Reply to Anonymous Referee #1

We copied the review below and imbedded our responses within.

General comments

The study by Vérin and colleagues investigates snow metamorphism on the arctic sea ice and in particular how it impacts on spectral albedo, and how well the albedo variability can be reproduced using a RTM. I believe that this study provides great insights into the temporal variability of snow sea ice albedo during the melt season, and that the albedo dataset could be useful to parametrize or validate the evolution of snow sea ice albedo in global models. The results are clearly presented and the methods are robust - my comments are minor and mostly focused on the albedo measuring and modelling since snow metamorphism is out of my expertise.

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

Specific comments and technical corrections

Title: Arctic marine snow can be confusing because it commonly refers to debris sinking in the ocean, maybe it would be clearer to indicate "Arctic snow on sea ice" or equivalent in the title?

We have changed this phrase to "snow on Arctic sea ice" in the title

Abstract:

Line 19-20: Maybe a minimum BBA value can be indicated here rather than the value at 1000nm? Then it makes it comparable with the number from line 18.

Indeed. We have replaced this with the BB value (0.76, line 20).

Line 24-25: "based on measurements" is probably unnecessary here because "measured" is already written earlier in the sentence - maybe reformulate to ".. using measured SSA, density vertical profile and impurity content"?

Actually, impurities were not measured for each profile unlike SSA and density. We changed the wording to "Spectral albedo was simulated by radiative transfer using measured SSA and density vertical profiles, and estimated impurity contents based on limited measurements.", line 24.

Line 25: "calculationS" - also, "at the interface snow-ice" may be clearer than "at the top of the sea ice"?

Thank you. Changed.

Introduction:

Line 56-57: What about longwave radiation on overcast days – can this also have an impact on the dry snowpack?

The important variable is temperature, which is determined by the energy budget. LW radiation is one term of this energy budget, and special consideration of this term is not necessary here.

Line 70: In Domine, F., et al. "Three examples where the specific surface area of snow increased over time." *The Cryosphere* 3.1 (2009): 31-39, the authors conclude that "SSA increases are probably not rare", including cases that are described in this paragraph like depth hoar formation. Is this reference maybe worth mentioning here? (especially as other "occasional" processes are mentioned in the introduction, such as summer snowfalls in line 86.

Sure, SSA increases are not rare but the general and most common process is SSA decrease. In Domine 2009, depth hoar formation leads to SSA increase only when it forms from melt-freeze crusts, which is not the case here. Summer snowfalls add new snow, so this cannot be considered as the SSA increase of a given snow layer. Therefore, citing Domine (2009) does not appear justified here. (Domine is writing this).

Line 74: "samplings" is probably incorrect, maybe "a lot of samples" or "a lot of sampling"?

"samplings" seems fine to us.

Line 93: global radiative transfer "in" sea ice not "of"?

Sure, changed to "in". Thank you.

Line 101: "light absorption" may be clearer than "optical absorption"?

Sure, changed to "light". Thank you.

Line 102: instead of "and using measurements on melted snow samples", maybe "and measuring absorption coefficients of LAPs isolated from snow samples"?

Thank you, suggestion followed (line 103).

Line 116: I think that the correct spelling is "set up", not "setup"?

Setup is in the dictionary, with the intended meaning.

Line 157: LAPs are often distributed in the top cms – if the top 7cm are discarded, how is it possible to get data on the LAPs from the profiles? Or do the authors mean that the top 7cm was removed only from thin snowpacks on line 150, which is why thick snowpacks were used for retrieval of LAPs properties?

LAPS are preferentially distributed in the top cm mostly in melting snow. In dry snowpack, deep dark layers are very common, see e.g. (Kaspari et al., 2014). We do not say that the top 7 cm were discarded, only that "profiles were normalized to the irradiance at a depth of 7 cm". In any case, irradiance profiles were used to determine the type of impurities based on their spectra, rather than the actual absorption coefficients, which were determined from impurities extracted from melted snow. The new wording lines 160-163 reflects this. "Analysis of the profiles yield the spectral absorbance of LAPs, which were used to determine the type of absorbing particles (essentially black carbon (BC) or mineral dust). LAPs were also analyzed using chemical methods to obtain absorption coefficients, as detailed in the following section."

Line 180: Is it possible to indicate the reference of the electronic scale and the sensitivity?

We specify that the resolution of the scale was 0.1 g (line 185). The reference of the scale is not known at present.

Line 196: "then" instead of "them"

Changed, thank you.

Line 207: "As the snowpack was already ripe, the study of spatial variability using large scale measurements was favored." What do large scale measurements mean here – UAV measurements, or transect measurements?

Changed to "transect measurements" line 212.

