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The Cryosphere
Discussions

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

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The authors would like to thank the referee for their comments on the paper "Optical properties of laboratory grown sea ice doped with light absorbing impurities (black carbon).

Changes made to the paper based on their review are detailed below on a point-by-point basis:

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.

The paper has been reviewed to sharpen-up the writing and improve the manuscript.

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.

The title of the paper has been changed to include radiative-transfer modelling and now reads: *"Optical properties of sea ice doped with black carbon- An experimental and radiative-transfer modelling comparison".*

The abstract has also been completely rewritten:

Radiative-transfer calculations of the light reflectivity and extinction coefficient in laboratory generated sea ice doped with and without black carbon demonstrate that the radiative transfer model TUV-snow can be used to predict the light reflectance and extinction coefficient of sea ice typical of first year sea ice containing typical amounts of black carbon and other light absorbing impurities. The experiments give confidence in the application of the model to predict albedo of other sea ice fabrics.

Sea ices, ~30 cm thick, were generated in the Royal Holloway Sea Ice Simulator (~2000 L tanks) with scattering cross-sections measured between 0.012 and 0.032 $m^2\ kg^{-1}$ for four ices. Sea ices were generated with and without ~5 cm upper layers containing particulate black carbon. Nadir reflectances between 0.60 and 0.78 where measured along with extinction coefficients of 0.1 to 0.03 cm^{-1} (e-folding depths of 10–30 cm) at a wavelength of 500 nm. Values were measured between light

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wavelengths of 350 and 650 nm. The sea ices generated in the Royal Holloway Sea Ice Simulator were found to be representative of natural sea ices.

Particulate black carbon at mass ratios of ~75, ~150 and ~300 ng g^{-1} in a 5 cm ice layer lowers the albedo to 97%, 90%, and 79% of the reflectivity of an undoped "clean" sea ice (at a wavelength of 500 nm).

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 circular- 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!

The abstract has been re-written to be clearer

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 modelled 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. C2 12 (lines 2 – 4) reinforces this circularity.

The objectives have been removed as they caused confusion. Also there is no circularity in the procedure presented here; we are demonstrating we can reproduce our experimental data with modelled data and then perturb the experiment (with black carbon) and continue to reproduce the experimental data with the model. Furthermore using the reviewer's (and Mobley's) nomenclature:

- The AOPs of the sea ice are measured and modelled to determine values of the IOPs

- To determine how well the model AOPs fit the measured AOPs we compare the modelled AOPs with the measured AOPs

- We perturb the experiment with the addition of black carbon and measure AOPs. We than compare modelled and measured AOPs

Thus we demonstrate that:

1) We can reproduce our experimental AOPs (n.b. we use e-folding depth and albedo, most studies only use albedo thus our system is more constrained) by modelling IOPs of pure ice. The TUV-snow radiative-transfer model can be used to describe the radiative-transfer in and above the sea ice

2) We can perturb the experimental system with a known amount of a known absorber and reproduce the experimental results with our model. Note the system is constrained by amount, size and MAC of an absorber and these cannot be used as modelling parameters such as those used in other studies.

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.

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The following text has been added to the manuscript to clarify this: *"At each depth drilled the same fibre optic is inserted into the hole and the light intensity (upwelling radiance) measured via an Ocean Optics spectrometer. In an optically thick sea ice the measurement of either up or downwelling light for e-folding depth is not important as has been shown by France et al. 2012."*

And: *"Measurements used to calculate the e-folding depth are only conducted in the middle of the ice as the irradiance profile changes rapidly at the air-ice and ice-water boundary (a good example shown in King et al. 2005). The calculation of an e-folding depth from the modelled downwelling irradiance was calculated from similar depths as the experimental ice. The modelled ice had the same thickness and underlying tank radiance field as the experiment"*

The authors are well aware of the boundary effects that occur at the air-ice and ice-water interfaces, therefore no measurements were performed near the upper or lower ice boundaries, as we (King, et al 2005) have previously shown the irradiance profile changes rapidly at these edges. The optically "thin" sea ice would not produce an asymptotic value of the e-folding depth in the experiments measured but produce a phenomenological value for this particular experiment. Thus our use of radiative-transfer modelling to reproduce the reflectance and the e-folding depth to determine values of σ^{+}_{abs} and σ_{scatt} which are not phenomenological. It should be remembered that sea ice, like snow, is a diffusing media and that in an optically thick sea ice the measurement of either up or downwelling light for e-folding depth is not important as has been shown by France et al. 2012 for optically thick media. It should also be noted that the limiting case mentioned by the referee for the experimentally measured upwelling radiance in this study would not be zero as the bottom of the tank is reflective and the radiative transfer modelling included the measured reflective bottom. It has been shown in France et al. 2012 that the angle at which measurements of the e-folding depth are made in an optically diffuse media has no effect on the resultant values.

