The Cryosphere Discussions

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Reply on RC3

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-AC3, 2022

We thank the reviewer for his detailed perusal of the manuscript. We have worked intensively to address each point, as explained in the point-to-point response below.

Major Comments

- **A validation of the radiative transfer in the ice is suggested in this paper nevertheless, by the adjustment of the physical parameter (ice density, temperature profile, BC amount ...) used as model inputs to match the observed optical parameters as well as the too-small data set, it does not seem possible to conclude on the robustness of the model presented here. A solution would be to change the focus of the paper to better highlight the sensitivity analyses that are led in the manuscript.**

There is some misunderstanding here. Except for the density, which is inverted from the spectral measurements, other input parameters are from in-situ observations, including temperature, salinity and ice thickness. The BC and chlorophyll concentration, as well as the snow properties, are not from in-situ measurements, because they are only used for sensitivity tests. A new subsection (3.1) has been added for clarity (see response to point 2 below).

This exercise aims at assessing the capabilities of COART through comparison with experimental data. Additionally, we have provided relevant sensitivity tests in the Appendix, which now contains two more figures (see also response to Rev. #2). The model accurately accounts for all relevant radiative transfer processes in the atmosphere, ice and ocean. The sensitivity tests highlight the flexibility of COART in representing variations in the observed signals. Sensitivity tests do not require experimental data. On the other hand, every validation effort includes a “sensitivity tests'' component. Since we sourced all possible in-situ measurements to constrain the input parameters (ice density, temperature, salinity profiles, and thickness), restraining the focus of the manuscript to “sensitivity tests” would be deceiving.

2. Methodological information is hard to find in the paper due to the lack of a proper methodology section. Some information needed for a good understanding of the manuscript is missing or is diluted in the “validation study” section :

- **A description of the in-situ observations used in this manuscript should be**

added and gathered. Information such as the region of both campaigns, the number of observations for each section (FYI, MYI and Ponded ice), the physical parameters that have been measured, and how each parameter (optical and physical) has been recorded should be gathered in a same paragraph or section.

- **A paragraph or section about the evaluation protocol is also missing. How are the simulated transmittance and albedo calculated based on the IOP retrieved in the look-up tables? What are the inputs of the model? Among these inputs which ones come from observations, and which ones have been adjusted?**

A detailed description of the in-situ observations and the region is provided in L15 (and Polashenski et al., 2015) for ICESCAPE and in Perovich et al., 2002, for SHEBA observations, as referenced at lines 55-56 of the original manuscript. The papers we referred to also detailed "**how each parameter (optical and physical) has been recorded".** A new subsection (3.1) now summarizes general information on the region and the observations used in the simulations:

"3.1 Data and Methodology

Because the ice IOPs are linked to the ice physical properties through the parametrization described in Sec. 2, the input parameters required by the radiative transfer model become simply the ice salinity, density and temperature in the ice layers. Together with the physical properties in atmospheric and ocean layers, COART derives the IOPs in all layers from the input physical properties in the coupled system and then calculates the irradiances at any desired level. The irradiances at the ice surface and base are used to calculate the albedo and transmittance of sea ice for comparison with observations.

The ICESCAPE and SHEBA campaigns proved to be the most suitable data sources for our study. Both campaigns were conducted in the Arctic Ocean, between the Chukchi Sea and the Beaufort Sea regions. Of all the physical variables needed at the 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 (Timco and Frederking, 1995), which also include those found in L08: 0.90-0.94 g/cm3 for the IL and 0.82-9.925 g/cm3 for the DL. Because no density has been reported for the thin top SSL, we use either 0.55 or 0.60 g/cm3 for this layer. For each SSL density, we looped the DL and IL densities in steps of 0.001 g/cm3 and compared the modeled spectral albedo with the measured albedo in each step. When the mean square difference falls below 0.02 (~5%), the densities are considered retrieved. Otherwise, the densities (within the given ranges) giving the minimum difference are used. This process is similar to the method used to obtain the scattering in L08 and L15 but not band by band. Temperature profiles are also sporadic, so we choose melting or ponded ice cases for which the temperature can be estimated based on straight physics. For example, for ponded ice cases, the top temperature can be set at 0°C because this is the coexisting temperature of water and ice. The bottom ice temperature of -2°C is based on the freezing temperature of seawater, and then the temperature in any depth of the interior ice is obtained by linear interpolation. Note that the sensitivity to temperature in this narrow range (0°C to -2°C) is small and does not substantially affect the quality of the fit, as shown in the Appendix. The total ice thickness and solar zenith angle (which are generally measured) are also required to calculate the albedo and transmittance. The melt pond depth required for ponded ice is generally available from observation. Snow properties, chlorophyll concentration, and black carbon, are used for sensitivity tests and to demonstrate that the fit can be improved should information on these constituents be available."

