised manuscript will clarify the referenced language and explicitly define the "landscape" and "snow-patch" scales.***

• P3831, L21 – P3832, L6: It is not clear how this analysis is done and the sentences seem to be very complicated.

> ***Response:*** ***This paragraph will be rewritten as necessary to clarify the methodology used.***

• P3832, L14: Change "times series" to "time series".

> ***Response:*** ***Accepted revision. The revised manuscript will reflect this revision.***

• P3832, L13–15: "For both seasons, ..., the season": I had to read this sentence multiple times to understand what it means. It should be rewritten in a simpler way, e.g. "The aerial ablation rates from both seasons were calculated using the time series of snow-covered area".

> ***Response:*** ***Revising this sentence in this manner would leave out a step in the methodology. I believe the confusion may stem from the use of the word "differentiated". In this context, the word is intended to have a mathematical connotation (i.e. a fitted curve was calculated and the derivative of the curves was then calculated). This is a necessary step because the original time series is a set of discrete data points. This will be clarified in a revised manuscript.***

• P3832, L21: Two years are not enough to be talking about "trends".

> ***Response:*** ***Your comment is correct. The original intent of this statement was to suggest that the Lake Hoare meteorological data is representative of all the regions (i.e. the other regions exhibit similar changes in air temperature, wind speed, etc. throughout the ablation season). The use of the phrase "seasonal trends" is misleading and a revised manuscript will clarify this statement.***

• P3832, L27–28: Please specify what the characteristics are for the 5 regions, how they were determined, and subsequently what the characteristics of the 5 modeled snow patches are.

> ***Response:*** ***For the UEB model, parameters are the same for each of the five regions. The phrase "characteristics representative of the 5 regions" refers model inputs such as snow density, snow depth, etc. These inputs are the same for each region, however. Only the meteorological data is unique for each region. This will be clarified in a revised manuscript.***

• P3833, L6–7: The assumption that the snow temperature is equal to air temperature is not necessary correct and the implications of this assumption should be briefly discussed.

> ***Response:*** ***Yes, this is not necessarily correct, but this assumption is only used to initialize the model (see Tarboton and Luce, 1996). We do not have actual snow temperature data to initialize the model. This will be clarified in a revised manuscript.***

• P3833, L13: "generally held belief" need to be referenced.

> ***Response:*** ***This is not something that has been reported on previously. Researchers have assumed that snow patches will form in the same locations each year based solely on field experience. Our study is the first to include data that supports this claim.***

• P3834, L8: Why was the 2009–2010 snow covered area used as a baseline here, when all other calculations (Eq. 1–3) use 2010–2011 as baseline?

> ***Response:*** ***This sentence does not contradict the explanation of Equations 1-3 (P3830, L17-19), which states the 2009-10 season is used as a baseline. This means that "% Seasonal Overlap" is defined by how much of the 2010-11 SCA intersects with the 2009-10 SCA. One can view this as setting the 2009-10 snow patches as a baseline, and seeing how much of the 2010-11 snow cover returns to these same locations. I believe that the confusion stems from the fact that the denominators of Equations 1-3 contain data from the 2010-11 season. What is intended by "baseline" will be clarified in a revised manuscript.***

• P3834, L11: It is not clear to me why you assume that most snow patches in 2010 have ablated by the time the first images were taken. Why could it not be that the snow never accumulated in that season? Please explain.

> ***Response:*** ***In the Bonney Region, imagery was not available until late in the ablation season during 2010-11 (see Figure 2). During the time from peak accumulation (mid-October) to the date of the first available image (late November), positive energy fluxes into the snow patches must have induced ablation. Other regions exhibit ablation during this time. However, this is not stated definitively in the manuscript.***

• P3834, L25: I do not understand the sentence "The modes of the distribution, ..., single season".

> ***Response:*** ***The intent of this sentence is to state that the modes of each distribution with respect to elevation, slope, and aspect exhibit a much lesser snow-covered area for the 2010-11 ablation season (see Figure 7). This will be clarified in a revised manuscript.***

• P3836, L4–5: "Temporal changes, ..., landscape scale". This sentence/ conclusion seems a bit obvious in the ablation season, because the snow usually ablates at all scales, so the trend will always be the same. There is no reason to assume that the snow at the local scale will not ablate while the snow at regional scale does, because once ablation occurs at large scale it is obvious that this ablation reflects the ablation at the small scale. I am therefore not sure what the point of that conclusion is. It might need to be rephrased.

Response: ***This sentence was intended to state that selected snow patches reflect similar aerial ablation rates observed at the landscape scale. This is not an obvious conclusion, because microtopographic effects produce a significant amount of variation among individual snow patches with respect to aerial ablation.***

• P3836, L27–28: "There is clearly, ..., distance from the sea.": Assuming that the scale "Along-valley distance" in Fig. 10a means that a distance of zero is the distance from the sea, this sentence does not make sense, because the snow-covered area is decreasing along-valley distance. Please either correct or clarify.

