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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 #1

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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

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Discussion paper

useful contribution to the literature. However, I think substantial revisions are necessary prior to publications.

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

Minor points Figure 3, : missing axis labels, also legend labels need more description ('Transect'g 1, 2, 3). P4, L14: "Mie-code version", how about "a parameterized Mie model"? Table 1. What are the units for various "h" columns? I assume meters, but it is not stated. Fig. 4: missing y-axis labels. Fig. 5: missing x- and y-axis labels. Fig. 6. Missing x and y-axis labels. And legend labels? p. 10 line 21: without legend labels, and no description in the legend, difficult, if not impossible, to tell which curves correspond to the model, or bare v. ponded cases. p. 12 line 21-22: is that Eqn 2? There needs to be some motivation for using this approach. Fig. 7. Axis labels! Color bar labels? Fig. 8. Axis and legend labels! 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 itâĂŤwhat worked, what didn't, what would be needed to make this technique viable?

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