Response to Reviewer 1 comments. My co-authors and I thank this reviewer for providing very helpful feedback. We have addressed each comment and indicate how we have done so in red below. Overall we believe that the manuscript has greatly improved with the help of this and the other reviewer's comments.

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.
We have taken this suggestion and have incorporated the MODIS albedo product data in our study in order to compare our albedo measurements with MODIS. We have added a section into the methods describing how we acquired these data, a figure in the manuscript comparing all measurements to MODIS, two figures in the supplemental information comparing both apparent and corrected albedo to MODIS for each flight, and a section in the discussion about our findings from the comparison.

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.
We have added a figure (now figure 6) including a timeseries of air temperature in order to better discuss the energy balance and how this relates to changes in albedo. We believe that calculating

the energy balance conditions that lead to sublimation and melt require more complex modeling that is beyond the scope of this study (i.e. Lewis et al., 1998, Hoffman et al., 2008, 2014, 2016). Modeling work for this time period is underway and we plan on publishing results of this work that include the relationships and feedbacks between albedo and ablation.
We do agree that sublimation likely does affect albedo and have added sentences to the discussion addressing that (now p 14 lines 20-27).

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.
We agree that certain parts of the manuscript mix surface type and this discussion may be confusing. We have edited throughout to try and clarify this. We believe the biggest source of confusion was in section 3.2 (now 4.2). This section has been edited heavily with most of that language removed. We thank the reviewer for this suggestion and believe the manuscript is now more clear.

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.
We have been very explicit in the manuscript that while both lakes and glaciers have ice whitening, the ice structure is different between them and the exact ways in which ice albedo changes are a function of that difference. We discuss the previous research on lake ice in the second and third paragraphs of section 4.1 (now 5.1). We discuss what we know about ice whitening in glacier ice from previous research in the fourth, fifth, and sixth paragraphs of that section. We also state twice in this section that there are differences between lake and glacier ice. We respectfully disagree with this comment and believe that we adequately separate seasonal changes in albedo between lakes and glaciers and do not attempt to apply research on lakes to glaciers.

For the discussion of spatial and temporal albedo patterns in a wider Antarctic context, I miss a connection to the results of Pirazzini (2004).
We have included comparisons to this study throughout the manuscript. We appreciate the reviewer bringing this paper to our attention. It has added valuable discussion material to this manuscript.

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.
Figures 1 and 2 have been enlarged. We believe that all details of each figure are now discernable.

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)?
We observed the box throughout the flight on most flights and believe the box was maintained level in flight due to the weight and the fin incorporated in the box design. Unfortunately tilt meters were not available to us and we do not have those data to be able to make those corrections. We do add language acknowledging this, discuss how swing can be a potential source of error, and estimates of the error adapted from Allison et al. (1993) as per the suggestion of reviewer 2. This can be found in the new section we added titled "Error sources and albedo correction"

P4, L25-27: Could you add the solar zenith angle of your acquisition time?
We added a sentence at that location stating the range of solar zenith angles (54.3 – 61.8 degrees) across all flights.

P5, L28: Could you add the slope of the location of the weather stations?
The meteorological stations are all leveled and are measuring reflected radiation on a flat part of the glacier (slope = 0°). We have added this to the text to clarify this fact.

P6, L7 and caption of Figure 2: Correct spelling is Thiessen.
This has been corrected in both locations.

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.
We have changed all text and figures so albedo ranges from 0 to 1 and use of percent has been removed from the manuscript. We thank the reviewer for calling this inconsistency to our attention.

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?
The variation is due to clouds on those days. We attempted to makes all flights on uniformly sunny or cloudy days while balancing helicopter availability, weather, and well-spaced data acquisition throughout the season. We did not have perfect conditions on those days due to a stretch of cloudy days (Figure 2) and limited helicopter availability because of weather delays. We are able to mostly control for this by using the closest meteorological station for incoming radiation data.

Section 3.2: Could you add an explanation, if the lakes were permanently frozen, or if there were open water areas or melt ponds?

The lakes are permanently frozen across the majority of their area. Moats of open water melt around the shoreline of the lakes however, they are less than 100m wide and therefore are not included in this analysis.
Canada and Taylor Glaciers have melt ponds that also have a permanent ice cover.
We add some clarifying language to this section to indicate that no measurements are made over open water for either glaciers or lake ice.

P12, L8: You should be able to estimate melt and refreezing conditions from the weather stations.
This section is referring to the process of refreeze of meltwater as it is moving through the ice matrix and supraglacial stream network. If meltwater is generated high on the glacier and far from a supraglacial channel (i.e. long travel time) it is highly likely that it will refreeze before it leaves the glacier. Melt is generated in the subsurface due to internal heating from penetrating shortwave radiation, rather than sensible heat. This melt and subsequent refreeze process is rather intensive to calculate and cannot easily be estimated from meteorological stations. For this reason, we believe it is beyond the scope of this paper. This process has been modeled however and we believe it is sufficient to cite that work to support our hypotheses here. A citation has been added to this line to clarify that this process has been identified for these glaciers.