The Cryosphere Discuss.,
doi:10.5194/tc-2017-36-RC2, 2017

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.

Anonymous Referee #2

Received and published: 24 April 2017

The paper "Modelling radiative transfer through ponded first-year Arctic sea ice with a plane parallel model" by Torbjbørn 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.

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. 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.

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 context of existing literature later in the text.

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.

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.

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

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

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.

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

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

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.

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?)

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

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.

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

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.

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

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.

Figure 3: Again all axes labels and legend entries are missing

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.

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

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.

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

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.

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.

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.

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.

Figure 6: All labels are missing

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).

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.

Figure 7: All labels are missing

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.

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

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

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.)

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

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