Line 213: short wavelengthS

Changed, thank you.

Line 223: Is it possible to indicate at what time were the albedo measurements recorded?

Albedos were recorded at various times during the day. We unfortunately cannot specify the time of each measurement. SZA was between 47 and 57°, as specified line 244.

Line 238-240: Why were simulations performed considering diffuse radiation and not using the exact SZA values corresponding to each albedo measurement?

As stated line 243, diffuse radiation is equivalent to direct light with SZA ~ 50°. As stated line 244, the SZAs of our measurements were always between 47° and 57°, close to 50°. We therefore make the implicit approximation that SZAs between 47 and 57° are equivalent to 50°. Sky conditions were not always fully overcast of bright blue. A variable cloud cover was often present. Using the actual SZA would therefore not be necessarily better than our approximation. To clarify this, we write line 244 "simulations were performed by approximating the illumination conditions with diffuse radiation." Note alos that the following sentence reads "Doing so, the maximal error on albedo is ~0.01 at 1000 nm." To further justify the approximation.

Line 252: "whose" to replace with "of which"?

Merriam Webster grammar specifies "animals and objects, lack a possessive form, so whose *can be used for their possessive forms as well, as in "the movie, whose name I can't remember." Please see https://www.merriam-webster.com/words-at-play/whose-used-for-inanimate-objects' . "whose" is therefore correct here and we keep this as is.*

Line 259-260: So density, SSA and irradiance profiles were not measured in 2016? Please indicate this in the methods when describing the sampling and analysis of snow physical properties (2.3 and 2.2.2)

Line 149, section 2.2.2, we added "in 2015". Line 180, section 2.3, we added "These were performed in 2015 and not in the melting 2016 snowpacks."

Figure 3: How were the wavelengths of 500 and 1000 for albedo chosen? Why not calculate broadband albedo in the IR and VIS? Or an averaged value?

Reviewer 2 has made a related comment so we need to address this in detail.

First, why use 500 and 1000 nm?

The reason for presenting the results for two wavelengths at 500 and 1000 nm is two-fold:

- a single wavelength is a true surface property, independent of the incident solar spectrum (especially cloudy vs clear-sky conditions) and is easier to compare or to generalize than wideband albedo (integrated in the VIS or NIR) which depends on illumination conditions.

- 500nm and 1000nm are round numbers representative of the VIS and NIR domains. They are respectively close to the minimum of absorption (450-550nm) where SSA has a minimum effect and the first main absorption feature of the ice (1030nm) where SSA has a predominant effect. They are also in the domain of sensitivity of our spectrometer.

Of course, an infinite number of wavelength combinations can be investigated: VIS, IR, PAR, MODIS bands, bands from other instruments on various satellites, etc. These wavelengths combinations would require additional Figures that would lengthen an already long paper. More importantly, it would detract the reader's attention from our actual focus: investigating the relationship between snow optical and physical properties.

In conclusion, for our purpose, linking physical and optical properties, we believe the wavelength chosen are sensible although probably not unique, but choices have to be made. To keep pour paper focused and concise, we much prefer to limit our results presentation and discussion to these 2 wavelengths. Note however that we do discuss the full solar spectrum and PAR in Figures 10 and 11, and we believe this extension is sufficient. Numerous other Figures could be added, but we really do not wish to lengthen our paper and we need to stay focused on our purpose.

Is it possible to indicate more clearly the different phases in the figure - eg the end of phase 3 in 2015 seems to extend to end of June on the figure but extends to mid-June in the text?

2015: we left on 18 June, so we have no data beyond that date. We have now specified this in the Figure caption (line 271) to make this clear.

Similarly, phase II starts on May 19 in the text but before may 14th in the figure.

Phase 2 starts on May 25 in the text (line 286), and in the Figure as well.

The albedo of the highly heterogeneous ponded sea ice from 2016 was calculated from the transect measurements?

It was calculated from all available measurements in 2016 and these were mostly transects. We do not think it is critical to detail this.

Is it possible to indicate in the figure legend how many measurements are included in the box plots?

This was already done in the lower panel of Figure 2, which specifies the number of samples corresponding to each data set.

Line 294: Did you mean "The transition from snow cover to bare ice"?

Yes, thank you. Changed line 299.

Line 356: Typo "junE 6"

Changed, thank you. Line 364.

Line 365: "wicked up the first snow layer" I don't understand the meaning of this, could it be reformulated?

We added "by capillary rise" line 372.

Table 1: Is it possible to indicate in the methods or in the table how many samples were analysed for density and SSA to derive averages and standard deviations?

The number of samples has been added in parentheses in Table 1.

