

The Cryosphere Discuss., author comment AC2
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Reply on RC2

Zhonghai Jin et al.

Author comment on "Validation of a fully-coupled radiative transfer model for sea ice with albedo and transmittance measurements" by Zhonghai Jin et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-106-AC2>, 2022

General Comments

This manuscript aims to validate the implementation of a sea ice radiative transfer model into the advanced COART. The model inputs are vertically resolved salinity, temperature and density. These inputs together with the phase-equilibrium relationship developed by Cox and Weeks 1983 are used to predict brine and air total volume. Using these latter, an empirical mathematical equation developed by Light 2003 calculates gases and brine channels size distributions assuming inclusions to be spherical. Mie theory is used to predict the inherent optical properties (IOPs) of the different layers representing sea ice. A radiative transfer model based on the discrete ordinates method is finally used to calculate the output apparent optical properties (AOPs).

Modelled AOPs based on structural measurements are validated by comparison with measured AOPS obtained during the SHEBA and ICESCAPE campaigns. Three scenarios were considered: first-year ice bare ice, multi-year bare ice and ponded ice. The effect of soot impurities and algae are also assessed for these scenarios.

A model incorporating Mie theory and using physical structure to calculate AOPs would be a valuable tool as mentioned by the authors. However, the fact that the model as to be tuned in order to obtain agreements undermines the validity of this model. Two aspects (1) and (2) would have to be addressed in order to demonstrate the validity of the model.

We thank the Reviewer for the feedback. Before proceeding to the point-to-point response, we'd like to briefly address this general comment. We believe a major misunderstanding generated most of the questions below, and it has to do with the choice of using the word "tuning". The main objective of this paper is to evaluate our IOP parametrization for bare ice and the relative RT processes. Within our approach, the bare-ice IOPs are parameterized exclusively as a function of the fundamental ice physical properties: salinity, density, and temperature. Such parameters, together with the ice total thickness, are based on real measurements, obtained after a thorough search for the best available datasets (see below for more explanations). We simply invert for the density needed to best fit the measurements. The density values are within reported typical ranges for each of the SSL, DL or IL. In one case (19 July), we were even able to

use the density measured in a core extracted next to the location of the optical measurements.

Major Comments

1. The inclusions distributions described by Light 2003 are valid for columnar (interior) ice. These distributions of inclusions size and shape are probably significantly different in the drained layer (DL) and surface scattering layer (SSL) because of several processes (e.g., surface melting/refreezing, channel drainage, air bubble inclusion under dynamic growth, surface ablation by sunlight, etc.). The induction of inclusions distributions describing interior layer to drained and surface scattering layer might explain the mismatch between the untuned model and measurements. The author should suggest a different approach in order to predict inclusions distributions for these two layers. Microstructural observations of the SSL obtained during the MOSAiC expedition could be a start for such a model (when they will be available).

We understand that the size distributions for the inclusions described in L03 are sampled from the interior ice. We extended it to the SSL and DL because there are no such measurements specifically for these top layers. In fact, this assumption is no different from the assumption of a constant scattering asymmetry factor in all ice layers used in other studies (e.g., Briegleb and Light, 2007, L08, L15, and Lamare et al., 2022). Since the scattering asymmetry factor is solely dependent on the size distribution for a given wavelength, using the same asymmetry factor in different ice layers implies a constant size distribution profile. Actually, in contrast to the studies mentioned above, our assumption is less severe (more physical), because the asymmetry factor in our approach depends on wavelength and also on the ice physical properties (i.e., it is layer-dependent). Some assumptions are obviously inevitable whenever measurements are not exhaustive. However, the good model-observation agreement in the spectral albedo indicates that using the size distributions of the IL also for the SSL and DL is acceptable, particularly in the NIR where the modeled albedo is more sensitive to the optical properties of the SSL and the DL. The agreement might be partially attributed to the (generally) much thinner dimensions of the SSL and DL, and to error compensation between size distribution and ice density, similar to the error compensation between the asymmetry factor and the adjusted scattering coefficients in the studies referred to above.