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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 ice which 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.

The comparison to literature albedos have been removed as the measurements of the albedo reported in literature would have been performed under clear skies and therefore the referee is right to point out these are incomparable to the measurements taken of the laboratory grown sea ice under diffuse conditions. The manuscript has been checked that the terms radiance and irradiance have been used correctly throughout.

The spectral nadir reflectance of the tank bottom has been added to the supplementary information and a reference to this added in the text (added here as figure 1).

6. Figure 7 shows wavelength-dependent absorption cross-section derived from reflectance and e-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?

The absorption spectra of chlorophyll from Bricaud et al. (2004) and chlorophyll in ice from (Mundy et al. 2011) have been added to figure 7 (now figure 9) to demonstrate the algae absorption (added here as figure 2). In previous experiments, not reported here, we have observed algae at visible concentrations in the tank.

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!

The wording has been corrected to read "to" instead of "by"

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?

Further explanation of the TUV-snow model has been including here for clarity: *"The following study presents the first data from the RHUL sea ice simulator used to validate the Tropospheric ultraviolet and visible (TUV)-snow radiative-transfer model for use with sea ice. The TUV-snow model is a coupled atmosphere-snow radiative-transfer model, described in detail by Lee-Taylor and Madronich (2002). The model has been used multiple times for investigations of radiative-transfer in snow (e.g. King et al. (2005), France et al. (2011), France et al. (2012), Reay et al. (2012)) and has also been adapted for use with sea ice (e.g. King et al. (2005), Marks and King (2013), Marks and King (2014) and Lamare et al. (2016)). The model has previously been experimentally validated for photochemistry in snow by Phillips and Simpson (2005) but it has not been experimentally validated for sea ice."*

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The reference to Lee-Taylor and Madronich (2002) is a full description of the model. TUV stands for tropospheric ultraviolet and uses the DISORT code of Stamnes et al. (1988). These are standard, freely available tools for radiative-transfer studies.

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.

The title of the paper has now been changed to reflect the main objective of the paper and the list of objectives has been removed from the paper and instead the section restructured to portray the main aims of the paper.

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 modelling problem than the one examined here.

The suggestion the model could be used "confidently" has been toned down, with the text changed from *"If the radiative-transfer modelling with TUV-snow can reproduce the laboratory grown ice with absorbing impurities it will allow the model to be used confidently for other sea ice types and absorbers."* to read *"The work presented here will demonstrate that radiative-transfer modelling with TUV- snow (Lee-Taylor and Madronich, 2002) model can reproduce laboratory grown ices with differing fabrics with a range of mass ratios of light absorbing impurities. Such a validation will give confidence to others in the calculations of TUV-snow for other sea ices and other light absorbing impurities."*

The TUV-snow model has been applied to ablating sea ice (King et al. 2005) and to the sea ice described in Grenfell and Maykut (1977) in Marks et al. (2013) and so has been used for other sea ice types. To the authors knowledge very few other radiative-transfer models of sea ice have been validated in a laboratory experiment, for doping with light absorbing impurities and whilst such experiments cannot cover all scenarios it gives more confidence than in an unvalidated model. If all scenarios could be validated by experiments then a model would not be required. It is the author's assertion that a model that has been successfully validated with realistic laboratory experiments is more useful than a model that has not.

p.3 L 9: "temperature" is "higher/lower", not "warmer/cooler"

The sentence referring to "warmer Polar temperatures" has been rewritten based on different comments from the other referee. The text now reads *"The sea ice simulator is designed to replicate a Polar sea ice growth environment under UV and visible wavelengths of solar radiation."*

Figure 1: I see the tank volume is 2000 litres, 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".

The diameter and depth of the tank were originally stated in section 2.1. For clarity they have also been added to the caption of figure 1.

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

Yes, the pump achieves vertical mixing and therefore both temperature and salinity will not be stratified, this was the purpose of the pump. The text has now been altered to read: *"To create circulation within the tank, ensuring temperature and salinity stratification does not occur, an Iwaki. . .."*

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

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For clarity this sentence has been restructured to read *"Illumination replicating short-wave solar wavelengths over 350–650 nm is provided with a set of . . ."*

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.