Figure 7: Why are albedo data presented only from 400nm if the spectroradiometer could measure from 300? Is it possible to indicate the number of samples used in the boxplots, or at least a range? What does "specific albedo spectra" mean, are they averages, or how were these examples of bare ice and melt pond albedos chosen?

The signal was too low below 400 nm. We changed 300 to 400 line 128. The number of samples has been added to the Figure 7 Caption. We changed "two specific albedo spectra over bare ice only and melt pond only are also shown." to "one albedo spectrum over bare ice and one spectrum over a melt pond are also shown." in the Figure 7 Caption.

Line 409: I am not sure I fully understand what was done here: "It was 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". If I understand correctly, the absorption of particulates from melted snow leads to a coefficient expressed in m-1 of melted snow.

How can this then be multiplied by the density of snow, leading to units of kg snow per m4 of snow, and give an "average absorption coefficient"? and where are these "average coefficients" shown? Do the authors mean that they divided the coefficient in m-1 of melted snow by the density of water and then multiplied it by the density of the snowpack in order to get an absorption coefficient in m-1 of snow instead of melted snow?

If possible, would it be possible to divide the absorption coefficient in m-1 by the LAP concentration (in kg m-3) in the solution of melted snow that was filtered to carry the spectrophotometric analysis in order to get a mass absorption cross section in m2 kg-1? Then the data could be used in other widely used radiative transfer model such as SNICAR.

We apologize for the lack of clarity. We now specify line 421 that "It was divided by the density of water (1000 kg m^{-3}) and multiplied by the average density of snow (350 kg m^{-3})". Units therefore remain in m^{-1}.

Line 434: "It is possible to fit each spectral albedo with optimized LAP concentrations, but without measurement in each pit, this is not very meaningful" I do not understand what the authors mean here – if it is possible to retrieve the impurity concentrations by inversing TARTES, why would it not be meaningful?

Indeed, it is always possible to invert TARTES to retrieve a hypothetical impurity concentration for each profile. However, our preferred approach, mentioned line 428, has

been to use a single value for all snowpits, close to the limited number of measured values. This is certainly debatable, but no more than fitting each profile without measurement of impurities in each profile.

Line 438: "consistence" -> "consistency"

Thank you. Changed, line 455

Line 444-447: It would be clearer to have this paragraph at the beginning of the section 3.4.2 to understand the reasoning in comparing LAPs concentrations between 2015 and 2016 in line 443.

We agree. This paragraph has been moved to the start of section 3.4.2. Thank you.

Figure 9: typo "albedo witH LAP" (h missing) and maybe write "LAPs" instead of "LAP" because both MD and BC are included in the simulations if I understood correctly. It would be beneficial to add the phases as in Figure 3.

The legend box has been corrected as requested. Phases are indicated by grey shading, as explained in the caption.

Lines 458-462: It would be clearer to indicate only absolute values without units in this paragraph since the relative errors in % are in the table already – the error at 500nm is given in % (indicated as absolute value?) whilst the 0.04 and 0.02 are indicated without % units (are they %?).

We apologize for this unclear writing. All values are absolute, there should not be any %. This has been modified. Lines 477-480.

Line 467: Why were LAPs omitted in these simulations (Figure 10) if they improve the fit between measured and modelled albedo?

LAPs were not omitted.

Line 512-513: the link in parenthesis should come before the dot and there should be a dot and a space after the last parenthesis

Changed, thank you. Line 531

557-558: According to figure 3, the albedo at 1000nm still varies a lot in phase III (similar slope than phase II?)?

It seems that the Reviewer is mixing up data from 2016 and 2015. The dates for Phase 3 are different in both years. In Figure 3, if one considers only data for Phase III in 2015 and in 2016, paying attention to the different dates, it is clear that the albedo at 1000 nm for Phase III varies little. Of course. If the dates for phase III in 2015 are used to analyze albedo data for 2016, it does appear that albedo varies a lot, but this is clearly an error. The reduce the risk of error we have reworded line 576 as ", as visible in Figure 3 for both 2015 and 2016 and in Figure 7c."

Line 568: What is meant by "the coupling with the grain size"?

We changed "coupling" to "enhanced impact of LAPs for low SSA values". Line 588.

Line 575: What is meant by "not adjusted"?

We changed to "used without adjustment". We feel that the term "adjusting" to described fitting values in models is pretty common. Line 595.

Line 600: snow cover instead of coverS

We believe our writing is correct. Line 620.

Reference

Kaspari, S., Painter, T. H., Gysel, M., Skiles, S. M., and Schwikowski, M.: Seasonal and elevational variations of black carbon and dust in snow and ice in the Solu-Khumbu, Nepal and estimated radiative forcings, Atmos. Chem. Phys., 14, 8089-8103, 2014.