We have made several attempts at including at least a preliminary version of MOSAiC data, but constantly hit the embargo that will last until January 2023. Requests to PIs for preliminary versions failed as well. On the other hand, it was demonstrated in L08 that SHEBA and ICESCAPE data, albeit collected 20 years apart, show remarkable consistency (see response to your point 2 for more details). In addition, we are not sure how reliable the inclusion distributions obtained in the loose, fast-changing SSL of a few centimeters are, and if it is possible to measure them at all. Can the reviewer advise on how to promptly obtain such data? Notwithstanding the fact that we'll always strive to implement the most recent advances in the model, the fate of the paper cannot depend on potential availability of future datasets.

Regarding the suggestion of "a different approach in order to predict inclusions distributions", the development of new techniques to obtain size distributions is out of the scope of this paper. The model can accommodate any size distribution, but in absence of reliable measurements it makes little sense to privilege one or the other if the fit to the measurements are satisfactory.

2. The density, temperature and salinity measurements as well as absorbing particles concentrations used as inputs are often guessed by the authors because they were not measured on the field at the location of AOPS measurements.

Without these reference measurements, the system is undetermined. Unfortunately, it is difficult to demonstrate the validity of the model without having the actual measurements or justifying the choice of the input with strong evidence. A solution could be to change the scope of the paper in order to study the sensitivity of AOPS to the tuning of the different inputs. Another suggestion would be to validate the model by comparison with another model. However, point (1) would need to be addressed first.

Again, this is a misunderstanding. For the modeling, we use every available observational data for input. The ICESCAPE and SHEBA data proved to be the most suitable for our study. Furthermore, the appendix completes the study by highlighting the insensitivity to small variations in a lot of these parameters. The properties of the SSL have instead to be estimated (because exhaustive measurement sets are not yet available) based on the measured albedo in the near IR spectrum, as already done in L08.

The vertical profiles we use are documented by Polashenski et al (2015) and partially in L15, and we have used their exact values. We were even able to use density measurements for the 19 July 2011 case, based on the uppermost 80 cm of the annotated core (see L15, Fig. 7). Effectively, we adjusted the ice density in the other simulations only when forced by the lack of in-situ observations, but the adjustments ranged within commonly accepted values which also include those found in L08. The fact that these measurements are scarce (and we advocate for extensive collections of vertical profiles as one of the final messages) cannot be held as a flaw in our approach. Note that the manuscript highlights the use of such observations in several places, e.g.:

- Lines 147-148: "We strived to use all available observational data to determine the input to the model, focusing on two common ice types: bare and ponded ice."
- Lines 156-157: "The Solar Zenith Angle (SZA) is calculated based on the reported observational time and location latitude and longitude."
- Lines 166-168: This paragraph has been slightly corrected to: "We were able to exploit one of the few density measurements collected during ICESCAPE, although the density was only measured in the uppermost 80 cm where it varied between 0.625 and 0.909 g/cm³. When such measurements are not available, we assume typical FYI densities as reported by Timco and Frederking (1995): between 0.84 and 0.91 g/cm³ for the ice above the waterline, and between 0.90 and 0.94 g/cm³ for the ice below the waterline."
- Lines 185-188: "For the salinity, we use the profiles reported from the core analysis of Polashenski et al. (2015), averaging the available data points in each model layer. Since measurements are not reported for the SSL (which normally gets destroyed when collecting the core) and the bottom of the ice, we assume the shallowest and deepest value extend to these remaining portions."
- Line 306: (slightly modified): "The vertical profiles of salinity are also from cores, and vary between 0.5 and 3.0 ppt (Polashenski et al., 2015)."