Note that more detailed information on each case was already provided in the subsections: for first-year bare ice, see lines 185-191 in the old Sec. 3.1.1 (now 3.2.1); for multi-year bare ice see Sec. 3.1.2 (now 3.2.2); for ponded ice see Sec. 3.2 (now 3.3). Moreover, we have added a new paragraph and a figure in the Appendix to describe the treatment of ice surface roughness, and edited the sentence at Line 188 to read: "Our tests show negligible sensitivity of the AOPs to small variations in temperature within the chosen ranges".

Specific Comments

P1 lines 31-35: "*The interactions between snow, sea ice and solar radiation in most climate models are based on empirical parameterizations that are often just a function of snow depth, sea ice thickness and surface temperature.*" References to these models should appear here.

This sentence was already adjusted in response to a comment from Rev. #1:

"Many sea ice models employ simplistic parameterizations for the albedo, resulting in large uncertainties in both present-day simulations and future climate projections in the Arctic (Solomon et al., 2021; Notz et al., 2016; Koenigk et al., 2014; Solomon et al., 2007)."

Now we added two more references:

Solomon, S., D. Qin, M. Manning,M. Marquis, K. Averyt,M.M. B. Tignor, H. L. Miller Jr., and Z. Chen, Eds., 2007: Climate Change 2007: The Physical Science Basis. Cambridge University Press, 996 pp.

Keen, A., Blockley, E., Bailey, D. A., Boldingh Debernard, J., Bushuk, M., Delhaye, S., Docquier, D., Feltham, D., Massonnet, F., O'Farrell, S., Ponsoni, L., Rodriguez, J. M., Schroeder, D., Swart, N., Toyoda, T., Tsujino, H., Vancoppenolle, M., and Wyser, K.: An inter-comparison of the mass budget of the Arctic sea ice in CMIP6 models, The Cryosphere, 15, 951–982, https://doi.org/10.5194/tc-15-951-2021, 2021.

P2 line 59: "to complement the observations from SHEBA and ICESCAPE". Previously in the paragraph, the FIRE ACE project is mentioned. Why is it not mentioned anymore here? and why is it not used in the present analysis?

Because FIRE-ACE provided measurements of (only) albedo and BRDF from airborne instrumentation, which is not the kind of measurements we focus on in this study.

P2 lines 72-74: This part of the introduction could benefit from a better description of the focus of the present study.

We have edited the entire paragraph as:

"Meeting the modeling needs described above requires a tool capable of rigorously calculating the radiative distribution in the atmosphere-sea ice-ocean system. As described in the next section, Jin et al. (2006) developed a Coupled Ocean-Atmospheric Radiative Transfer (COART) model. Here, this previously validated (Jin et al., 2002; 2005) COART model is extended to include the sea ice medium. The sea ice optical properties are directly parameterized as a function of its measurable physical properties, so as to eliminate the need to provide at input the Inherent Optical Properties (IOPs), whose direct measurements are more challenging. The rest of the ocean/atmospheric column can accommodate any species whose IOPs are known. 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. The augmented COART model is described in Sec. 2, and its performance is evaluated against ICESCAPE and SHEBA measurements of spectral albedo and transmittance in Sec. 3, including sensitivity studies with respect to light-absorbing impurities. The ice types vary between bare and ponded sea ice in the melting season. The presence of snow is not a focus of the present study, although it can be accounted for by the model and we included a comparison with a relevant study that used snow grains to model the surface scattering layer (old Sec. 3.1.1, now 3.2.1). The conclusions are presented in Sec. 4. An appendix is also provided to show relevant sensitivity tests."

P3 lines 100-101: "*The presence of other possible inclusions (BC and phytoplankton) is also considered.*" The part of the model that considers these inclusions should be described.

There's nothing really to "describe" here, these values are part of the tabulated constants used by the code. We have anyway modified the sentence to read: "In addition to the absorption by pure ice, and scattering and absorption by brine pockets and air bubbles, the presence of other possible inclusions (BC and phytoplankton) can also be considered. The addition of scattering and absorbing particulates is trivial, and achieved via the compilation of tabulated IOPs".

P3 line 126: What do the authors mean by "*the actual scattering coefficient*"?

It refers to the scattering coefficient associated with all scattering energy without scaling resulting from the forward scattering truncation. To avoid confusion, we deleted "actual".