The following text has been added to the manuscript *"The radiance, as a function of azimuth and zenith angle within the experiment was checked with a fibre optic probe and a broadband visible wavelength measurement and found to vary by 5–10%".*

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.

The text has been revised from *"destroys the fabric of the ice"* to *"destroys the homogeneity of the ice"*

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.

The following text has been added to the paper: *"During a reflectivity measurement a 30 cm $\times$ 30 cm Spectralon panel is added to the diffuse lighting environment above the sea ice. The addition of this panel increases the radiance, L, within the diffuse lighting environment. A very conservative estimate of the effect on the measurement of the reflectivity can be calculated by analogy to an integrating sphere. The Spectralon panel represents 0.66% of the area of the diffuse lighting environment, which is approximately* *a cube made up of white panels and sea ice (i.e. 6 $\times$ 1.5 m $\times$ 1.5 m = 13.5 m^2). Treating the diffuse lighting environment above the sea ice as a crude integrating sphere and considering fractional change in radiance, $\frac{\delta L}{L}$, after Ball et al 2013 who suggest $\frac{\delta L}{L} \approx \frac{A_{panel}}{A_{environment}\rho}$. Where A_{panel} is the area of the Spectralon panel, $A_{environment}$ is the area of the diffusing "cube" and ρ is the overall reflectivity of the diffusing cube. A very crude analysis assumes reflectivity of the panel is 1 and the part fraction of the hypothetical integrating sphere is 0. In the limit of a reflective environment $\frac{\delta L}{L} \rightarrow \frac{A_{panel}}{A_{environment}\rho} \sim 0.67\%$. Thus the overestimation of the radiance (∼0.67%) is significantly less than the uncertainty displayed on the measurement of nadir reflectivity displayed in figure 3 and figure 7."*

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.

The following text has been revised for clarity *"The extinction coefficient and e-folding depth measured in the work presented here is asymptotic (reaches a constant value as shown in King et al. (2005))"*

The asymptotic region is the region where the logarithm of the radiance decreases linearly with depth and the value of the e-folding depth reaches an asymptote or constant value as shown in King et al. (2005), figure 10.

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

"Proportion" has been changed to "portion"

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

These values for the mass-absorption cross-section were obtained using different comparison materials; acetate and polypropylene sheets. The 0.58 m^2 g^{-1} value (obtained from using the acetate sheets) has been removed from the text and was an erroneous value left in from an earlier edit for a failed determination. The acetate sheets were unsuitable for these type of measurements.

Table 1: units for density are not g cm^3. Also, I am confused about the cross-section units of cm^2 kg^{-1}. Cross-sections on previous page are cited in m^2 g^{-1}. Those are not equivalent.

Units for density have been changed from g cm^3 to g cm^{-3}. We are keeping nomenclature of Lee-Taylor and Madronich (2002) and the body of our work (e.g. King et al. (2005), France et al. (2011), France et al. (2012), Reay et al. (2012), Marks and King (2013) and Marks and King (2014) in similar units. The units of m2 kg-1 are used for scattering cross-sections, while cm^2 g^{-1} are used for absorption cross-sections, the authors are aware these units are not equivalent.

Fig. 2 Y-axes have different labels should 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.

The y-axis on both of these figures has been changed to "Relative spectral absorbance"

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

The sentence has been changed to read *"Grenfell et al. (2011) showed that for small amounts of black carbon the mass loading is directly proportional to the absorbance measured by the integrating sandwich spectrometer."*

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.

When describing sea ice it is normal to record its mass density (Eicken, 2003) as it can be used to calculate other properties of the sea ice as described by Weeks (2010)

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

rewritten for clarity.

Agreed, this sentence was poorly written. The sentence has been changed from *"The reflectance under the ice is the measured, wavelength dependent, nadir reflectance of the bottom of the water filled tank"* to *"The wavelength dependant, nadir reflectance of the water filled tank is measured and included in the model as the under ice reflectance."*

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.

The authors agree with the referee's logic that a larger value for the temperature of the air would result in slower ice growth, however as stated in the paper the air temperature is kept constant for all experiments.

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.