When not measured in situ, we use data based on pertinent observations reported in the literature. As for "**absorbing particles concentrations used as inputs are often guessed**": please note that the particle concentration is NOT an input variable and is not guessed but determined by the ice density, temperature, and salinity through the Cox-Weeks equation for the given size distributions (see Eqs 1-3). Among the three ice properties, salinity is the one most available for the examined cases. To avoid large temperature uncertainties, we chose melting ice and ponded ice cases, for which the top temperature can be set to a very good approximation to 0°. Because the ice base (ice-water interface) can be fixed at -2°C and the temperature profile in the ice is generally very close to linear (based on observations), the temperature in each layer is easily

estimated. In addition, our sensitivity test shows small dependence on temperature (see Appendix). The density is the most scarcely measured variable, and we therefore use reported observational values (L15; Timco and Frederking, 1995). One of our goals is to improve the radiative transfer in sea ice in the NASA climate model (ModelE), which simulates the ice temperature and salinity profiles in sea ice but not the density. Therefore, the ice density is an adjustable parameter in ModelE. Accordingly, we present a physically-consistent parameterization of bare sea ice IOPs as a function of its physical properties that can be directly measured or modeled in climate models. In contrast, many alternative sea-ice RTMs employ as input parameters directly the IOPs (extinction and scattering coefficients, and asymmetry factor), which are very challenging to measure in-situ or even in the laboratory. It is certainly true that a comprehensive set of in-situ measurements, required for a rigorous validation of RTM in sea ice in all conditions, is presently lacking. Because in our physically-based parameterization the IOPs are linked to the ice properties, changing the ice density changes all the IOPs consistently and differentially in different spectral bands: band-by-band adjustments as in the studies referred to above are not needed. If the parameterization is not correct, obtaining model-observation agreement in both albedo and transmittance (again, simultaneously and at all wavelengths) is extremely unlikely even if a few unknown input properties are "adjusted". For all these reasons, we have full confidence that our approach is legitimate to validate radiative transfer processes in sea ice. Should more complete suites of input parameters become available in the future, the focus can shift towards the betterment of the IOP parametrization.

"However, point (1) would need to be addressed first": This size distribution issue has already been addressed in the response to point (1) above.

The detailed explanation above has been condensed in a new paragraph added to the beginning of Sec. 3:

"The ICESCAPE and SHEBA data proved to be the most suitable for our study. Of all the physical variables needed at input and measured in situ, the total ice thickness and the vertical profiles of salinity within the ice column are the most available. For ICESCAPE, we use the exact values of layer-resolved salinity documented by Polashenski et al. (2015) and partially in L15. For SHEBA, density profiles from cores are generally very scarce. We were able to use density measurements for the 19 July 2011 case, based on the uppermost 80 cm of the annotated core (see L15, Fig. 7). When forced by the lack of in-situ observations the values were varied within commonly accepted ranges, which also include those found in L08. Temperature profiles are also sporadic, but estimates based on straight physics can be used that do not substantially affect the quality of the fit, as shown in the Appendix. For example, the top temperature of ponded ice can be set at 0°C because this is the water-ice coexisting temperature of water and ice. The bottom ice temperature of -2°C is based on the freezing temperature of sea water."

Finally, all available RT models for sea ice are approximated to some level. As for the comparison with other models, it was indeed done in Light et al. (2003), which compared DISORTB (an earlier version of our RT solver used in COART) and found that the modeled sea ice albedo and transmittance are consistent with their 4-stream results as should be (see Figs 4-6 in L03). Their model is most commonly used for applications to sea ice RT. The extended COART model is more advanced and can adopt any number of streams (not just 4), in addition to the coupling feature and the treatment of ice roughness.

Reference: Light, B., G. A. Maykut, and T. C. Grenfell (2003), A two-dimensional Monte Carlo model of radiative transfer in sea ice, *J. Geophys. Res.*, 108(C7), 3219.

Specific Comments

28 Morphological changes and thinning of sea ice along with sea ice cover reduction are responsible for lower albedo and shortwave absorption in the ocean (Arndt and Nicolaus 2014).