P3 lines 132-133: "*the cases studied here pertain to sea ice surveyed in the warm, summer season*". This should be specified much earlier in the paper (maybe in the introduction section with the focus of the study). The fact that snow is not considered in the model should also be specified earlier.

Good suggestion! We have implemented it, see response to your comment above (P2 lines 72-74).

P4 lines 144-145: Can the authors precise what they mean by "*standard subarctic atmospheric profile*" and by "*open-ocean water properties*" or give references here?

It is simply a static atmospheric model of how the pressure, temperature, density, and viscosity of the Earth's atmosphere change over a wide range of altitudes, similar to the "US standard atmosphere" but for subarctic environments. The reference is added (McClatchey et al., 1972). Similarly, the "open-ocean properties" refer to the "standard" ocean model, in which the ocean optical properties are associated with the chlorophyll content. Based on climatological data, the chlorophyll concentration is around 0.1 g/cm^3 on average, a typical value for open ocean. We have modified the paragraph to read: "In the model, we use a standard subarctic atmosphere for vertical profiles of pressure, temperature and density to model the Rayleigh background atmosphere. For the ocean layers beneath the ice, the Chl-a concentration is set to 0.1 mg/m^3, about the average reported for the arctic ocean (Gordon and Morel, 1983; Morel and Maritorena, 2001; Morel and Gentili, 2004)".

- A. McClatchey, R. W. Fenn, J. E. A. Selby, F. E. Volz, J. S. Garing, Rep. AFCRL–72–0497, (Air Force Cambridge Research Laboratories, Bedford, Mass., 1972).

P4 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.*" As said in the major comment, this requires more explanations. What are the inputs of the model here? And what is done in case the observational data does not exist?

See new Subsec. 3.1, as explained in the response to Major Comment 2 above.

P4 lines 152-153: Why only these two dates have been retained from the ICESCAPE campaign? Are these the only bare ice stations led during the ICESCAPE campaign in 2010 and 2011?

It is explained in the paper that we surveyed all the ICESCAPE (and SHEBA) data, and those particular dates were selected because of optimal observational conditions: clear sky (if possible, otherwise diffuse illumination with SZA=48° which ensures minimal sensitivity), and most consistent set of measurements over best-defined bare sea ice, as per the field notes. We have modified anyway the start of Sec. 3.1.1 (now 3.2.1) to read:

"The gray areas in Fig. 1 show the total range of a series of albedo and transmittance measurements collected at each of two ICESCAPE stations in the Beaufort Sea: the top panels are for the 3 July, 2010, and the bottom panels for the 19 July, 2011, case. These particular dates were selected because of optimal observational conditions and the most consistent set of measurements over best-defined bare sea ice, as per the field notes."

P4 lines 163-165: "*The strong spectral dependence of the absorption coefficients for brine, ice, water and organic or other inclusions (Grenfell and Mayakut, 1977; Perovich and Gow, 1996) is responsible for the nearly constant albedo in the visible region and the significant decrease in the near infrared region*". Are the authors still describing Fig. 1 here? Also, there is a mistake in the reference: Grenfell and Maykut, 1977.

Thanks for catching the typo in the reference (now corrected). The listed statement is general, but the paragraph and what follows refer to Fig. 1 in terms of choosing the density values.

P4-5 lines 180-182: Are these densities measured?

Not in situ. They are typical values for the profile reported in the literature for this type of ice.

P5 lines 189-190: "It is clear that a single layer is insufficient to adequately reproduce both the albedo and transmittance". I assume the authors are referring to the analysis they performed in the appendix. It should be specified.

That's right. Added: "(See Appendix)".

P5 lines 193-194: "*It is clear that a single layer is insufficient to adequately reproduce both the albedo and transmittance.*" Is this result shown in figure 1? If so, it should be specified.

We have modified the paragraph to read: "To highlight the importance of using at least three layers, Fig. 1 includes the results for single- and double-layered ice, with densities taken as the combinations of those used in the 3-layer model. It is clear that a single layer is insufficient to adequately reproduce both the albedo and transmittance, as shown by the blue and magenta lines that are far off the range of measured albedo and transmittance."

P5 line 201: "*We adjusted the ice density*". I am confused here. The authors said earlier that the AOPs are more sensitive to density than to salinity or temperature. This explains why they choose to simplify the temperature profile. But if they now adjust the density (the only parameter that has a real impact on the AOP) of the observations to match better with the optics parameters, how can the radiative transfer model can then be validated with this adjusted "observation"? If the simulated parameters do not match the observation with the measured physical parameters as inputs, this should mean that the radiative transfer model misses something. Changing the physics won't fix the optics.

