

Interactive comment on “The seasonal evolution of albedo across glaciers and the surrounding landscape of the Taylor Valley, Antarctica” by Anna Bergstrom et al.

Anonymous Referee #1

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General

The authors present a study of airborne surface reflectance measurements over different landscape classes in the McMurdo Dry Valley, yielding at characterizing the albedo variability over the ablation zone of glaciers during summer. The manuscript lacks a clear hypothesis and presents a useful dataset for the region instead. The measurements were carried out thoroughly and the relation of the results to in situ observations at weather stations is meaningful. However, I have two strong suggestions to the authors, which should be implemented in the paper.

First, for the authors aim to characterize the temporal and spatial variability of albedo,

the exclusion of satellite remote sensing data (P2, L32-33 and P3, L3-5) is not justified enough. Therefore, I strongly recommend comparing the measurements to an independent dataset of spatial albedo, which is e.g. the albedo product from MODIS. There might be lags between the acquisition dates, issues with spatial resolution (maybe even a just a few available pixels), cloud cover and high solar zenith angles. However, the MODIS albedo product is widely used in Polar science and discussing space vs airborne albedo measurements would bring additional benefit to the manuscript and to the robustness of the dataset.

Second, in the discussion the authors give various reasons for the peculiarities of their results. Unfortunately, most of the reasons are not well quantified, due to lacking field evidence and thus, remain a bit speculative. I clearly miss a discussion how the high proportion of sublimation of up to 80% of total glacier ablation (Lewis et al., 1998) could impact spatial or temporal albedo variations (Lhermitte et al., 2014; Winkler et al., 2009). Since the authors have in situ measurements from weather stations, they could estimate conditions for sublimation from the difference of the vapour pressure or the dew point temperature between the surface and the near surface air layer, at times when the surface temperature is below 0°. Consequently, it would be possible to distinguish between melt and sublimation events in the days or weeks prior to the albedo acquisition.

Structure

Although the authors state to focus on glacier surfaces, their analyses often concern lake and soil surfaces. I think this is valid as the airborne measurements potentially increase our knowledge of albedo variations within the McMurdo Dry Valley compared to other remote sensing data due to a higher spatial resolution. However, in the discussion the three different surface types are often mixed and it is not always clear which one is addressed. Maybe clearer structure in the discussion or a synoptic table in the conclusion could clarify this point.

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Discussion

Chapter 4.1 "seasonal shift in albedo without the presence of snow" possibly applies an incorrect analogy. The authors focus their analyses on glacier surfaces, but use ice aging and structural changes of lake ice as an explanation for their findings. Lake ice and glacier ice have a different genesis, different deformation and recrystallization fabrics. At least for glacier surfaces I doubt that this analogy holds, especially as the relationship is just qualitatively. A more rigorous assessment applying this method on glacier ice would strengthen the manuscript.

For the discussion of spatial and temporal albedo patterns in a wider Antarctic context, I miss a connection to the results of Pirazzini (2004).

Figures

Figures 1 and 2 are too small to read accurately. Especially in Figure 2 b+c the reader can not distinguish the colour of the points and the buffers. In Fig 2c it is not clear that the Thiessen polygons really connect the measurements to the closest meteorological station. It seems that some points in the purple part would be closer to the green part.

Specific comments

P4, L12-15: How was a level measurement of the surface reflectance assured? Was the weighted box equipped with tilt meters and was a correction of the data applied (Weiser et al., 2016)? P4, L25-27: Could you add the solar zenith angle of your acquisition time? P5, L28: Could you add the slope of the location of the weather stations? P6, L7 and caption of Figure 2: Correct spelling is Thiessen. P6, L9 and throughout the manuscript: I assume you can delete percent as either the unit of the given number states it, or like in Figure 4 or 5 dimensionless values are shown. P7, L16 and Figure 3: There is also a high variation in incoming shortwave radiation on 7 Dec 2015, 22 Nov 2017 and 7 Dec 2017. What happened on these dates? Section 3.2: Could you add an explanation, if the lakes were permanently frozen, or if there were open water areas

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or melt ponds? P12, L8: You should be able to estimate melt and refreezing conditions from the weather stations.

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