Yes, thinning of sea ice (and *some* morphological changes, not all) also contribute to ice albedo reduction, but to a much smaller extent. We have modified the text to read: "A reduction in sea ice cover and its thinning lower the albedo and increases shortwave absorption in the ocean, causing more melting in a mechanism known as ice-albedo feedback (Curry et al., 29 1995; Hall, 2004; Déry and Brown, 2007, Arndt and Nicolaus 2014)

46 The range is probably wider than that, especially for SSL. Would that be the range for columnar sea ice?

This is a very good point! The density of the SSL is largely unknown, although some descriptions are contained in very recent publications (<https://www.osti.gov/servlets/purl/1844399>). Since this point is made in the introduction, we prefer to specify that this is a general range for bulk sea ice. According to Timco and Frederking (1996) "the in situ density of sea ice may be quite different above and below the waterline. In the upper part of the ice sheet, there may be a wide variation in the ice density, with realistic values in the range 0.84 to 0.91 Mg m⁻³ for first-year ice, and 0.72 to 0.91 Mg m⁻³ for multi-year ice. However, below the waterline, the density values are much more consistent and range from 0.90 Mg m⁻³ to 0.94 Mg m⁻³ for both types of ice."

86 What is the value of the refractive index used for ice? Please provide a number or a range for this value.

As specified in the manuscript, the spectral refractive index of ice is taken from Warren and Brandt (2008). A plot of it has been uploaded as a supplement (see figure "S1_rev2"). We have added "spectral" to the sentence in the text. We think that reporting specific values makes the text unnecessarily heavy, without being particularly useful.

88 The explanation of how the surface implemented is insufficient. Water and ice have different roughness, therefore the parameters of the Gaussian equation describing its roughness should be different. From an optical perspective, it would be helpful to have a description of how it translates into the distribution of diffuse reflection. It would also be useful to describe the importance of purely specular reflection at the ice surface in the model.

We have added the following section to the Appendix:

"Ice surfaces are naturally rough, and the extended COART model offers full flexibility in the treatment of roughness (Jin et al., 2006). . A Gaussian equation is used to describe the statistical distribution of the surface facets, in a similar fashion as the Cox-Munk model used to parameterize the distribution of ocean waves. The extension to any other distribution is trivial, should observational evidence indicate the need. Since no sunglint has been reported on ice surfaces and granular features are observed, the sea ice surface is likely rougher than a calm ocean surface. Lacking appropriate measurements of surface roughness statistics, we simply used $\sigma=0.5$ and $\sigma=0.1$ for the ice and pond surface, respectively. Based on the Cox-Munk formulation, $\sigma=0.1$ represents a minimal ocean surface roughness (wind speed=1.4 m/s). For the ice cases in this study, a coarse-grained surface layer was observed. Because the SSL appears granular, many surface facets must have large tilt angles. A value of $\sigma=0.5$ implies that 95% (99.7%) of the facet normals are $<45^\circ (<56^\circ)$ from the local vertical. No observational data exist to constrain the roughness value, but again (differently from its impact on the radiance) the effect on the irradiance is

small. In any case, while surface roughness affects both the direct and diffuse light components (and therefore the surface BRDF as shown in Lamare et al. (2022)), its effect on albedo and transmittance is small, as demonstrated in Fig. A5.”

For more information on surface roughness, please refer to:

Jin et al., 2006: An analytical solution of radiative transfer in the coupled atmosphere-ocean system with a rough surface. *Applied Optics*, 45, 7443-7455.

99 *Is absorption coefficient based on volume fraction? More details should be provided.*

Yes, it is based on the volume fractions of ice and brine. To clarify, we have added: “The total ice layer absorption is the average of ice and brine water weighted by volume fractions.”.

