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The Cryosphere Discussions

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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First, regarding the figures. The figures did of course have axis labels and legend entries when we submitted, I believe there were some issues relating to font embedding that caused them to disappear when the header was added by the journal. As I said to the other reviewer as well, I do wish you had posted a short comment on the discussion page, so that a working PDF could have been provided.

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General comments

The paper "Modelling radiative transfer through ponded first-year Arctic sea ice with a plane parallel model" by Torbjørn Taskjelle et al. describes modelling exercises of sea ice light transmittance based on measurements taken in 2012 in the Arctic. The topic is of high interest for large scale estimation of solar energy fluxes in the Arctic climate system. The manuscript provides a worthy contribution to the ongoing evolution of sea-ice optics, however it falls short on its claims.

The manuscript will need some improvements before publication in the Cryosphere: My main point of criticism is, that the authors claim to solve the puzzle wether spatial mean values can be correctly represented using plane parallel models. This single case study does not provide this proof, especially as the main critical topic in this case is not even discussed in the manuscript: That plane parallel models can be used to estimate mean values of light transmittance, when the individual patches are much larger than the spatial scale of pond-related edge effects is a rather trivial conclusion. The authors fail to address the critical case, when ponding features have a diameter of less than two times the edge influence radius, when the full pond transmittance value is not reached. At an ice thickness of 1m, this is the case for all features smaller than 4m which might be a significant portion of the ponded landscape.

It was not our intention to claim a final solution to that puzzle, so perhaps some of the statements were a bit strong. Your final part, regarding ponds that are small relative to the ice thickness, is a very good one. Hence, we have made a couple of additions to the text.

To the end of section 4.4.:

"While the average values simulated values correspond well to measured values, one should note that the ponds in this study are generally wide compared to the ice thickness. A similar correspondence would not necessarily have been seen if the ponds were so small, or the ice so thick, that the light field beneath most or all of the ponded

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area were affected by the surrounding bare ice."

To conclusion:

"Our study does not, however, address the case in which the typical pond width is less than twice the ice thickness, so that the entire ponded area is affected by edge effects."

> *While I support the use of aerial images as a great tool for spatial analysis of optics in ponded sea ice, some aspects are missing: A very similar approach was used by Katlein et al. (2015, JGR) which should be mentioned in the text. An aerial image is rather a measure of surface albedo, so transmittance should be anti-correlated and not correlated, also ice-thickness should play a role here.*

It is clear from figure 3 that the correlation is negative, but the text is edited to explicitly state so:

"Average intensity is seen to have a clear negative correlation with transmittance . . . "

The reference you mention has been added in the first paragraph of section 2.4.1.

> *Secondly, the authors should double check, wether their use of Luminosity (which seems to be incorrectly used as term for Luminance) – a photometric quantity – makes sense in this purely radiometric context. Sea ice light transmittance, as well as RGB CCD sensitivity is independent from the visual impression of a human eye.*

We have changed the method here according to your comments, see the specific comment below.

Specific comments

> *p.1 ll. 2-3: These transmittance values seem rather high compared to other values observed in the same area in the same year. They should be discussed in the*

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context of existing literature later in the text.

Section 4.1 has been extended to include a comparison of several other studies, including Wang et al. (2014) (http://dx.doi.org/10.1002/2013JC009459) and Katlein et al (2015) (http://dx.doi.org/10.1007/s00300-014-1634-3).

Figure 1: Almost all figures are lacking proper axes labels. Here it would be extremely helpful to provide a scale bar or ticked axes. Also the profile lines and names should be presented in a higher contrast.

Figure labels were discussed above. The profile lines and names have been modified to hopefully make them clearer, and a scale bar added.

p.2 ll. 9-10: This is not a general property of ROV surveys. In principle ROV surveys can provide exactly the same positioning directly against the ice with sufficient precision. This is a particular property of the cited surveys but not a general property of ROV surveys.

Thank you for that insight, the particular sentence has been modified to the following:

"A disadvantage of some ROV studies is that the ROV is operated too far below the ice on horizontal transects to completely observe local effects."

p.2 l.12: Is it really necessary to abbreviate radiative transfer as RT? Readability in- creases when writing it out.

Edited as suggested throughout the manuscript.

p.3 l. 19: Where exactly was this surface sensor? On the ship? On a tripod? Which height? What distance to the site?

