Reply on RC1

Zhonghai Jin et al.

We thank the reviewer for the comments. We address them one by one below.

General comments:

The authors have extended a coupled ocean-atmosphere radiative transfer (COART) model to the radiative transfer model (RTM) in the atmosphere-sea ice-ocean system (hereafter, the extended COART model). They compared the simulation results with measurement data of spectral albedo and transmittance collected at SHEBA and ICESCAPE stations to validate the extended COART model, and showed the agreement with previous studies that the model representations were improved by considering the vertical structure of sea ice such as SSL, DL and IL. They also showed the effect of contamination (LAPs and ice algae) of spectral albedo and transmittance of sea ice. The effectiveness of the extended COART model was emphasized through the series of analysis.

The result of albedo and transmittance comparisons between measurement and model looks good, but there are some questions on the results. In particular, following major comment (1) is critical issue. This paper's main purpose is to validate the extended COART model. However, because there are few in-situ measurement data required for the validation of RTM, most of the simulated results are based on guesswork. Therefore, this paper is a qualitative discussion and is insufficient to validate the accuracy of the extended COART model. The authors should reconsider how to validate the RTM, so that a major revision would be needed.

This study validates a physically-consistent parameterization of bare sea ice IOPs as a function of its physical properties that can be directly measured (temperature, salinity, density and thickness). 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. Therefore, we focused on bare and ponded ice cases (as defined observationally), for which we could source the most critical input parameters. In contrast, many alternative sea-ice RTMs employ as input parameters directly the IOPs (extinction coefficient, single scattering albedo and asymmetry factor), which are very challenging to measure in-situ or even in the laboratory. For example, the RTM used by Lamare et al. (2022) (referred to in a reviewer's comment) uses a constant, wavelength-independent scattering asymmetry factor of 0.98 and constant scattering coefficients. Similarly, L08

and L15 use a constant scattering asymmetry factor of 0.94 and tuned scattering coefficients. Using the ice IOPs directly bypasses the complex relationships between the sea ice physical structure and its optical behavior, while in our case each physical parameter affects the IOPs at all wavelengths as it should be. Therefore, 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 were adjusted. For these reasons, we have full confidence that our approach is legitimate to validate radiative transfer processes in sea ice. Further details are provided in the following.

Major comments:

- *Most of snow and sea ice physical parameters (sea ice density, ice temperature, salinity, ice thickness Chl. a concentration, snow grain size, LAPs, snow density, snow depth), which are input parameters used for the radiative transfer calculations, are not based on in-situ measurement data, but on the guess due to the lack of the information about snow and sea ice. Although the result of albedo and transmittance comparisons between measurement and model looks good, it is no exaggeration to say that the input variables are adjusted to match the calculation results with the observation ones. In general, since we simulate spectral albedo and transmittance based on the measurement data, we can validate a proposed model and can also find the physical processes that cannot be considered yet. In order to achieve the purpose of this paper, sufficient data must be prepared. The authors need to review the data used for the validation work again. If it is difficult to prepare the data, an alternative method is to confirm the reproducibility of your model by comparing it with a well-validated model.*

We recognize a fundamental misunderstanding regarding this point. The reviewer puts forth a complete list of input parameters as if they all had the same relevance in the study. We are not focusing on the modeling of the albedo and transmittance of snow-covered ice. 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. The ICESCAPE and SHEBA data proved to be the most suitable for our study. 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. On the other hand, it was demonstrated in L08 that SHEBA and ICESCAPE data, albeit collected 20 years apart, show remarkable consistency. Furthermore, the appendix completes the study by highlighting the insensitivity to small variations in a lot of these parameters (especially temperature). The properties of the SSL have instead to be estimated (because no reliable measurement sets exist) 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)."

The misunderstanding regarding the snow layer concerns the treatment of the SSL. We think the reviewer was misled to believe that we were required to use in-situ measurements of snow properties. Instead, we correctly consider the ice SSL as a low-density ice layer, and the thin snow layer used to produce Fig. 3 and relative discussion is merely used to mimic the method used by L08, in which a snow layer with assumed physical and optical properties is used to represent the ice. We thought it was very informative to include this comparison with this flawed modeling of the SSL as snow, which inevitably leads to assuming meaningless "snow" properties (grain size, density, depth). These comparisons reinforce the advantages of considering a physically consistent model versus an ad-hoc approach that directly acts on the scattering coefficients and makes unjustified assumptions.

Next, we found that the simulated albedo and transmittance of bare ice are often higher than observation in the visible spectrum in most of the cases. We then tested the effects of possible LAPs: in these sensitivity tests, the Chl-a concentration and particulate amount are based on climatologies that are referred to in the manuscript. These are merely sensitivity tests aimed at illustrating how the inclusion of these contaminants could further improve the model-observation agreement.

We appreciate the reviewer's suggestion of "comparing the code with a well-validated model", but all available RT models for sea ice are approximated to some level. According to the reviewer's standard on validation, we argue that such a model does not yet exist. We think the most intensively validated model is the 4-stream code used by Dr. Light's group at the University of Washington. As detailed in the paper, this model was developed by Grenfell (1991), used in L08 and several other studies, and used the same discrete-ordinate method as the extended COART model, but in a simplified 4-stream version. The extended COART model is more advanced and can adopt any number of streams, in addition to the coupling feature and the treatment of ice roughness.

