The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-76-RC2, 2017

The Cryosphere
Discussions
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Interactive comment on "Optical properties of laboratory grown sea ice doped with light absorbing impurities (black carbon)" *by* Amelia A. Marks et al.

Anonymous Referee #2

Received and published: 15 June 2017

This manuscript describes results from a laboratory experiment and related numerical modeling. The experiment entails the growth of laboratory-simulated sea ice. Four ice blocks were grown, the first with no black carbon (BC) added, the following three each with progressively larger concentrations of BC. The BC was restricted to a 5cm thick surface layer. As each ice block was grown, spectral reflectance was recorded. At the end of each growth, the vertical profile of upwelling radiance within the ice was measured. The manuscript then describes the comparison between observed apparent optical properties and apparent optical properties predicted with the "TUV-snow" radiative transfer model. The overall topic of this manuscript is of interest to the TC readership. The results of the paper are interesting and timely. However, I have a num-

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ber of both major and minor concerns that need to be addressed before this paper can be published.

Major concerns: 1. The paper is not particularly well written. I find numerous instances where the writing is sloppy and imprecise. I will attempt to point them out in my minor comments below, but the manuscript could be dramatically improved if some attention were given to the writing.

2. There seems to be a mismatch between the title and the stated objectives. The title suggests the point of the paper is to present the optical properties of laboratory-grown sea ice containing black carbon. The abstract, however, starts by suggesting the reader should expect a manuscript detailing the validation of a radiative transfer model. P. 1 lines 1-4 are really not clear what this manuscript is setting out to do.

3. I am not entirely comfortable with the nature of the comparisons being made between the observations and the model. There seems to be some circularity here. In the abstract (lines 6 – 7) it is stated that measured apparent optical properties (albedo and extinction) are used to derive inherent optical properties (scattering and absorption cross-sections) "using the model". It is not at all clear what this means. Then lines 10 -12 state that light extinction (e-folding depth) is calculated using the model and the IOPs that were derived directly from AOPs (lines 6 -7)? This sounds rather circularâĂŤ-like saying that the measurements are used to define the inherent optical properties of the domain (using the model), which are then fed back into the model to produce apparent optical properties, for comparison with the measured AOPs. Well, I would hope those would agree! Page 2 line 30 states that it is the third objective of this work to use measured [apparent] optical properties to recreate the irradiance within the sea ice using the TUV-snow radiative transfer model and compare modelled and measured values. To me, this says that the objective is to use the observations to infer IOPs appropriate for input for the model, and to then compare modeled and measured AOPs. I don't think this is a legitimate comparison! The model is being forced to agree with the observations! There is no independent comparison here. The further discussion on p.

12 (lines 2 – 4) reinforces this circularity.

4. Use of upwelling radiance to determine e-folding depth in finite-depth domain with forward peaked scattering phase function? If I understand correctly, the e-folding depth is calculated from the measurement of upwelling radiance. I would expect the measurement of e-folding depth in this relatively thin (30 – 50 cm) ice block to be biased low, but measuring the upwelling radiance makes it only worse. Take the limiting case of an upwelling radiance measurement immediately above the ice-water interface. I would expect it to be near zero, whereas the downwelling radiance would be non-zero. The e-folding depth from those two different measurements should be quite different.

5. Confusion between radiance and irradiance? I thought that the ratio of upwelling nadir radiance to downwelling nadir radiance was being measured, but on page 12 line 13 it sounds like the measurement is ratio of upwelling irradiance to downwelling irradiance. Is there confusion here between "radiance" and "irradiance"? They are radiometrically distinct quantities and should not be interchanged. Additionally, p. 20 line 30 states that the reflectance of the laboratory grown (not 'gown') ice is considerably larger than first year ice, and resembles a reflectance closer to multi-year ice. Does this statement mean that the spectral ratio of upwelling to downwelling radiance (as far as I can tell, the only optical property measured above the ice) is being compared to spectral albedos published in the literature? Here again, I think it is possible there may be some confusion between radiance and irradiance? Or is the model being used to estimate the albedo of the iceâĂŤwhich is then being compared to the albedos of natural ice? What natural ice measurements are being used in this comparison? Also, it would be helpful to present the measured spectral reflectivity of the tank, since it possibly matters so much.

6. Figure 7 shows wavelength-dependent absorption cross-section derived from reflectance and de-folding depth data from the four runs, with no BC. I am concerned about the interpretation of these data. These curves don't really look like chlorophyll absorption spectra to me. Chlorophyll typically has absorption maxima at 430 – 450

nm and 640 - 670 nm. How was the absorption of water and ice represented in the model? Is it possible there was some error in representing the ice and/or the brine, and these spectra, which look similar in nature to the absorption of water?