Response: ***You are correct. This sentence will be corrected in a revised manuscript to read "… gradient of snow-covered area decreasing with along-valley distance (distance from sea)."***

• P3838, L25: Please specify "milder".

Response: ***"Milder" refers to meteorological conditions (wind speed, air temperature, etc) that are less favorable for ablation. This will be clarified in a revised manuscript.***

• P3839: See also Comment 5 in "General comments".

Response: ***See response in "General comments".***

• P3840, L20–21: "shallow snow patches have been observed to ablate faster.": I assume "ablate faster" refers to "aerial ablation", in which case this sentence is redundant, as it is quite clear that a shallower snow patch will disappear faster than a deeper snow patch. I might be misunderstanding the sentence, in which case it needs to be rewritten and clarified.

Response: ***While this paper does not include quantitative data to show mass ablation rates are greater for shallow snow patches, this effect has been observed in the field. This is why there appears to be different aerial ablation rates for neighboring snow patches of similar size in area.***

• P3842, 5–7: Please see suggestion of references (Comment 5), which have come up with identical conclusions.

Response: ***This statement was intended to be specific to the Dry Valleys region. However, references pertaining to general snow studies will be cited here in a revised manuscript.***

• P3843, 5–9: I do not follow the argument that similar temporal trends (increasing ablation (SWE and areal) during ablation season) leads to the conclusion that the snow-covered area is proportional to the mass of ablated snow. E.g. If the snow consisted of deep snow patches, then the area would be losing mass but there would be no aerial ablation.

Response: ***Your example of deep snow patches is correct, but the temporal trends of discussion are for observations at the landscape scale. The assumption herein is that the entire set of snow patches across the landscape (on the order of tens of thousands) consists of a wide range of varying depths. Thus any effects associated with snow depths are averaged out. This will be clarified in a revised manuscript.***

• Figure 5, caption: Does "smoothed over weekly timescales" mean using a running mean over 7 days?

> ***Response:*** ***Yes, it is a running mean over 7 days. In a revised manuscript, this will be added to the discussion of meteorological data in the methodology section.***

• Figure 6 A: What are "total snow patches". Is that all available data? Please define.

> ***Response:*** ***"Total snow patches" refers to snow patches from the 1 km^2 plots in all five identified regions. This is explained in the methodology and results sections, but the Figure 6 caption will be revised for clarification.***

• Figure 10, caption: There is no location in this caption.

> ***Response:*** ***In a revised manuscript, the location of this region will be identified in the caption of Figure 10.***

References

Deems, J. S., Fassnacht, S. R., and Elder, K. J. (2008). Interannual consistency of fractal snow depth patterns at two Colorado mountain sites. Journal of Hydrometeorology, 9(5):977–988.

Erickson, T., Williams, M., and Winstral, A. (2005). Persitence of topographic controls on the spatial distribution of snow in rugged mountain terrain, Colorado, United States. Water Resources Research, 41(W04014).

Essery, R. L. H. and Pomeroy, J. W. (2004). Vegetation and topographic control of wind– blown snow distributions in distributed and aggregated simulations for an Arctic tundra basin. Journal of Hydrometeorology, 5:735–744.

Gruenewald, T., Schirmer, M., Mott, R., and Lehning, M. (2010). Spatial and temporal variability of snow depth and ablation rates in a small mountain catchment. Cryosphere, 4(2):215–225. Liston, G. and Sturm, M. (2002). Winter precipitation patterns in Arctic Alaska determined from a blowing-snow model and snow-depth observations. Journal of Hydrometeorology, 3:646–659.

Liston, G. E. (1995). Local advection of momentum, heat and moisture during the melt of patchy snow covers. Journal of Meteorology, pages 1705–1715.

Mott, R., Gromke, C., Grünewald, T., and Lehning, M. (2012). Relative importance of advective heat transport and boundary layer decoupling in the melt dynamics of a patchy snow cover. Advances in Water Resources, this issue.

Pomeroy, J. W. and Gray, D. M. (1995). Snowcover: Accumulation, redistribution and management. Science report no. 7, National Hydrology Research Institute, Saskatoon, Saskatchewan, Canada.

Schirmer, M., Wirz, W., Clifton, A., and Lehning, M. (2011). Persistence in intra-annual snow depth distribution: 1. measurements and topographic control. Water Resources Research, 47(W09516).

Winstral, A., Elder, K., and Davis, R. E. (2002). Spatial snow modeling of wind–redistributed snow using terrain based parameters. Journal of Hydrometeorology, 3:524–537.

