Reply to Anonymous Referee #2

We copied the review below and imbedded our responses within.

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.

We thank the Reviewer for this positive appreciation and for constructive comments.

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.

This is an interesting point also made by Reviewer 1. We will not repeat our response to Reviewer 1. Briefly, our focus is on the link between snow physical properties and albedo. These are best investigated by performing calculations for specific wavelengths. We cannot extend our paper to the multiple other possible applications such as remote sensing and comparison with other works, which by the way did not measure snow SSA. In any case, our Figure 7 can be used by the interested reader to compare our data with those of Brandt et al. 2005.

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

There is no well-defined depth threshold? The snow depth at which albedo starts to decrease depends on several variables which include snow density and snow SSA. No precise value can be given. We therefore much prefer not to engage in such a discussion.

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

We agree with the Reviewer. However, this does not affect the validity of our statement, that snow in winter and spring reflects up to 90% of incoming solar radiation.

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

We agree with the Reviewer. Here again, this does not affect the validity of our writing.

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.

Snow affects the amount of light reaching the ocean regardless office thickness. Now, we do agree with the reviewer that with specific applications in mind, ice may by itself reduce *incoming light sufficiently that snow effects do not matter anymore. Here, we are just mentioning a physical process.*

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

Thank you for this interesting reference. It does not however affect the validity of our text.

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.

There are sure limitations we are well aware of and we have contributed to quantify them (Picard et al., 2009). However, SSA (or equivalently optical diameter) is used in most radiative transfer models. Such a discussion would be justified in a paper focused on the importance of SSA, particle shape, etc. in optical modeling. This is however not our focus and we do not wish to lengthen our paper in what we feel are unnecessary considerations, at least for our present scope.

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

Indeed. However, this is anecdotic and does not affect our statements.

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).

We added "by about 0.15" line 82 to quantify this. We still think this is remarkable, as it doubles the radiative energy absorption.

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

We agree with the Reviewer. Our writing very clearly indicates that that snowmelt contributes to an indirect effect that enhance snowfall. "The combined effects of surface melting and atmosphere warming enhance the air moisture content, often producing persistent overcast conditions leading to snow precipitations." This is reported in the references we cite. We never write that snow melt directly enhances 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

Indeed, this is the case. However, (Webster et al., 2022) themselves do write "Even so, a few snow drifts by ridges persisted throughout the melt season." Indicated that this is anecdotic with little if any impact on our arguments. We therefore feel it is not useful to lengthen our text with only marginally relevant statements.

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.

We agree that "still remain largely qualitative" is an exaggeration. What we meant is that all variables required to calculate albedo were not measured. Calculations therefore had to rely on the estimation of some physical variables, in particular snow SSA. Our study is the first to actually measure albedo on sea ice and calculate if from measured variables only, and this is the reason to focus our work on the relationship between physical and optical properties. We do recognize the extremely valuable pioneering work of Warren, Brandt, Grenfell, Perovich and others, and cite a number of their papers. However, they never measured snow SSA and we therefore add an increment to the topic. We changed "still remain largely qualitative" to "never measured all variables required to calculate albedo, and relied on the estimation of some physical variables, in particular snow SSA." Lines 93-94.

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.

It is not clear to us which physical processes the Reviewer is referring to.

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

We respectfully disagree. We are not aware of any study on sea ice which measured both optical and physical properties of snow, in particular which would have measured all snow properties required to calculate albedo, such as SSA.

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

We replaced "close to the ice camp" with "about 100 m from the ice camp". Line 119.

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?

The "auto-adjustment" consists in increasing / decreasing the integration time to maintain the maximal numerical count in the spectra (usually around 500 nm) in the range 70-100% of the maximal count (i.e. 100% is the saturation level of the sensor). This is exactly how works other field spectrometers (ASD, SVC, …). It does not create "inconsistencies" (if we understand this term correctly) because in general the integration time does not change between the incident and reflected measurements. The maximum numerical count is in fact similar for both measurements because the snow albedo is close to 1 around 500 nm. We propose to remove this sentence to avoid confusion because this is a standard operating mode, there is nothing special in this auto-adjustment.

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

This is the TCD version, with reduced resolution. The final version will be significantly improved.

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

As stated in the following line "because thicker layers are more suitable to determine accurately irradiance profiles." Line 150. The next sentence also explains "The optical absorption by LAPs in the snowpack were determined from the exponential rate of irradiance decrease in a homogeneous layer (Belke-Brea et al., 2021; Tuzet et al., 2019) so that thicker layers yield more reliable measurements."

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

We added "(0.1°C accuracy)" line 182.

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

We now specify "and is larger for soft snows such as depth hoar and fresh snow". Line 187

Line 196. typo 'them'

Thank you. Changed.

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.

Throughout the Figures, light grey is for 2016 and dark grey is for 2015. This is shown in the legend box in the lower panel and in the horizontal bars separating both panels in indicating the different phases. We have slightly modified the colors and layout to insure it is obvious.

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?

No melt forms at the base of the snowpack were observed. We are reporting only observations that were made, not observations that could have been made or expected and were not made, for concision. Also, please note that melt forms formed in fall can be totally transformed to depth hoar and undetectable in spring. Since we only performed spring campaigns, we feel it would be speculative and not useful to discuss all possibilities not confirmed by observations.

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?

We now specify line 284 that "The subsequent increase in air temperature led to surface warming and a reversal the temperature gradient in the snowpack".