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This paragraph was clumsily written, so has been rewritten. It now starts as:

"Incident irradiance was measured coincidentally, with the same type of instrument as below the ice, but mounted on a tripod by the dive hole, near the start of the transect. We assume no horizontal variability of incident irradiance between the measuring site, and the transect."

> *p.3. ll.21-22: This procedure is described in Nicolaus et al. (2010, CRST); Currently the sentence is very cryptic for someone not knowing the RAMSES sensor.*

This has been rewritten as:

"Spectra were collected simultaneously from the two sensors, and as RAMSES sensors are not generally not calibrated to exactly the same wavelengths, the spectra were interpolated to a common wavelength grid, with $1\ \mathrm{nm}$ *spacing."*

> *p.3. ll.23-28: Why do you mentioned what was NOT done? Maybe this can be left out.*

My thought was that readers might ask themselves whether such observations were carried out or not, if no mention of them were made.

> *p.3 l.30: The word designate sounds weird in this context.*

I think *"designate"* is appropriate, but we have changed to *"identify"* throughout the manuscript.

> *p.4 l.4-5: This infomation is not as central as the following information about AccuRT so it should be moved down or maybe even left out.*

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Fair point, this has been moved to the end of the section.

> *p.4 l.15-17: At this point parameter choices seem very arbitrarily. Maybe one should highlight early on in the paper, that the model parameters were selected according to best fit field observations (and then, how were they selected? Inversion?)*

I don't understand why this comment refers to these lines, which describe what model output one can obtain, at this point we haven't said anything about parameter choices at all.

But we have added the following at the start of section 2.4:

"Model parameters were chosen through an iterative process to obtain a good correspondence with the measured data."

> *p.4. l.21: It would help if you label the consituents of net irradiance to be planar irradiances.*

Have specified that it is planar irradiance.

> *p.4. l.23: the more common order would be E_0-up as E_0 indicates spherical. If you split it with comma, the 0 could refer to a depth level.*

Good point, thank you. Edited to $E_{0\uparrow}/E_{0\downarrow}$.

> *p.5 l.2: I think you mean "spectral resolution" instead of "bandwidth"*

Edited as suggested.

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p.5. l.4-5: The choice of 10cm of SSL seems fairly arbitrary here (See comment above). The reason for parameter choices should in general be made clearer in the following. Explanations are there but poorly structured.

This top layer of ice beneath the ponds is not meant to represent a typical SSL composed of granular ice, simply a layer with more bubbles than the deeper ice. Its exact thickness in simulations is however arbitrary, as stated explicitly on line 7 of that page. See also above comment on parameter choices.

p.5. l.18: I am surprised, that you evaluate your radiative transfer model down to 3000m. Isn't this very unefficient numerically?

No, with the discrete-ordinates method used here (see e.g. book by Thomas and Stamnes, 1999) the layer depth isn't really significant for computation times.

p.6. l.14 ff: I think, that luminosity is the wrong term here (it is not even used in your reference), secondly i do not see the reason why you would want to correct for human perception. I suggest to stay in the radiometric context and either use mean intensity or you need to weigh the three channels by the incoming values. Also this paragraph lacks any reference to Katlein et al. 2015 who already showed a very similar approach. Also, you seem to ignore any effect of ice thickness here.

Using mean intensity is perhaps a better approach, so we have changed the method accordingly. Thank you for the suggestion.

We do not ignore the effect of ice thickness, cf. p. 6 line 22 ff.

Figure 4: What is the actual width of your presented image stripes? This should be mentioned in the text. Also, cross transect mean luminosity is not the approriate quantity to compare to. Mean intensity should be calculated over a circular footprint area (weighted by radius) to resemble the sensor footprint at the ice surface.

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It is mentioned in the text (line 13, page 7).

You are of course correct in your second point here, and the method has been modified accordingly. Section 2.4.1 now has the following:

"The along-transect average intensity, shown in fig. 3, is calculated as follows. For each pixel along the approximated center line of the transect, a weighted average of a 21 pixel wide square around the pixel was calculated. The weights were given by a Gaussian function, with a standard deviation corresponding to 5 pixels, centered on the middle pixel."

> *Figure 5: Labels are missing. Your histograms seem very jagged. Likely your bin sizes are too small for this small dataset.*

Perhaps you're right. The bin width has been doubled.

> *p.10. l.1-2: Edge cases will not amount to a 3rd "edge" mode, but rather show up as a continuous plateau between both modes. This rather looks like you do not have a clear bi-modal distribution on your floe, but rather a 3rd mode which might be related to light blue melt ponds. This is likely evident from an "albedo" histogram of figure 2.*

Thank you for that insight, I can see your point. The paragraph has been rewritten 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."

