The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-36-AC1, 2017

Interactive comment on "Modelling radiative transfer through ponded first-year Arctic sea ice with a plane parallel model" *by* Torbjørn Taskjelle et al.

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Received and published: 6 June 2017

This manuscript describes observations and modeling of the spectral transmittance of solar radiation penetrating through melting first-year ponded Arctic sea ice. The measurements were carried out over three unique transect lines in the proximity of a single ice station during the month of July. The observations give unprecedented detail about the transmittance of light through a ponded ice cover. The measurements were accomplished with the help of a diver, and as a result of this logistical aid, make up a very valuable data set. The corresponding modeling was done with diligence and care and appears to accurately simulate the observations. The text is clear, concise, and very readable. The figures are generally appropriate and relevant, although almost all of them seem to be missing axis and

legend labels. In general, I find this manuscript a useful contribution to the literature. However, I think substantial revisions are necessary prior to publications.

Thank you for the positive initial remarks!

Major points

A note on the figures first. Naturally, axis labels and legend text was included in our figures, and they were present in the PDF that was submitted to The Cryosphere (TC). As for why they failed to appear in the discussion paper, I cannot say for sure, but presumably it has to with how fonts are embedded in the figure PDFs. TC takes the submitted PDF and inserts it somehow in a new document containing the page header, and in this process the fonts have apparently disappeared. I should perhaps have noticed myself, but at the same time I do wish you had posted a short comment on the discussion page as soon as you noticed this, and a working PDF could have been provided. I will make sure that the problem does not occur in the published version, should the paper make it to that stage.

Equation (2) needs to be derived, or referenced, and motivated. What's the motivation for calculating heating rates in this way? Why is this method chosen? Calculation of the derivative with respect to depth of the net planar irradiance is the more conventional way to estimate heating, but this method of using the scalar irradiance is used instead. Why? What is the advantage of using this method? Are there differences between the two approaches?

The two methods are essentially equivalent, as equation 1 states. As for why use scalar irradiance, it is a little more convenient when the scalar irradiance and total absorption are available directly from the model. We could have calculated the change in net planar irradiance between detectors and obtained the mean heating rate for the layer, and the result would have been nearly the same. The only difference is that with scalar irradiance you get a value for a discrete depth, but with planar irradiance you get a value for a layer.

We have rewritten this paragraph so that it hopefully is more clear.

Figure 2. It would be helpful to place some of the stated b values in context. b = 270 m-1 for the SSL seems small, but the value of g is not stated, so it is difficult to tell, also the geometric thickness of the SSL is assigned the entire freeboard depth. Likewise b = 25 m-1 for the ice interior seems large, but difficult to compare without specifying g. Would be interesting to see how these compare with other published estimates (eg., Light et al., 2008 Fig. 11 and Light et al., 2015

We have modified the figure to report the effective scattering coefficient, $b_{\text{eff}} = b(1-g)$, instead, and discussed the values in comparison to in particular Light et al. 2015.

Fig. 8). The need to assume a 20% brine volume fraction (p.5, line 11) for this ice probably means this ice was structurally rotten. This summer ice has likely undergone significant retexturing and excessive brine drainage. This casts doubt on the reliability of a traditional structural-optical model (based on freezing equilibrium relationships), as appears to have been used here. I think it important for the authors to comment on this. I understand that ice microstructural analysis was beyond the scope of this work, but I wonder what the ice looked like? Is there a relevant photograph of an ice core that could be included?

Also, on p. 11, 1st paragraph, it is interesting that the authors inferred a much smaller air volume fraction than at least one previous study. Not all air content is the same! Some air will be associated with bubbles formed directly in the ice, some associated with brine inclusions and freezing equilibrium, and some air volume may result from the retexturing of ponded snow. Additionally, some air volume may be associated with the above-freeboard portion of the ice after it drains. Given this,

it would be useful to see some discussion about the nature of the air expected in this ice.

We have added the below discussion of this matter to the manuscript. Figure X is a new figure showing an ice core extracted near the dive hole for the third transect (added to end of this document)

"The ice was in a late stage of melt, with temperatures near 0C at the top and around -1.2C at the bottom. On the bare ice regions there was a surface scattering layer above the freeboard, which consisted of deteriorated ice that was in a granular form, similar to coarse grained snow. Figure X shows that the interior ice was visibly porous, with some deteriorated internal layers. The ponds on the surface had drained to sea level, indicating that most brine pockets or channels would have been filled with melt and/or sea water. Detailed structural analysis and identification of air- versus liquid-filled inclusions was hampered by the immediate drainage of the liquid inclusions when a core was taken up, but it is safe to say that the ice did not retain the traditional structure of sea ice, determined by freezing equilibrium relationships. As a result of the heavily modified state of the ice, our estimates of brine and air volumes and sizes may be highly uncertain."

p.14, line 3-10: The authors do correctly explain that there will in reality be little or no spatial variability of incident irradiance along the transects, so I have trouble understanding why results treating cloud-surface interactions are even presented in a 1-D modeling study.

Merely to point out that fixed atmospheric conditions in simulations with different surfaces give different incident irradiance, which matters if you'd want to calculate absolute values of energy absorption.