Figures 5 and 8 should appear on the same page of the final manuscript for easy comparison of the figures. The figures were kept separate for clarity as the figure becomes cluttered to read when plotted on one graph. Any adjustment can be made at the proof stage.

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.

The reference to sea ice always being at its eutectic point has been removed from the text.

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

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The figure has been changed to make it more obvious which axis corresponds to which curve (attached as figure 3 here).

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

Reference to the "similarity principle" has been added to the text.

p. 20 L 15, following: does the exchange of seawater 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.

The sea ice thickness is kept thin so that the salinity of the water below the sea ice does not get too large to be unrealistic. The salinity of the underlying liquid is measured during ice growth and does increase slightly due to brine expulsion into the water.

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

Correct, the text in the manuscript has been changed from "by" to "to".

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

The following has been added to the manuscript for comparison *"The derived values of the scattering cross-section are typical of sea ice (e.g. Grenfell and Maykut 1977, Timco and Frederking, 1996, Perovich 1996), while the derived. . ."*

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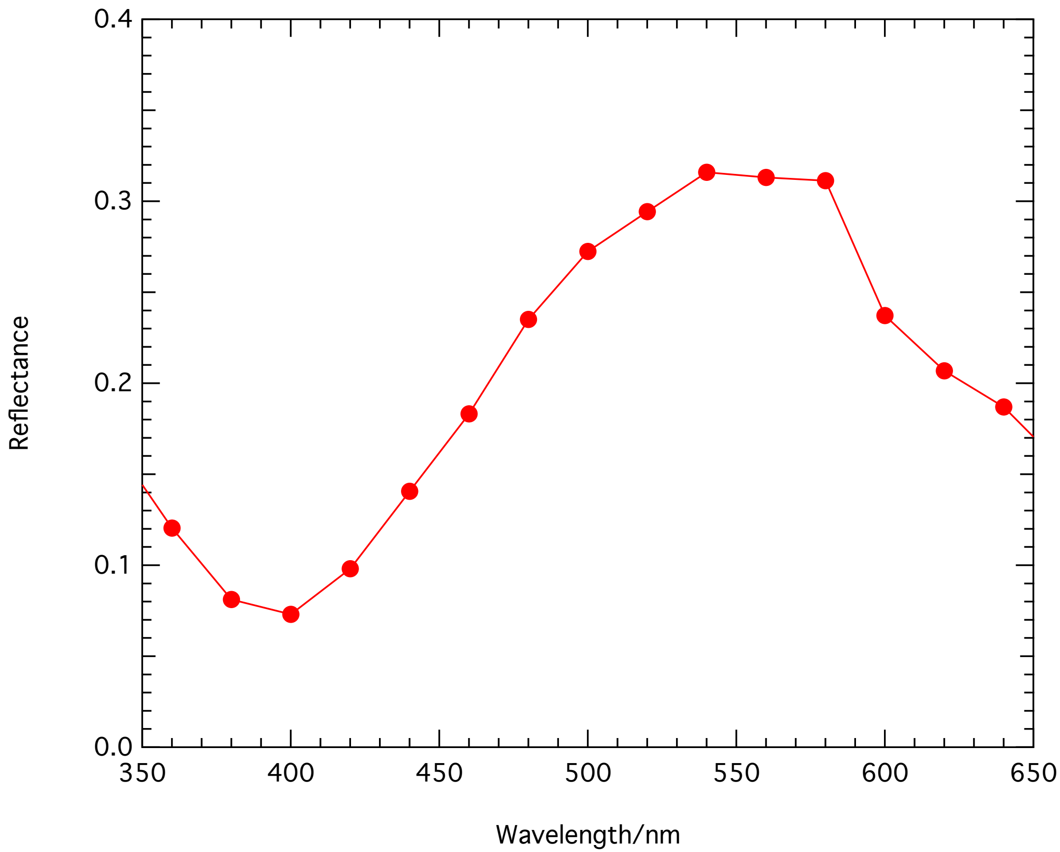

Fig. 1. Spectral nadir reflectance of tank bottom added to the supplementary information