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

The previous paragraph about Phase I explains that at the end of Phase I the surface warmed and the temperature gradient reversed, so that the bottom of the snowpack was then colder than the top. The modification detailed above will make it clear that during phase II, temperatures decreased 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?

Our observations started on May 13, so we unfortunately are unable to comment. The most likely response is erosion and deposition by wind, but at this point, this would be speculation so we prefer not to comment.

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?

While wind slabs are often denser that indurated depth hoar layers, it is not rare to have basal indurated depth hoar denser than upper wind slabs. We have added line 327 "Having basal indurated depth hoar denser than upper wind slabs is not rare on sea ice (Sturm et al., 2002)." Figure 3 and Table 2 of (Sturm et al., 2002) show their layer "d", indurated depth hoar, to have an average density of 344 kg m^{-3}, while layer "j", wind slab, has an average density of 316 kg m^{-3}.

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

The Reviewer probably means horizontal layers, rather than vertical ones. Our initial draft of this Figure did feature layer boundaries and grain types. Layer boundaries have interest only if grain types are shown. However, the Figure was then so cluttered that it was essentially illegible, so we opted for the current version, which we feel is preferable for most or all readers. In many cases (not all, we admit), layer types can be inferred from SSA and density values.

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

Same response as above.

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.

White regions indeed mean no data. This has been added to the caption. We have added "Since no major modification affected the snowpack during Phase I, except near the surface, a lot of the variability observed in the stratigraphies of Figure 6 during Phase I results from spatial rather than time variability." Lines 338-339.

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?

We have specified line 358 "Some of these new layers were thick enough to be distinguishable in Figure 6 because of their high SSA." These are clearly visible on May 26, May 28, and May 30. The densities appear on the lower panel of Figure 6.

Line 356. Typo Jun 6.

Changed, thank you.

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

Several snowfalls did occur after June 4, as reported in Figure 4. The Reviewer may think these snowfalls are not notable because they did not result in significant increase in snow height. This however is because the general decreasing trend in snow height caused by melting masks the contribution of these snowfalls to snow height. In any case, we do not draw any strong conclusion from the impact of these snowfalls and also mention melting bottom sea ice as a cause of negative freeboard (line 365).

Table 1 caption. Do you mean Figure 5 here? I suggest adding an additional column that describes the predominant snow layer morphology (indulated 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.

We do mean Figure 5, thanks for spotting the typo. We have added a column to describe the predominant snow layer morphology to Table 1. There was indeed a typo for layer I, thanks again for spotting that.

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

We added lines 385-389: "The slopes of the dunes where measurements were made was barely perceptible, most likely lower than 1° in all cases. (Picard et al., 2020) have evaluated slope effects on albedo. Under diffuse light, over entirely snow-covered surfaces, a slope has no impact on albedo (their Figure 3). Considering a SZA of 45° and only direct light, the error caused by a 1° slope is slightly over 0.01. Given that the slopes involved here are <1° and that there is always significant diffuse light, we estimate that the slope-caused error is always <0.01, and in most cases much smaller, so that we neglect it."

In passing, the thicker snow does not necessarily mean that the surface elevation was higher. It can be that the ice is thinner and we think this is what happened in most cases.

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.

The sample size has been added to the caption. During the campaign pond limits were not always clear so that in most cases an ice-water mixture was measured, as now indicated in the caption.

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.

Same response as above.

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.

The spread is too large to allow a clear Figure. Instead we added in the caption "At 500 nm, values ranged from 0.03 to 0.42 m^{-1}" to indicate the range of values. Line 440.

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

Reviewer 1 made a similar comment. We again apologize for the lack of clarity. We have clarified this, lines 421-423. "It was divided by the density of water (1000 kg m^{-3}) and multiplied by the average density of snow (350 kg m^{-3}) in order to obtain an average absorption coefficient of the impurities in the 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?

To clarify this point, the new wording says "a mixture (red dashed line) of 1700 ng g^{-1} of MD and 14.4 ng g^{-1} of BC were found to be the best values to correctly simulates, in term of shape and amplitude, the average particulate absorption measured within the snow." Line 428.

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.

Indeed, this is written in the text, just one line down (now line 431): "we used our 2015 vertical irradiance profiles obtained in five snow dunes"

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

We have added "below about 20 to 25 cm" line 498. This value was mentioned later on, line 571.

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?

It did rain on 22 June 2016, after all the snow was gone. The rain therefore had no impact and it is not useful to discuss it. We removed the mention to rain, line 562.

References

Picard, G., Arnaud, L., Domine, F., and Fily, M.: Determining snow specific surface area from near-infrared reflectance measurements: Numerical study of the influence of grain shape, Cold Regions Sci. Tech., 56, 10-17, 10.1016/j.coldregions.2008.10.001, 2009.

Sturm, M., Holmgren, J., and Perovich, D. K.: Winter snow cover on the sea ice of the Arctic Ocean at the Surface Heat Budget of the Arctic Ocean (SHEBA): Temporal evolution and spatial variability, J. Geophys. Res., 107, 8047, 10.1029/2000jc000400, 2002.

Webster, M. A., Holland, M., Wright, N. C., Hendricks, S., Hutter, N., Itkin, P., Light, B., Linhardt, F., Perovich, D. K., Raphael, I. A., Smith, M. M., von Albedyll, L., and Zhang, J.: Spatiotemporal evolution of melt ponds on Arctic sea ice: MOSAiC observations and model results, Elementa: Science of the Anthropocene, 10, 10.1525/elementa.2021.000072, 2022.