Minor points: P. 1 L 14: As pointed out by a different reviewer in a short comment, the last line of the abstract states that “albedo is reduced by” as much as 97%. This cannot be accurate!

p. 1 L 22: What is the “TUV-snow radiative transfer model for sea ice”? I am not familiar with it, and I find it rather confusing that it is a “snow” model for “sea ice”. What does TUV stand for?

p.2 L2: Here the authors mention that the sea ice simulator has “not been experimentally validated for sea ice.” If that is an objective of this manuscript then it should appear perhaps in the title, and probably in the abstract. The paper is a bit diffuse because it seems to have many different objectives, as listed at the bottom of p. 2.

p.2 L 14-15: It is not clear why validating the TUV-snow model just for a single type of sea ice, grown under particular circumstances, and a single absorber, in this case BC, necessarily means the model can be used “confidently” for other sea ice types and absorbers. For instance, I can imagine that ice grown with very few scatterers could have much smaller optical depth, and perhaps would be a different modeling problem than the one examined here.

p.3 L 9: “temperature” is “higher/lower”, not “warmer/cooler” Figure 1: I see the tank volume is 2000 liters, but there is no indication of the diameter and depth? They matter, particularly in regards to the exchange of salt between the growing ice sheet and the “ocean”.

p.4 L7: does the pump achieve vertical mixing? Are you only worried about temperature stratification? What about salinity stratification?

p.4 L14: what does “majority of shortwave solar wavelengths” mean? Please clarify.

p. 4 L18: Was the incident light field isotropic? Or just diffuse? It is difficult to create an isotropic light field in the laboratory, but it is also difficult to simulate a diffuse light field that is not isotropic in a model. Just saying they are both diffuse, does not ensure a valid comparison.

p.5 L7: Drilling a hole breaks the horizontal homogeneity of the ice block, could cause additional brine drainage, and does reduce the integrity of the ice, but the authors should be wary of stating that it "destroys the fabric of the ice", as I don't think this is accurate.

p.6 L5: what size is the reflector panel? At some size it will reflect significant radiation back to the "sky" (lighting panels and white boards) and enhance the downwelling radiation field, biasing the reflectance. Please state the size of the panel and discuss the possibility of it affecting the measurement of the incident light.

p.6 L22: what does it mean that the "e-folding depth . . . is asymptotic"? please clarify. Also, it is not accurate to say "there are no near surface effects". The fact that this is a finite domain means there necessarily will be some surface effects.

p.7 L17: "proportion"? I think "portion" is intended?

p.7 L27: why two (very different!) values for the mass absorption cross-section?

Table 1: units for density are not g cmˆ3. Also, I am confused about the cross-section units of cmˆ2 kgˆ-1. Cross-sections on previous page are cited in mˆ2 gˆ-1. Those are not equivalent.

Fig. 2 Y-axes have different labelsâĂŤshould they not both be "Relative spectral absorbance"? I understand the two figures are for different materials, but I think they are intended to be compared, and if that is so then they should have the same label on their y-axes.

p. 9 L 8 and following. This sentence is cryptic. It needs to be rewritten for clarity.

p. 12 L 5: Why is ice density measured and reported? Is it used in the modeling? If so, the way that is used could be important and should be described.

p. 12 L 10 -11: Sentence beginning "The reflectance under..." needs to be rewritten for clarity.

p. 13 L 4- 5: Higher air temperature should result in slower ice growth. Slower ice growth would be expected to result in less entrainment of brine within the ice. Less brine would be expected to yield fewer and/or smaller brine inclusions, which would then result in reduced scattering.

p. 14 L 3 -4: see comments above about reduced salinity The data displayed in figures 5 and 8 really should be presented on the same plot; it is very difficult to make the comparison when they are in different figures.

p. 18 L 13: No, sea ice is not at its eutectic point, unless it is very cold (about -37 C). When in thermal equilibrium, it is always at its melting point, hence the required equilibrium concentrations of brine and ice.

Figure 9: please specify which y-axis corresponds with which curves.

p. 19 L 4 -7 : This relates to a commonly recognized "similarity principle" in radiative transfer.

p. 20 L 15, following: does the exchange of sea water in the "ocean" of the simulator correct for salinity variations? I would expect even a 30 cm thick ice cover could affect the salinity of the ocean, but since the dimensions of the tank are not given (other than total volume), it is impossible to estimate the salinity enhancement in the 'ocean' due to freezing of the ice and resulting salt rejection.

p. 21 L 11: Here again, "...reduce the albedo of the ice by 97%..." I think this should be "...reduce... to 97%".

p. 21 L 15: "The derived scattering cross-section values are typical of sea ice..." what

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are the derived values being compared to?

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