126 *This explanation mixes a few concepts. The scattering efficiency Q (ratio of scattering surface area to geometrical surface area) in the Mie regime is close to 2 no matter the phase function. The reduced scattering coefficient or similarity variable $b'=b*(1-g)$ which describes the mixed effect of scattering coefficient and phase function will indeed go down as the phase function represented by the asymmetry parameter g goes up. This concept should be addressed in this explanation. It is not that obvious that the reduced scattering coefficient goes down significantly for big inclusions. One would need to prove that claim quantitatively using the similarity principle (where b could be calculated from cross section area and g from Mie Theory).*

The reviewer is mistaken on this point and we attempt to clarify in what follows. For large particle scattering (like brine pockets in sea ice), more than half of the scattered light is due to diffraction which goes nearly straight forward. Another fraction of the incident light is refracted and transmitted in and near the forward direction. This forward scattering contribution increases with particle size and as the relative refractive index approaches 1. Because brine pockets are large compared to the wavelength and the refractive index difference between brine water and ice is small, more than 90% of the scattered photons are concentrated in a very small cone around the forward direction. In IOP calculation, we modified the Mie code and didn't account for this forward scattered energy in the scattering phase function and accordingly in the scattering efficiency. In radiative transfer calculations, this forward scattering component can be considered as not being scattered, and can be added back to the incident beam after the scattering. Therefore, the scattering coefficient is drastically reduced (by over 90% and so we consider it significant). This treatment goes beyond the simplistic similarity concept the reviewer pointed out. A final remark: COART can also accept the phase function as a direct input. This is useful when accurate radiance calculations are performed, and it is not important for the irradiance computations contained in this study.

131 *Mirabilite crystals precipitates under -8 C (Light 2004).*

The focus here is on the temperature at which precipitated salts start to significantly affect radiative transfer, not the temperature at which salts start to precipitate.

137 *As stated in (1), the approach described in this section is based on a description of columnar (interior ice). Since the processes dictating the bubbles and brine channels size distributions and refractive index are different in drained and surface scattering ice, we cannot consider the treatment described here as complete.*

As addressed in the response to (1), our size distribution assumption is analogous to assuming a constant g in all ice layers adopted in previous studies (L08, L15, and Lamare et al., 2022). Lacking observational data, this assumption is necessary. However, in our approach g changes with wavelength as physically expected. We have adjusted the relative sentence to "This physically-based approach provides a sophisticated and complete treatment of radiative transfer in sea ice. The extension to novel discoveries on, e.g., size distributions for the inclusion can trivially be extended to any size distribution obtained from observational evidence."

159 Cloud optical thickness of 10 is too low for an upper bound. 100 would be recommended.

See figure in the appendix, where it is seen that the results have already "converged" using $\tau=10$, especially for the typical low-sun conditions in the Arctic, so there is no need to use $\tau=100$. For the reviewer's benefit, in the supplemental figure "FigS2_rev2.pdf" we have specifically isolated the differences obtained using $\tau=10$ or $\tau=100$.

201 The finality of L08 and L15 is to determine the IOPs of sea ice. The use of tuning in this case is justified because it is needed to find what the IOPs should be in order to meet measured AOPs. Furthermore, an explanation of the bias which they are rectifying by tuning is also provided. In the case of this study, density is an input and not a value that is being determined. Therefore, it is illogical to modify the input density in order to meet the correct answer. Unless these density tunings are justified with an explanation.