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p.10. l.4: "Observation uncertainties"

We have in this paragraph added some comparisons with other transmittance measurements, as requested. So while changing the heading as suggested made sense considering the original content of the paragraph, as it is now we'll keep the original heading.

p.10. l.6-10. While you try to correct for positioning errors on cm scale, i doubt, that your data has actually this precision. Drills are already 5cm thick and just a very slight tilt results in several decimeter error. I doubt, that you have a sub dm accurracy which seems to be described in this paragraph.

Your point about the precision of the data is certainly valid. However, the rope stretching, would cause positional differences of up to 0.5 m, even if it some cases it was just a few centimetres, so all of them was interpolated. Additionally, location error due to tilted drilling will be minimized here because the ice was so relatively thin. We have rewritten this to hopefully make it clearer, the paragraph now ends as follows:

"Finally, the rope stretching also means that the under-ice irradiance measurements were not performed in the same locations as the thickness drillings, with differences possibly up to $0.5\,\mathrm{m}$. To account for the rope stretching, ice thickness, pond depth and freeboard were interpolated linearly to the estimated locations of the irradiance measurements (fig. 4), though there remains some uncertainty in the exact locations of both the irradiance measurements and the thickness measurements."

p.10 ll. 12-14: You describe the difference in sampling times, but you do not discuss it or even state that you don't think it affects your data.

It was implicitly mentioned in lines 13–14, but we have rewritten to:

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"As the surface may have changed somewhat in the interim, the average intensity obtained from the image may not exactly represent the conditions at the time of the radiometric measurements."

> *p.10. ll.15-16: These statements feel more like a method description rather than a discussion. I suggest to move the remnants of this paragraph to the end of the discussion and present important discussion first.*

The sentence in question has been removed.

> *p.10. l.17. ff: The reason and purpose of this paragraph does not become evident. It is certainly valuable, but needs more motivation and explanation.*

We have updated this paragraph with a short motivation for the figure and also rewritten the discussion of the figure in a way we think makes its relevance clearer.

> *p.11 l.10: Obviously AccuRT can not account for anisotropic radiative transfer, however anisotropy is crucial to edge-effects at pond edges, so it should be mentioned also along the discussion of spatial scales (above general comment).*

The following has been added to the end of section 4.3:

"Further, the vertical orientation of brine inclusions commonly seen in sea ice, appears to cause anisotropic scattering in the ice (Trodahl et al., 1989; Katlein et al., 2014), which AccuRT cannot account for. This issue may be important for determining the exact radius over which edge effects are important."

> *p.12 l. 12ff: This does not account for small spatial scales (see above). Furthermore this equation is written for transects only, but it would improve the usability of this paper if you suggest a form for aereal estimates.*

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The paragraph in question has been extended with the following:

"This approach can easily be applied to area fractions of surface types by replacing the lengths by areas."

Figure 8: All lables are missing. What is the relation between PAR and BB and at which depth can we use a conversion constant between them.

A conversion constant between these two quantities and any relation between them will be strongly dependent on the properties of the ice and on the incoming spectrum. Clouds will reduce the amount of incident light outside the PAR range, decreasing the depth at which the two become nearly a constant multiple of each other. Increased scattering near the surface would allow for more interactions within a shallower depth, reducing the depth at which most non-PAR light has been absorbed. Overall, this seems like a complicated topic that cannot be properly addressed as a side note here.

p.15 l. 6-7: Your manuscript lacks a comparison of measured fluxes to existing datasets.

Section 4.1 has been extended to include this.

p.15 l.14: I suppose it is also very much supporting the works of Arndt & Nicolaus (2014,Cryosphere) and Nicolaus (2012, GRL).

Yes, and they have been cited.

p.15 l.15: This approach is not new. It has been demonstrated in Katlein et al. 2015 and also in some works from the campaign at hand (Divine et al.)

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Good point, rephrased to

"Obtaining information about ponded ice from aerial images as described above shows potential for this particular type of study, and has also been applied successfully in other studies (Divine et al., 2015; Katlein et al., 2015)."

> *p.15 l.26: Pleas provide an accurate pointer (doi) for the associated dataset. A general pointer to a search engine is not enough.*

As the text stated, the dataset had not yet been published at the time of submission, so the DOI was not available. It has now been added.

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