We apologize for the confusion on the word "adjusted", but density is simply used as one of our input parameters. In situ measurements are used when available. Otherwise, we use typical values obtained from climatologies pertinent to the ice types in question, as described in the new Subsec. 3.1.

We agree that "the AOPs are more sensitive to density than to salinity or temperature", but salinity and temperature also impact the AOPs, as demonstrated in the sensitivity test results. It is the density, salinity and temperature that all together control the phase equilibrium and the brine and air volumes, which in turn determine the ice IOPs. 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. If the parameterization is not physical, 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.

P5 line 210: "*in the absence of completely measured density profiles*". Here is an illustration of the second major comment. Since there is no previous description of what inputs are measured and what inputs are not, it is difficult to understand the results here.

See new Subsec. 3.1, as explained in the response to Major Comment 2 above.

P5 line 211: "*These results demonstrate how the augmented COART model enables a fine tuning of the AOPs.*" I don't understand why the authors referring to the tuning of the AOPs when it seems that only the physics were tuned.

It is clear that most of the confusion comes from the use of the word "tuning". We have rewritten the sentence as: "These results demonstrate how the augmented COART model can capture many of the spectral signatures and their changes in observed albedo and transmittance".

P5 lines 213-214: "*The snow is composed of spherical grains, whose size determines the albedo at absorbing wavelengths (Warren 2019).*" How the albedo and transmittance through the snow are calculated by the model should be better described.

This sentence has been modified as: "The module of the radiative transfer model used to calculate the albedo and transmittance of snow is described in Jin et al. (2008). This model can handle different snow particle habits but, to be consistent with L15, the snow here is assumed to be composed of spherical grains, whose size determines the albedo at absorbing wavelengths (Warren 2019; Wiscombe and Warren, 1980)."

Jin, Z., T.P. Charlock, P. Yang, Y. Xie, W. Miller, Snow optical properties for different particle shapes with application to snow grain size retrieval and MODIS/CERES radiance comparison over Antarctica. Remote Sens. Environ., 112, 3563-3581 (2008).

P5 lines 214-215: "*300 µm to represent new snow, and 1000 µm to represent aged, melting snow.*" Where do these values come from?

These are typical values for newer versus older snow. We have added the reference to the seminal paper: Wiscombe, W. J., & Warren, S. G. (1980). A Model for the Spectral Albedo of Snow. I: Pure Snow, *Journal of Atmospheric Sciences, 37(12)*, 2712-2733.

P6 line 234: "*The SHEBA observations show.*" Why are the authors giving SHEBA's value while it is only ICESCAPE data that are treated in this section?

Because we harvested every possible information on impurity content to inform our input. In the effort to find climatological values, we listed those available. We used these plausible values to factor in the plausible absorption amount in the ice, and also showed a sensitivity study that captures the full range of plausible values. There were no BC measurements reported in ICESCAPE.

P6 line 250: "The salinity profile were assumed". Here again, the manuscript would benefit from a better description of inputs that are measured and those that are assumed or adjusted.

See new Subsec. 3.1, as explained in the response to Major Comment 2 above.

P6 line 268: "*In our modeling, 5 mm of snow with grain size of 200 µm were considered*". Again, how the albedo and transmittance through the snow are calculated by the model should be better described. And where do these values come from?

More description has been provided on "**how the albedo and transmittance through the snow are calculated"** (see response to your "**P5 lines 213-214**" comment). The previous sentence says: ""a few mm of new snow on surface" was reported." These are a few millimeters of a size typical of new snow. If the reviewer has a better guess of what the notes imply, we can regenerate the figure. However, note that we already present sensitivity studies to both snow depth and snow grain size in Fig. 3.

P7 lines 289-290: The sentence should be cut after "*solar irradiance is largest*".

Absolutely. Fixed.

P7 lines 290: "A series of observations". Again the number of observations used should be specified.

See new Subsec. 3.1, as explained in the response to Major Comment 2 above.

P7 line 293: "It is reasonable to expect that the accumulation of water on top of ice should annihilate the SSL." Why? This should be justified with references.

We thought it intuitive to think that water accumulating on top of a fragile, granular thin layer would melt it or at least change its properties dramatically. We have contacted

Melissa Webster, who recently published a new study based on MOSAiC observations ("Spatiotemporal evolution of melt ponds on Arctic sea ice: MOSAiC observations and model results"; Elementa: Science of the Anthropocene (2022) 10 (1): 000072). However, the discussions in her work mostly focus on subnivean ponds. To keep in line with the spirit of our original sentence, we have modified it to:

"The SSL is composed of coarse, crumbly grains of ice and voids of air. Meltwater infiltration into the SSL is expected to significantly alter its physical and optical properties. Typically, a water-saturated SSL is indeed less reflective and more absorptive, or absent altogether (Light et al., 2008)."