RESPONSE TO REFEREE COMMENTS BY R. MOTT

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 referenced 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 analyzed 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,

> ***Response:* The conclusion that snow patches generally form in the same locations each year is specific to the McMurdo Dry Valley landscape. This is something that has not been previously confirmed and is integral to understanding spatial processes associated with biogeochemical cycling and the structuring of microbial communities. For researchers investigating the Dry Valleys, this is a very important conclusion. Landscape characteristics, climatic patterns, and snow distribution mechanisms are unique in the Dry Valleys, and are not reflected in other more general snow studies, typically in alpine environments. Additionally the spatial scale (tens of meters) is much finer than what is typically investigated. The revised manuscript will clarify how this conclusion differs from previous publications, and such previous publications will also be referenced. An expanded discussion of microtopographic effects and a review of relevant literature will be included in the revised manuscript.***

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 analyzed. 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.

***Response:* While the authors agree such analyses would contribute greatly to understanding snow dynamics in the Dry Valleys and the associated impacts on biogeochemical cycling and microbial communities, the spatial resolution of the only available digital elevation model (DEM) does not allow such analyses. While the WorldView and QuickBird imagery has a spatial resolution of at least 0.5 meters, the available DEM only has a spatial resolution of 30 meters. Additionally, including such analyses would greatly increase the scope of the paper making it cumbersome and difficult to read.**

The reference list is incomplete. Although not focusing 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.

***Response:* Again, the Dry Valley landscape is unique to typical alpine environments. However, the above references are still relevant and will be included in the revised manuscript.**

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

***Response:* This study is set in the context of snow patches being an important ecological control over biogeochemical cycling and the distribution of microbial communities. Snow patches may act as a control in two ways: 1) snow cover acts as a thermal insulator to the underlying communities (snow-covered area is important in this respect), 2) Appreciable amounts of melt from snow patches is a source of moisture for underlying communities (snow water equivalent is important in this respect). This study relies heavily on using remote sensing imagery to infer snow dynamics of the region, but this methodology on provides information on snow-covered area. It is important, therefore, to show that landscape patterns of snow-covered**

area are also true for snow water equivalent. This modeling merely serves as a check that snow-covered area is reflective of snow water equivalent at the landscape scale (We do not believe this is true at the snow-patch scale, however, due to microtopographic influences). Note that the modeling of snow water equivalent does not entirely match observations of snow-covered area, which is discussed in the relevant section.

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 (landscape, plot and snow patch scales)

Response:* *Additional snow distribution studies will be reviewed in the Introduction Section, including those mentioned in the above comments. The language used to describe the multiple spatial scales will be clarified.

Methodology:

What are the length scales of the snow patches observed?

Response:* *This information is supplied in the introduction section, P3826 L9-12 ("..on the order of tens of meters in size.") This will also be added to the Methods Section.

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

Response:* *I do not understand this comment. The spatial resolution, along with a description of the remote sensing platform is described in the second paragraph of the methods section (after identifying the study areas, but before any description of the data set or analyses).

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

Response:* *"Dynamics" refers to spatial and temporal dynamics. The line will be revised to read "spatial and temporal dynamics".

What makes a snow patch representative for your analysis?

Response:* *All snow patches on the "valley bottoms" with the exception of those immediately adjacent to lakes and glaciers are included in the analysis. The defined areas of interest (all

snow patches within are included) are displayed in Figure 1 and a detailed rationale for delineating the boundaries is included in Eveland et al, in press.

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

> ***Response:*** ***This study is set in the context of snow patches being an important ecological control over biogeochemical cycling and the distribution of microbial communities. Snow patches may act as a control in two ways: 1) snow cover acts as a thermal insulator to the underlying communities (snow-covered area is important in this respect), 2) Appreciable amounts of melt from snow patches is a source of moisture for underlying communities (snow water equivalent is important in this respect). This study relies heavily on using remote sensing imagery to infer snow dynamics of the region, but this methodology on provides information on snow-covered area. It is important, therefore, to show that landscape patterns of snow-covered area are also true for snow water equivalent. This modeling merely serves as a check that snow-covered area is reflective of snow water equivalent at the landscape scale (We do not believe this is true at the snow-patch scale, however, due to microtopographic influences). Note that the modeling of snow water equivalent does not entirely match observations of snow-covered area, which is discussed in the relevant section.***

Results:

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

> ***Response:*** ***In a revised manuscript, a few sentences will be added to the results section discussing aspect and slope relative to Figure 7.***

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)

> ***Response:*** ***Yes, the authors agree that a data set including snow depths would be very informative, but our knowledge of this is limited to field experience. As mentioned in previous responses, microtopographical factors will be discussed more in the Discussion Section of a revised manuscript.***

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!

