

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2022-76

Anonymous Referee #2

Referee comment on "Metamorphism of snow on Arctic sea ice during the melt season: impact on spectral albedo and radiative fluxes through snow" by Gauthier V erin et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-76-RC2>, 2022

The manuscript presents a study on field observations of evolving snow physical properties and albedo during the early melt season on landfast ice in Canada, and uses those results for a snow albedo model analysis. The study nicely connects the changes in relevant physical properties to changes to albedo. Overall, the manuscript is well written, well organized, and with some minor revisions, it would be in good shape for publication. In particular, the discussion section was enjoyable to read. Please see comments below that I hope the authors will find useful.

General: It's not clear why the albedo results at the 500 nm and 1000 nm wavelengths are emphasized. Presenting the numbers in the visible (300-700 nm) and near infrared (1000+ nm) bands would make it easier to compare with previous works (e.g., Brandt et al., 2005) and be more relevant for remote sensing applications.

Lines 21 and 27. It would be useful to know the snow depth at which the visible band in albedo begins to decrease.

Lines 36-38. During winter, there's little sunlight, so the albedo the surface is not important. In spring and summer, it's important.

Line 39. The melt season begins when the snow starts melting. Snow may affect the duration of sea-ice melt.

Lines 40-41. This is true for thin sea ice, but not for thick sea ice. Snow has little effect on the amount of light reaching the ocean if the ice is very thick.

Line 45. The authors may be interested in reading an updated review of snow and ice optical properties by Warren:
<https://royalsocietypublishing.org/doi/full/10.1098/rsta.2018.0161>

Line 52. It would be worthwhile to add a description in the text about the limitations of using SSA for snow crystal representation in optical modeling.

Line 76. There are some cases where snow persists all summer.

Line 80. 'albedo drops remarkably'
It would be informative to include the albedo change from dry to melting snow. My understanding is that a change from 0.85 to 0.70 is not that remarkable relative to the change from snow (0.85) to melt ponds (0.25-0.65).

Lines 82-84. This is an overextension of results. Snow melt does not directly enhance snowfall.

Lines 86-87. In some cases, the snowpack is deep enough that it never fully melts away, as observed around ridges:
<https://online.ucpress.edu/elementa/article/10/1/000072/169460/Spatiotemporal-evolution-of-melt-ponds-on-Arctic>

Lines 91-92. 'However, studies which aim to link physical and optical properties of snow still remain largely qualitative'
This isn't true. Warren, Brandt, Grenfell, Perovich, and others have made a lifetime of work in linking snow physical and optical properties, including their co-evolution. I suggest rewording this section so that it recognizes that this work is standing on the shoulder of giants and is adding to a foundation of knowledge.

Lines 92-93. It is true there are data limitations, but the greater limitations may be the representation of physical processes, which are difficult to appropriately incorporate as parameterisations into earth system models.

Lines 94-95. There are several field campaigns that have done this.

Lines 116-117. How far away was the meteorological station? It would be helpful to include that information here.

Lines 135-136. What information was used to determine the auto-adjustments? Does the auto-adjustment create inconsistencies in the noise level of the measurements?

Line 140/Figure 2. These are useful photos. Is it possible to replace them with higher resolution versions?

Lines 146-147. What makes a relatively thinner snow pack less suitable? Wouldn't the combination of thin and thick be more representative?

Lines 177-178. The instrumental uncertainty of the probe would be helpful to include here.

Line 182. It would be good to expand on this a little more. What types of snow have larger uncertainties?

Line 196. typo 'them'

Line 265/Figure 3. Why are there different shades for the different horizontal bars? The shades don't match the gray legend in the bottom panel.

Lines 274-275. Often, there can be melt forms near the ice-snow interface from the previous autumn. Were there no melt forms observed at the base of the snowpack?

Lines 279-280. It would be informative to describe how the temperature gradient was reversed. Was the temperature range the same but with the upper surface being -4.5 to -5C, or do the authors mean that the snowpack was simply warmer near the surface and cooler near the base?

Lines 286-287. Did snowpack temperatures increase from the top down?

Line 300/Figure 4. Just after the May 8 snowfall, the snow depth increases. What caused the increase if no snowfall occurred?

Lines 315-317 and lines 320-321. I'm surprised by the higher density values for indurated depth hoar and the lower density values for wind slab in this study. Can the authors

comment on this with regard to previously observed values? Is it possible that the fresh snowfall events contributed to the density measured in the uppermost portion of the snowpack, lowering the average density for the wind slab layer?

Lines 325-326. Figure 6 doesn't show the distinct vertical layers. Is there a way that this can be added to the figure?

Line 330. Same comment as before that Figure 6 doesn't show the distinct vertical layers of the snowpack.

Line 335/Figure 6. What does the white at the base of these profiles represent? Is it no data? Also, how much of the variability in the uppermost profiles before May 25 is due to spatial heterogeneity versus variable weather conditions, such as snowfall events? It may be insightful to comment on this in the text.

Line 346. 'Some of these new layers were thick enough to be distinguishable in Figure 6.' It would be helpful to highlight these in Figure 6 somehow since they are not obvious. Was snow density of these new snowfall layers measured?

Line 356. Typo Jun 6.

Lines 357-358. This is a little confusing. How did the mass of the snow increase without notable snowfall events (Figure 4)?

Table 1 caption. Do you mean Figure 5 here? I suggest adding an additional column that describes the predominant snow layer morphology (induluted depth hoar, etc.) so that readers don't have to scroll back and forth to know which layer means what. Also, it looks like there may be a typo for Layer I.

Lines 377-378. Is it possible that the sloped surface of the dunes, and therefore the angle of reflectivity, affected the albedo measurements?

Line 385/Figure 7. It would be helpful to add the sample size for each panel, e.g., $N = 15$ to better interpret the changes between phases. It would also be informative to note what fraction of the albedo measurements were made over melt ponds.

Line 395. Similar to the previous comment, it would be informative to note what fraction of the albedo measurements were made over melt ponds in this section.

Line 407. 'which ranged from...'

It would be informative to add shading or thinner lines to Figure 8 to show the range or spread of the absorption spectra from the 12 samples.

Lines 408-409. Shouldn't it be divided by the snow density to get the average absorption coefficient per volume of snow?

Lines 414-416. It's not clear how these values were determined. Was this some sort of sensitivity study with some details missing in the methods section, or manually trying different values until a decent overlap was reached with the observed average?

Line 417. These measurements were only made over snow dunes, is that correct? It would be helpful to add that note in the text here to remind readers.

Lines 482-483. It would be interesting to include the snow depth at which PAR becomes significant.

Line 543. Rain events occurred? That would be informative to include in Figure 4. What impact did the rainfall have on the albedo and snow properties?