Finally, we note that the core point of the reviewer is contradicting: "the input variables are adjusted to match the calculation results with the observation ones". Isn't this the definition of a fit? Also, the point raised in the following sentence is obscure: "since we simulate spectral albedo and transmittance based on the measurement data, we can validate a proposed model and can also find the physical processes that cannot be considered yet".

For all the reasons listed above, and since we strive to provide the best possible study, we kindly ask the reviewer to elaborate further on the raised points if he/she thinks we haven't been clear enough.

- ***Regarding the extended COART model, (a) why is it necessary to add the surface roughness scheme in the sea ice surface? Please add the reason by referring to Lamare et al. [2022; TCD]. In addition, the surface roughness is related to the specular reflection, and the magnitude of the surface roughness differs depending on the value of σ in the Gaussian normal distribution. The authors need to explain how the value of σ was determined. Furthermore, did authors apply this scheme to the boundary between the atmosphere and the melt pond where there is a large difference in refractive index between two medias? Please describe the explanation in detail. (b) There are various sizes of melt pond in the horizontal scale. The authors should describe the applicability of the extended COART model which is a plane-parallel RTM.***

The reason to implement a scheme accounting for the ice surface roughness is that the ice surface is naturally rough, especially for melting MYI, and the surface roughness affects the ice albedo and transmission. 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 the figure attached as a supplement.

Lacking surface roughness measurement, we simply used σ=0.5 and σ=0.1 for the ice and pond surface, respectively. Based on the Cox-Munk formulation, σ=0.1 represents a minimal ocean surface roughness (wind speed=1.4 m/s). For the ice cases in this study, a coarse-grained ice surface layer was observed. Because the SSL appears granular, many surface facets must have large tilt angles. A value of σ=0.5 implies that 95% of the facet normals are <45o and 99.7% of them are within 56o from the local vertical. No observational data exist to constrain the roughness value, but again (differently from the radiance) its effect on fluxes is small. What is more important is that the extended COART model offers full flexibility in the treatment of roughness.

The applicability of a 1-D plane-parallel RTM depends on the relative dimension of the horizontal and the vertical scales. In the presence of a melt pond, the radiation field in the atmosphere calculated by a 1-D RTM is of course not valid. However, the atmosphere in our case only provides an approximate direct and diffuse partition for the incident solar spectral irradiance on sea ice surface. Since we model the RT in pond water and the ice beneath against "point" measurements provided by spectroradiometers, the use of a 1-D RTM to calculate radiative fluxes in the ice is totally justified as long as the pond horizontal extent is significantly larger than the ice thickness.

Technical comments:

L31-33: "The interaction between … surface temperature" this sentence is not clear. Explain the details about climate models mentioned in the text and cite references.

The reviewer is right, this sentence was left too "lonely". We have replaced it with:

"Many sea ice models employ simplistic albedo parameterizations for the albedo, resulting in large uncertainties in both present-day simulations climate and future climate projections in the Arctic (Notz et al., 2016; Koenigk et al., 2014)"

L48-: The last two paragraphs were well documented, but they do not mention the specific focus and the motivation for this manuscript. The authors need to describe it more clearly.

We agree. We have modified and expanded lines 74-ff to:

"Many sea ice radiative transfer models require the Inherent Optical Properties (IOPs) at input, which constitutes a major limitation since the IOPs are very challenging to measure in-situ and even in laboratory. As a consequence, the IOPs more or less always suffer from very significant approximations. For example, Briegleb and Light (2007), L08 and L15 use a constant, spectrally-flat scattering asymmetry factor of 0.94 and tuned scattering coefficients, while Lamare et al. (2022) use a scattering asymmetry factor of 0.98 and constant scattering coefficients.

As described in the next section, Jin et al. (2006) developed a Coupled Ocean-Atmospheric Radiative Transfer (COART) model with high spectral resolution (up to 0.1 cm-1) to finely resolve atmospheric absorption, and accurate treatment of surface roughness. Here, we extend this previously validated (Jin et al., 2002; 2005) model to include the sea ice medium, in order to rigorously calculate the radiative distribution in the atmosphere-sea ice-ocean system. As part of the extension, the sea ice optical properties are directly parameterized as a function of its measurable physical properties (i.e., temperature, salinity, and density), so as to eliminate the need to provide the IOPs (extinction, single scattering albedo, and asymmetry factor) at input. This physically-based strategy also enables a direct connection with the physical ice properties simulated in climate models. In developing such a GCM-oriented version of COART, the objective of this study is to validate said parametrization against observations of albedo and transmittance by constraining the physical properties with available measurements of their vertical profiles."

There is no mention about the ice algae in the section 2 which is a crucial for the transmittance of the sea ice though authors mentioned it in Fig. 6. Provide more details about the treatment of absorption/scattering properties of the ice algae.

We totally agree that the text around Line 100:

"The presence of other possible inclusions (BC and phytoplankton) is also considered."

Is insufficient. We have modified it to read:

"Beside brine pockets and air bubbles, the model can easily consider the presence of any other inclusion with a vertical distribution throughout the ice column. We have currently included a black-carbon-type of aerosol (Hess et al., 1998) and ice algal pigments (Arrigo, *Pers. Comm.*)."

L143: What does "AOPs" stand for?

Apparent Optical Properties (as opposed to Inherent Optical Properties). Thanks for catching the lack of the spell out (now added)!

The comments in the section 3 and below are omitted because they overlap with the major comment 1.

Please also note the supplement to this comment: