

The Cryosphere Discuss., referee comment RC3  
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## Comment on tc-2022-106

Anonymous Referee #3

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Referee 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-RC3>, 2022

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### General Comments

This manuscript aims to validate an implementation of a radiative transfer model in sea ice into the advanced coupled-atmosphere radiative transfer (COART) model. The influence of several environmental factors (number of the ice layer, density profile, presence of black carbon or phytoplankton...) on the simulated albedo and transmittance are studied using in situ optical measurements led in sea ice during the SHEBA and the ICESCAPE field campaigns. The authors show that the simulated albedo and transmittance are closer to the observation when at least three layers (for bare ice) and two layers (for ponded ice) are considered which is in line with previous studies. They also show that the ice density profile has a large influence on the representation of both optical properties.

The analyses suggested in this manuscript confirm results from previous studies and bring new material for a better understanding and representation of the radiative transfer in sea ice. However important elements are missing to (1) validate the model as the scope of the manuscript suggests it and to (2) well understand the methodology used here.

### 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.

- 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 Ponged 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?

## 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.

**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?

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

**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.

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

**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.

**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?

**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?

**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?

**P4 lines 163-165:** *"The strong spectral dependence of the absorption coefficients for brine, ice, water and organic or other inclusions (Grenfell and Maykut, 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.

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

**P5 lines 189-190:** *"Our tests show negligible sensitivity of the AOPs to small variations in temperature."* I assume the authors are referring to the analysis they performed in the appendix. It should be specified.

**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.

**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.

**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.

**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.

**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.

**P5 lines 214-215:** "*300  $\mu\text{m}$  to represent new snow, and 1000  $\mu\text{m}$  to represent aged, melting snow.*" Where do these values come from?

**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?

**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.

**P6 line 268:** "*In our modeling, 5 mm of snow with grain size of 200  $\mu\text{m}$  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?

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

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

**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.

**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?

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

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

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

**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.

**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".

**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.

**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).

**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 given and not those for the 19th? Are the results for July 19th similar to those for the 3rd?

**Figure 3:** Same comment as figure 2.

**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.

**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.

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

**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.