Note that Light et al., 2008, report:

"Ponded ice, on the other hand, generally shows a much more homogeneous structure throughout its depth. Although the ice-water interface in the ponds can be quite irregular, there are fewer isolated inclusions and fewer air-ice interfaces to scatter radiation, and the SSL is typically either absent or flooded."

P7 line 311-313: "For the thick ice with shallow pond (top row) observed on 19 July, a 3- layer ice model is required for satisfactory model-observation agreement." What could explain this third layer for this particular pond?

It is likely due to thicker ice requiring more layers to resolve the variations of the properties within the column.

P7 line 318: "(3-layer for thick ice)", how do we know that this is only the ice thickness under the pond that justify the number of optical layers?

Does the reviewer mean "How do we know that IT is only the ice thickness..."? What we mean is that thicker ice requires one more layer (at least). This doesn't preclude that in other cases even more layers could be needed, that's why we specified "in the cases analyzed here" in the previous sentence.

P7 lines 320-321: "If the albedo measurements in the near-infrared are accurate". Why this sentence? Is there anything that suggests the opposite?

We meant to refer to specific challenges in controlling the measurement accuracy in this wavelength regime, where the energy is very low. We propose to change the sentence to "If the albedo measurements in the near-infrared are accurate (in this regime the energy and the subsequent S/N are very low),..."

P8 line 348: "Sensitivity tests show that lower salinity values". The sensitivity tests for the salinity are missing from the main text or appendices.

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 revised manuscript), 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."

The new figure is attached (FigS3_rev2.pdf) as a supplement for the response to reviewer

#2.

P8 line 352-353: "and transmittance increases with pond depth for similar ice thicknesses below" this is the opposite of what is said in line 325.

Yes, this was a typo and is now corrected. Thanks!

P8 line 363-364: "An accurate and efficient radiative transfer model is also required for climate models, which use simple AOP parametrizations for sea ice." This sentence is not true since some ESM already use the Delta Eddington approach of Briegleb & Light (2007) which is not a "simple AOP parameterization for sea ice".

We referred to Briegleb and Light, 2007, in the Introduction. To our knowledge, their ESM is the only one using interactive RT for sea ice. However, it is a 2-stream model with a number of assumptions on ice IOPs (e.g., constant scattering asymmetry factor of 0.94 in all bands and all ice layers).

P9 line 370: "the density is used as a tunable parameter since in situ measurements are not 370 always available". As an input parameter of the model, the density should not be treated as a tunable parameter.

This point should now be resolved in view of the many earlier responses to this review.

Figure 1: Lime text in the legend is hard to read. Also, the authors should consider to better explaining the legend (by naming each layer and explaining what letters refer to).

We have improved the explanation to the legend explicitly in the caption. Regarding the color readability, we'll abide by the requirements of the editorial office should the quality be insufficient.

Figure 2: Albedo and transmittance curves should be differentiated by something (dashed line as it is already done in figure A1). Numbers given in the figure should be explained in the caption.Why are only the results for July 3rd are given and not those for the 19th? Are the results for July 19th similar the those for the 3rd?

We have changed the transmittance curves to dashed, as suggested (it makes total sense for consistency with the figures in the Appendix). The explanation of the legend has been improved as per the previous comment. Yes, the results for July 19th are very similar.

Figure 3: Same comment as figure 2.

Corrected accordingly. See response above.

Figure 4: The caption should better describe the figure here. It is not as Fig.2 as the density profile is not changing. Describing the physics (number of layers, density profile) in the caption and just giving the amount of BC for each line in the legend could help for clarity.

We totally agree. The caption now reads: "Sensitivity to albedo and transmittance to the addition of contamination from sootlike, BC particulate spanning amounts typical of the panarctic. The solid black lines are the optimal spectra for the 3-layer profile in the top panels of Fig. 1 (3 July, 2010).".

Figure 5: Same comment that for the other figures: the legend should be better explained in the caption. What the dotted line refers to should also be specified.

Changed as suggested, as done for Fig. 2 (see your comment above).

Figure 6: What the dotted line refers to should be specified.

Corrected as per the point above.

Figure 7: Considering the number of panels here, adding a letter to call each panel could improve the clarity of the main text and the caption.

Done, and indicated in the caption and text.