The histograms shown in Fig. 5 seem interesting, but there is no accompanying discussion about how to interpret them.

Some additional comments on this has been added to the corresponding paragraph, which now reads as follows:

"The distribution of simulated transmittance is bimodal, with one mode corresponding to ponded ice and chiefly to bare ice. Measured transmittance has a trimodal distribution, where the third mode may be at least partly a result of the lighter blue ponds. For the simulated transmittance, edge cases will have been shifted towards the one of the modes, while the data points corresponding to light ponds are part of the mode related to bare ice, due to their lower transmittance."

> *Figure 6 shows the mean gradient of the simulated and observed spectra, although there is little discussion of what motivated this calculation in the text. I understand that there are differences between the observed and modeled text, but there is no discussion about the physical meaning of these differences.*

Section 4.2 has been rewritten to include some more discussion on this.

> *The conclusions section seems weak. The aspects of this manuscript that seem most important are: 1. The high spatial resolution, and comprehensive nature of the observed transmittances along three transects. There was precious little discussion about how these observations compare with previous studies of light transmittance through ponded ice, and there was no discussion about the variability of the observed transmittances.*

Section 4.1 has been expanded with a comparison to various other studies.

In addition a sentence has been added to the conclusions highlighting the high spatial resolution:

"Under-ice irradiance was measured approximately every meter along three transects covering both ponded ice and bare ice, demonstrating how transmittance may vary near edges of ponds."

> *2. The modeling work, and a discussion of what new contributions are being made. This is not the first time radiative transport through bare and ponded sea ice has been simulated using a 1-D radiative transfer model (e.g., see Light et al., 2008, 2015), so some discussion of what new contributions are being made to our understanding of radiative transfer modeling in this domain seems warranted. I also think the computation of luminosity from the aerial imagery of the ponded ice, and its correlation with ice transmittance is a very useful result.*

Whereas e.g. Light 2015 also did modelling for ponded ice, that was for a few select cases, presumably in the middle of ponds. As such, edge effects were not considered. Added another sentence in intro:

"Neither did Light et al. (2008) or Light et al. (2015) consider edge effects, focusing instead on a few select case studies"

Minor points

> *Figure 3, : missing axis labels, also legend labels need more description ('Transect'g 1, 2, 3).*

Axis labels were discussed above. There was a *"T"* before the numbers in the legend, this has been changed to *"Transect"* for clarification.

> *P4, L14: "Mie-code version", how about "a parameterized Mie model"?*

This has been rephrased as follows:

"Their inherent optical properties are calculated using a parameterization based on Mie calculations (Stamnes et al., 2011)."

Table 1. What are the units for various "h" columns? I assume meters, but it is not stated.

Correct, that is meters. Units are added.

p. 12 line 21-22: is that Eqn 2? There needs to be some motivation for using this approach.

P12, L21–22: Correct, equation 2. A cross reference is added for clarification. See above for motivation.

p. 15, line 15 -16: Seems this is a worthwhile point and should be expanded upon. "Obtaining information about ponded ice from aerial images as described above. . . ". I presume the authors are referring to the calculation of luminosity from aerial images, but it would be helpful to, here in the conclusion section, fully summarize the calculation that was performed and comment on the utility of it – what worked, what didn't, what would be needed to make this technique viable?

Indeed, that is referring the calculation of average intensity (previously luminosity, see comments by the second reviewer). As pointed out by the second reviewer, similar methods has been used in other studies, and these have been mentioned in this context. A full summary of the calculation does not seem that useful here, but we have extended the lines in question as quoted below. An additional caveat is the question of pond size, as remarked by reviewer 2.

"Obtaining information about ponded ice from aerial images as described in section 2.4.1 shows potential for this particular type of study, and similar techniques has been applied successfully in other studies (Divine et al., 2015; Katlein et al., 2015). Using said information in radiative transfer modelling appear useful in this case, though

the exact method might depend on the data that are available, as well as the model itself."

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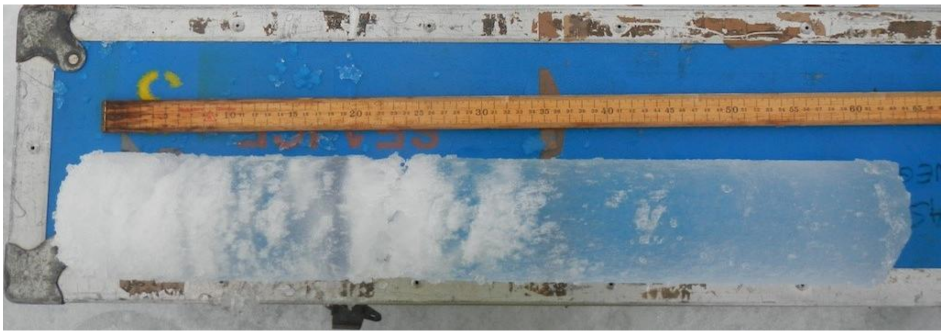

Fig. 1. Photo of an ice core extracted from bare ice near the dive hole of the third transect.