L08 and L15 directly use ice IOPs as an input to calculate the AOPs (albedo and transmittance). We agree that "it is needed to find what the IOPs should be in order to meet measured AOPs", but such adjusted IOPs are unlikely to represent the real IOPs because they are based on a constant particle size assumption. As expressed in the reviewer's major comment (1), the particle size (i.e., the asymmetry factor) should be different in different ice layers. In our approach, the ice IOPs are physically linked to the ice physical properties. As the density changes, the IOPs vary simultaneously across all wavelengths. L08 and L15 directly tune the IOP (scattering) band by band, with a fixed g (which is not logical). As a consequence, it is relatively easier to match the AOPs. One way or another, assumptions are necessary whenever complete measurements are not available. We understand the necessary IOP adjustments in L08 and L15, because it is impossible to derive all the IOPs (extinction, scattering coefficient and phase function) only using irradiance measurements. Even the extinction derived from the measurements may not represent the real IOP extinction, because of measurement limitations (e.g., angular coverage) and ice property inhomogeneity, which could result in different extinction values for upward and downward irradiances (real extinction is direction independent). In summary, we and L08 and L15 all determined some set of IOPs that best fits the measurements. The difference is in directly tuning the IOPs or letting them be driven (as it should be) by the ice physical properties. In other words, L15 retrieve IOPs from measured albedo, whereas we retrieve ice density (because of the scarcity of in situ measurements). To avoid the confusion, we have provided more details on how the density is obtained in the revision:

"Because of the lack of in-situ density measurement, we consider the density adjustable and invert it using the measured spectral albedo. Based on previous observations, we first set the density ranges for the IL and DL as 0.90-0.94 g/cm³ and 0.82-9.925 g/cm³, respectively. Because no density has been reported for the thin top SSL, we simply use either 0.55 or 0.60 g/cm³ for this layer. For a SSL density, we loop the density in DL and IL in step of 0.001 g/cm³ and compare the modeled spectral albedo with the measured albedo in each step. When the averaged difference reaches the threshold of 5% or less,

the iterating stops and densities are considered retrieved. Otherwise, the densities giving the minimum difference are used. This process is similar to the process used to obtain the scattering in L08 and L15 but not band by band.”.

203 Following the similarity principle $b'=b*(1-g)$, in the diffusion regime , it does not truly matter if g is kept constant or not. As long as the reduced scattering coefficient b' is consistent.

It may not matter in the visible spectrum where absorption is small but it does matter in the NIR, especially for transmittance. The error of this approximation depends on absorption and solar zenith angle. This similarity principle goes back to the time before the delta-M, which offers a more rigorous treatment. Is it possible to obtain the real scattering coefficient and phase function from merely the irradiance measurements?

216 Why change DL density and snow depth? These choices needs to be justified.

As hopefully made clear by the many explanations given above, starting from the optimal fit (black line) in Fig. 3, we show and discuss the sensitivity to the key parameters, as

advocated also by the reviewer. In comparing the approach of L15 (who modeled the SSL using properties of a snow layer) to our 3-layer model, we provide examples of the effect of assuming different snow properties.

228 Indicate figure number.

We replaced “in the previous figures” with “in Figs. 1-3”.

236 It needs to be kept in mind that absorption coefficient of suits in air and in ice are not the same.

Understood. It is considered.

250-257 if temperature, salinity, SSL thickness, SSL density are not measured, the system is strongly under-determined. Furthermore, the manuscript needs to clarify if the density of 0.915 g/cm³ and salinity of 3 ppt used for interior ice are measured or guessed.

For melting ice, using a temperature of 0°C at the top is a good approximation. RTMs used in other studies (Briegleb and Light, 2007, Light et al., 2008, 2015) treated this low ice density SSL as a layer of “snow” and with all guessed “snow” properties (grain size and density). We did similarly for this top SSL but treated it as a low density ice layer in the RT calculations. Density and salinity for the interior ice for this case are from observed typical values.

254 Granular ice and surface scattering layer are not equivalent. The word SSL should be kept in this case.

Smith et al. (“Sensitivity of the Arctic Sea Ice Cover to the Summer Surface Scattering Layer”, 2022) define the SSL as a “highly-scattering, coarse-grained ice layer”, a definition that seems to be consistent with the rest of the literature. “Granular layer” was verbatim found in the field notes for the relative measurement. Rather than removing “granular layer”, we prefer to substitute “SSL” with “top layer”, although we do not think this is needed.

268-274 The manuscript needs to clarify if these inputs are assumptions or measurements.