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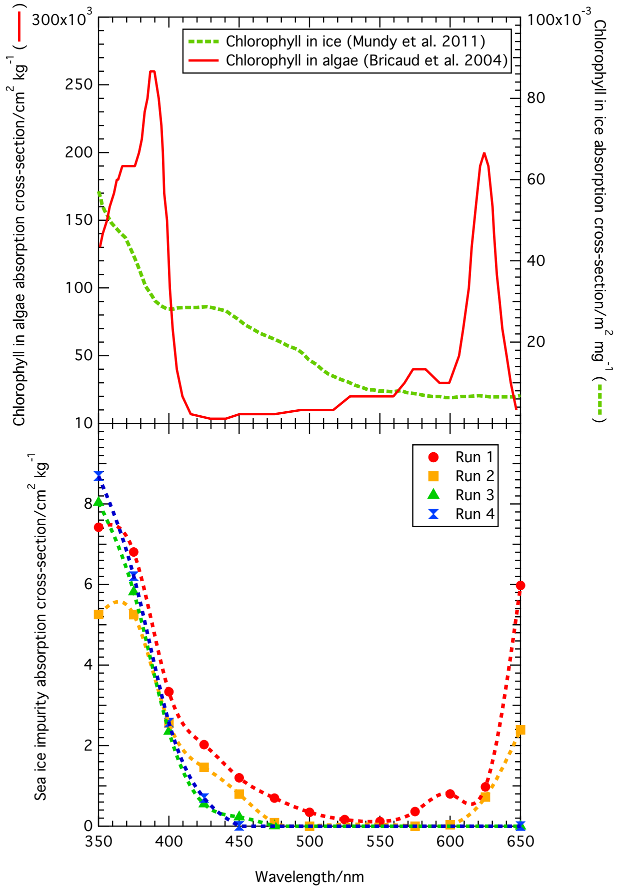

Fig. 2. Updated figure for absorption cross-section of ice with absorbing impurities (bottom) showing absorption by chlorophyll (bottom)

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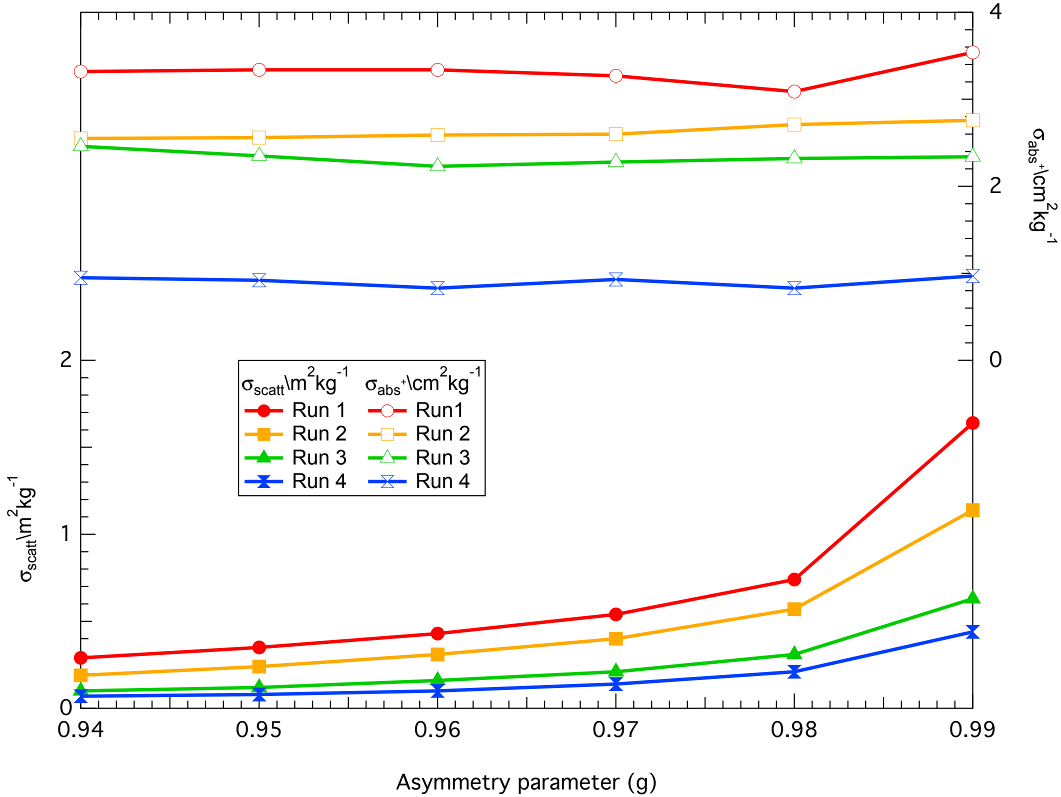

Fig. 3. Updated figure 9 with clearer axis labels

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