The Cryosphere Discussions

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Comment on tc-2022-106

Anonymous Referee #2

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-RC2, 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 latters, 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.

Major Comments

- 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).

- 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.

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).

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

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

88 The explanation of how the surface is 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.

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

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).

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

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.

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

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.

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.

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

228 Indicate figure number.

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

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.

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

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

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

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

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

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.

304 Is temperature measured or guessed?

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

312 Missing units after 0.83.

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.

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.

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.

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.

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

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

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

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.

Figure 4 Name of layers should be specified.

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

Figure 6 Same as fig. 5

Reference

Arndt, S. and Nicolaus, M.: Seasonal cycle and long-term trend of 75 solar energy fluxes through Arctic sea ice, The Cryosphere, 8, 2219–2233, https://doi.org/10.5194/tc-8-2219-2014, 2014.

Light, B., Maykut, G., and Grenfell, T.: A temperature-dependent, structural-optical model of first-year sea ice, J. Geophys. Res.- Oceans, 109, C06013, https://doi.org/10.1029/2003JC002164, 2004.