This recurring comment is now addressed by the new paragraph added at the beginning of Sec. 3, and reported in the last paragraph of the response to major comment 2.

275 The systematic bias could also come from guesses on ice physical parameters.

It could be, but the bias exists only in the visible spectrum and is small. If a "guess" (to use the reviewer's words) yields a match in AOPs simultaneously and at all wavelengths, it is likely a very good guess, since our approach avoids the unphysical wavelength-by-wavelength adjustment of the IOPs.

276 What is meant by "other species"? At this point in the manuscript, there has been no mention about any biological specie.

For example, dust aerosol deposition, sediments, CDOM. In any case, the reviewer is correct. We changed the sentence to: "Several species".

279 Please verify the claim that algal pigments are more concentrated at the top layer.

Higher concentrations in the bottom layer are common. In the discussion of the cited reference (Perovich et al., 1998), it is reported that "for both seasons, particle concentrations were high at the snow-ice interface. Algal cells transported by brine wicking in the spring are left behind after drainage occurs. These particles undergo growth, increasing in concentration and resulting in increased absorption of shortwave radiation at the interface during the melting season".

289 The explanation is unclear. The sentence seems to contain 3 ideas : (1) Melt pond occurs when sun irradiance is the largest, (2) Melt Pond water as a lower reflection and higher transmission than ice, (3) these two effects combined impact energy distribution.

Agree. We have reworded this sentence: "Melt ponds occur in late spring and summer when solar irradiance is large. Because water has much lower reflection and higher transmission than ice, melt ponds over the ice could significantly alter the solar energy distribution in the atmosphere-sea ice-ocean system."

304 Is temperature measured or guessed?

The two temperatures are estimated based on straight physics. The top temperature of the ponded ice is set at 0°C because this is the coexisting temperature of water and ice (neglecting salinity in the pond water). The bottom ice temperature of -2°C is based on the freezing temperature of sea water.

311 The manuscript should justify why thicker ice as a three-layer model while thinner ice only has a two-layer model.

This paragraph relates to ponded ice, not to thicker vs thinner ice. It results from our simulations that two layers are generally sufficient to obtain as good agreement as using three layers for ponded ice, so we are not sure what "justification" should be provided. Did the reviewer mean to suggest a sentence like "Given the presence of the pond water above, the vertical resolution of the ice layers is less important than for bare ice"?

312 Missing units after 0.83.

Added "g/cm³".

313 The use of tables to summarize inputs and to clarify how many total layers are used to represent ponds +ice would make the manuscript easier to read.To indicate whether the inputs are measured or assumed could also clarify the context.

This is now addressed by the new text added to the beginning Sec. 3.

325-326 The conclusion that transmittance decreases with increasing pond depth is counter-intuitive, since melt water contained in ponds scatters significantly less than interior sea ice. How would that be explained? Is it because of the high absorption of the melt water? This trend is in opposition with measurements from L15 fig 5b.

Although it might appear counter-intuitive, this is what the data say. The explanation might reside in the density change below the melt pond. When pond water fills the voids in ice, the ice scattering could be reduced significantly and therefore transmission increases. The significantly higher transmission of the ponded ice than the bare ice with similar thickness seems to further validate this hypothesis. For clarity, we have added a sentence at the end of this paragraph: "This probably results from the reduced scattering due that the pond water fills the voids in ice as indicated by the higher retrieved ice density."

The data in L15 Fig 5b covers ponded ice thickness from 50 cm to 150 cm, whereas the ice thicknesses for our selected cases are between 70 cm and 120 cm. If the linear fitting is applied to the same thickness range (70-120cm) in L15 fig 5b, you would get exactly the same as we described here (i.e., transmittance decreases as ice thickness increases).

343 Light_2015 used observation of the albedo to invert SSL IOPs. They used single diameter spheres approximation, as it is used for snow, only to provide an initial guess.