> ***Response:*** ***In the manuscript, it is eluded to that this is due to microtopographic effects producing some persistent snow patches fairly ubiquitously across the landscape. A few lines will be added to the discussion section in a revised manuscript to make this clear.***

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

> ***Response:** **I could not find the specific line to which this comment is referring. The relevant section has been reviewed for ambiguity in the use of the word “season”. On P3837, L7, the line “…near the end of the season.” has been revised to read “…near the end of the ablation season.”***

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.

> ***Response:** **This is related to the comment above about rates of aerial ablation. In the manuscript, it is eluded to that this is due to microtopographic effects producing some persistent snow patches fairly ubiquitously across the landscape. A few lines will be added to the discussion section in a revised manuscript to make this clear.***

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

> ***Response:** **This study is set in the context of snow patches being an important ecological control over biogeochemical cycling and the distribution of microbial communities. Snow patches may act as a control in two ways: 1) snow cover acts as a thermal insulator to the underlying communities (snow-covered area is important in this respect), 2) Appreciable amounts of melt from snow patches is a source of moisture for underlying communities (snow water equivalent is important in this respect). This study relies heavily on using remote sensing imagery to infer snow dynamics of the region, but this methodology on provides information on snow-covered area. It is important, therefore, to show that landscape patterns of snow-covered area are also true for snow water equivalent. This modeling merely serves as a check that snow-covered area is reflective of snow water equivalent at the landscape scale (We do not believe this is true at the snow-patch scale, however, due to microtopographic influences). Note that the modeling of snow water equivalent does not entirely match observations of snow-covered area, which is discussed in the relevant section.***

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

> ***Response:** **This is a good suggestion. A few lines will be added to discuss possible changes 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.

Response: ***While this paper does not include quantitative data to show mass ablation rates are greater for shallow snow patches, this effect has been observed in the field. This is why there appears to be different aerial ablation rates for neighboring snow patches of similar size in area. The point of this paragraph is to emphasize such microtopographic 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.

Response: ***Yes, the point of this statement is to emphasize importance of local topographic differences. This sentence will be revised to avoid any confusion.***

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 analyze 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)

Response: ***While the authors agree such analyses would contribute greatly to understanding snow dynamics in the Dry Valleys and the associated impacts on biogeochemical cycling and microbial communities, the spatial resolution of the only available digital elevation model (DEM) does not allow such analyses. While the WorldView and QuickBird imagery has a spatial resolution of at least 0.5 meters, the available DEM only has a spatial resolution of 30 meters. Additionally, including such analyses would greatly increase the scope of the paper making it cumbersome and difficult to read.***

P 3841, l9-10: add "at landscape scale"

Response: ***Accepted revision. The revised manuscript will reflect this revision.***

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

Response: ***In a revised manuscript, a few sentences will be added to the results section discussing aspect and slope relative to Figure 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!

Response: ***Yes, a few lines will be added to discuss changes with wind speed at different elevations. See previous comments for reasons for not extending the analysis.***

P 3842 l 12: you should add a reference here

Response: ***Past snow studies will be referenced here in a revised manuscript. Note that these studies are the Dry Valley snow studies referenced and discussed in the introduction section.***

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.

> ***Response:*** ***The point of Figure 12 is to show that spatial distribution patterns will evolve throughout the ablation season in the same manner each season. In other words, the distributions shown in Figure 12 are only a function of total snow-covered area and not a function of specific seasons or times within a season. Dividing the figure by year would not display the intended purpose of this figure. The discussion of this figure will be re-evaluated to ensure the intent of this figure is properly described. The authors agree that Figure 14 by itself is not a major contribution to the manuscript, but the snow water equivalent modeling is an important check to ensure that snow-covered area is reflective of snow water equivalent. The importance of SCA and SWE and the relationship between the two will be reinforced with a few additional lines in the discussion section in a revised manuscript.***

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.

> ***Response:*** ***This is not necessarily true for snow patches in the Dry Valleys (at least for the valley bottoms, which was our study area). The available imagery that is latest in the ablation season shows nearly zero snow-covered area in our areas of interest. To the best we can infer from remote sensing imagery and field experience, all or nearly all of the snow patches on valley bottoms will ablate entirely by the end of the ablation season and re-form the next seasons (generally in the same locations). We do not have imagery from very late in the ablation, however, so we do not confirm or reject this assumption in this manuscript. However, inter-annual consistency in snow cover is important to local ecology, which is emphasized throughout the manuscript.***

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?

> ***Response:*** ***Language referencing "season" and "seasonality" will be clarified in a revised manuscript. Any additional confusing language regarding "season" and "seasonality" will be clarified throughout the manuscript. Yes, the distribution of snow is a function of elevation. This point is emphasized in another accepted publication by the authors (Eveland et al, in press), which focuses more on spatial distribution patterns. The manuscript presented here is an expansion of such study.***