We are not sure of the point the reviewer is trying to raise here. Based on L15 paper, there is no measurement for the SSL and "these surface layers were typically not preserved in the core samples.". To obtain the absorption required for RT, the density must be provided. How was the density obtained in L15 if not by guess? The scattering asymmetry factor of 0.94 is apparently a guess too. Using these two guessed model inputs, the scattering (not measured either) is obtained by matching the measured albedo band by band, as explicitly quoted: "Scattering coefficients are thus assigned to the uppermost layers such that modeled albedos agree with observation, starting with near-infrared wavelengths and working progressively toward shorter wavelengths.". The adjusted scattering is in turn used as RT input to model the albedo. In addition, L08 and L15 treat the SSL as a layer of "snow", but the snow grain size was not and could not (because it is not snow) be measured. While the grain size can be inverted by matching with the albedo, such inverted grain size is different for different spectral bands. Thus, some guessing in the grain size is also required. Please note that we are not criticizing the "guessing" (to put it in the reviewer's words) used in L08 and L16. We understand the necessity of some tuning processes, but we don't understand why the reviewer prefers one guess (or inverting) to another.

348 A complete analysis of the sensitivity of AOPs to salinity was never presented. This notion was only mentioned qualitatively in the text of section 3.2.

That is not entirely correct, as a partial sensitivity test is contained in Fig. 5. In any case, we completely agree that it is useful to add one more specific figure to the Appendix (see attached supplemental figure "FigS3_rev2"), with the following text:

"Figure A3 shows the optical effect of different salinity profiles. The maximum value (10 ppt) is a rare occurrence anywhere in the ice column, but was deemed a good maximum value in order to capture the full range of potential variability. Detectable changes are present up to the NIR, and are significant in the visible. In most situations, it is observed that the model predicts maximum differences in both albedo and transmittance of up to 0.05, in correspondence of their peaks in the visible."

351 There are two ideas mixed in the same sentence. (1) Depending on pond depths and (2) the albedo (transmittance) is significantly lower (higher) than that of bare ice

For clarity, we deleted the second half of this sentence.

352 The claim on the relation between pond depth and transmission is in opposition with what was stated at line 325.

The reviewer is absolutely right, this was a typo. Changed to "...transmittance decreases with pond depth for 352 similar ice thicknesses below".

Figure 2 The scenario using measured density should be specified on the caption or legend. Name of the layers should be specified.

The caption was certainly not optimal. Changed to: "Fig. 2: Effect of different ice density profiles (colored curves, values in g/cm³ for the SSL, DL and IL) on modeled albedo and transmittance. The black lines are the optimal fits to the measurements (gray areas) for the 3 July 2010 bare, FYI ice site (see top row of Fig. 1). "

Figure 3 As a comparison, having a scenario with SSL modeled using the reference model would be useful. Name of the layers should be specified.

We will include the reference line (black line in Fig. 1). Caption changed to: "Figure 3: As in Fig. 2, but for a SSL consisting of spherical snow grains of the indicated thickness and effective radius R_s (in μm).". The reference to the DL and IL is not needed since the caption refers to Fig. 2.

Figure 4 Name of layers should be specified.

Caption changed to: "Figure 4: As in Fig. 2, but considering added contamination from sootlike, BC particulate in the top 10 cm of the ice column." The reference to the DL and IL is not needed since the caption refers to Fig. 2.

Figure 5 The difference between the dotted and full black lines representing measurements should be specified. Name of layers should be specified.

We will change the two experimental lines to solid gray, and modify accordingly the explanation already provided at lines 246-ff, "The gray lines are albedo and transmittance measurements collected during SHEBA with two different spectroradiometers: a Spectron Engineering SE-590 and Analytical Spectral Devices Ice-1."

Figure 6 Same as fig. 5

Same as above.

Reference

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<https://doi.org/10.1029/2003JC002164>, 2004.**

Please also note the supplement to this comment:

<https://tc.copernicus.org/preprints/tc-2022-106/tc-2022-106-AC2-supplement.zip>