

Interactive
Comment

Interactive comment on “Seasonal cycle of solar energy fluxes through Arctic sea ice” by S. Arndt and M. Nicolaus

Anonymous Referee #2

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General Comments

This manuscript describes a novel parameterization of solar energy partitioning in sea ice and attempts to use this parameterization to analyze long-term trends in light transmittance over the Arctic basin. The strength of the paper lies in the simplicity of the technique – only readily available remote sensing and reanalysis data are used to derive the estimates of transmittance. However, the paper also suffers from this simplicity. Several components of the parameterization are weak and the sensitivity analyses, although well intentioned, have little practical value and need to be revised. I recommend that the manuscript is suitable for reconsideration following major revisions.

Major Comments

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1. The title refers only to the seasonal cycle of energy fluxes, but the paper analyzes both the seasonal cycle and long-term trend in energy fluxes. A possible modification might be: ‘Seasonal cycle and long-term trend of solar energy...’.
2. Define transmittance early in the introduction and explain the energy budget of sea ice (surface EB, absorption, transmission, radiative flux, conductive flux, ocean heat flux).
3. The paper objectives need to be articulated better (P 2926, L 1-11). Exactly what are you trying to achieve? You must state here that you are aiming to estimate solar transmittance for the entire Arctic basin, for the period 1979-2011.
4. The melt pond fraction parameterization is incredibly generalized, given how far the values from a relatively small dataset [Nicolaus et al. 2012] are extrapolated in time and space. Currently the values for FYI and MYI appear too close – look at variations in measured pond fractions [Eicken et al., 2004; Polashenski et al., 2012; Landy et al., 2014]. The authors could try calculating average pond fractions for FYI and MYI as reported by Rösel et al for the period 2000-2011 and see how they compare to their constants. Alternatively, the authors could use pond fractions as predicted by the sophisticated sea ice/melt pond model of Flocco et al., 2010. For 1990-2007 Arctic-wide melt pond hindcasts see Flocco et al., 2012.
5. Transmittance varies significantly with snow depth [e.g. Perovich 1996]. Could the snow depth simulation product from AMSR-E [Cavalieri et al., 2012] be used to better parameterize transmittance in the winter and spring, along with the ice age (i.e. ice thickness)?
6. The transmittance values at P 2931, L 16-18 ideally should not be constants, but should change as a function of the ice thickness. I appreciate there is no available long-term remote sensing ice thickness product; however, the parameterization would benefit enormously if ice thickness is included. One possible solution is to use a sea ice model to provide an estimate for April-Sept ice thickness (again see Flocco et al., 2012)

and parameterize transmittance directly. Otherwise you need to discuss the potential limitations of using ice age as an indirect proxy for ice thickness.

7. The corrections mentioned at P 2933, L 2-6 need to be explained in more detail. L2-4: The trends in transmittance were normalized based on the trends in ice concentration? L4-5: Ice-covered area at the September minimum in 2011, or ice-covered area month-to-month between years? The paragraph starting at P 2936, L 24 was very difficult to understand because these corrections hadn't been adequately explained. Incidentally, why were the regions that were not ice covered in 2011 excluded? Given that you are attempting to estimate long-term trends in solar heat input to the ocean, would it not be more realistic to include open water areas by assigning a grid cell a transmittance of 1 as soon as it becomes ice free? The strong drop-off in solar heat input estimated for August (P 2934, L 9) must partially be attributed to this exclusion, despite the seasonal decrease in solar irradiance.

8. Figure 2 in Perovich et al. 2011 actually shows that the trend in solar input to the sea ice cover (not through the ice, as is written in the manuscript) is $< 2\%a^{-1}$ and generally $< 1\%a^{-1}$. Therefore, the author's results are quite similar to those of Perovich et al. – both demonstrate a positive $0-1.5\%a^{-1}$ trend in energy input to the sea ice cover or ocean. The author's interpretation of Perovich et al.'s results, and their reasoning that greater energy absorption in the ocean than the sea ice cover is required for a long-term acceleration in bottom and internal melt, are incorrect. However, the overall point is not necessarily wrong. Increasing energy in the ice and upper ocean should both lead to greater ice melt. Radiative heating of the upper ocean should produce a higher conductive ocean heat flux to the ice. Another relevant point to bring up here is the influence of biological material on the measured transmittance during the Tara drift study (P 2937, L 23). The observed solar input to the ocean was very low compared to the input predicted by your parameterization. Consequently, only a fraction of the predicted heat input would have actually contributed to ice melt, because the impact of absorption by biota was ignored. Can this fraction be determined from the difference in

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observed versus predicted solar energy input and used to speculate on how much the parameterization overestimates solar heat input to the ice, as a result of biota in the ice or ocean?

9. At present the sensitivity studies in Section 4.3 are relatively meaningless. The studies appear to show that solar heat input to the ocean is most sensitive to the timing of the transition between melt/freeze stages and the relative proportions of FYI versus MYI. However, the chosen 7 and 14 day shifts in EMO and MO appear to have been picked arbitrarily. Also it is unrealistic to estimate the variations in heat input associated with an entirely FY or MY Arctic ice cover. It would be more useful to calculate the sensitivity of estimated heat input or transmittance based on reasonable uncertainties in these independent variables. For instance, rather than choosing an arbitrary 7 or 14 days, why not calculate the average standard anomalies of EMO or MO and use these values to estimate the percentage change in heat input. Otherwise use the standard deviations of melt/freeze dates as provided in Table 2 of Markus et al. 2009. Markus et al report std dev in EMO of only 3.6 days and MO of only 3.7 days for the Arctic basin from 1979-2007. Similarly, instead of assuming an entirely FY or MY Arctic ice cover, look at the uncertainties reported by the data provider for their ice age classification (probably a few %) and use these to estimate the sensitivity of heat input.

10. The prescribed variations in melt pond fraction of 10 and 20% (at P 2940, L 22-26) are more realistic. Given that the estimated solar heat input to the ocean is particularly sensitive to these variations, melt pond fraction is clearly a key component of the parameterization. It would be very interesting to try using pond fractions and ice thickness derived from the model of Flooco et al. 2010, rather than constant FYI/MYI pond fractions and basic FY/MY ice age discrimination, to drive the transmittance parameterization and compare results. It is likely that these improvements would strengthen the results of the paper, in turn allowing for more robust discussion and conclusions.

Minor Comments

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Abstract. Line 17. What about the annual budget increases?

L 18-20. Is this speculation? This has not been proven in the paper.

P 2924, L 26. What do you mean by 'general'? A decrease in area-averaged or total albedo?

P 2925, L 18. 'Obtained' seems like the wrong word.

L 23-26. This sentence is confusing. Why are they only available in August? Do you mean that the method of Nicolaus was limited to August, because that was the only month where observations were available?

P 2926, L 9. Tara drift study? You must provide a brief explanation of these studies and give them their full name. There are other examples where a loose reference is made to a study but it is not explained properly, e.g. SHEBA and Tara on P 2930, TransArc on P 2931.

L 16. The method and parameterization of Nicolaus et al. should be explained in more detail if this study is building on it. What exactly did the former parameterization include and what is new about this one?

L 19. 'driven by', rather than merged with.

L 21. There is no mention of the method of interpolation. Also if the sensitivity of the results to the scale of interpolation was analyzed.

P 2927, L 7-8. What are these uncertainties? Crucially are they high enough to affect the resulting calculations of transmittance and heat input? This is important for the sensitivity analyses.

L 10. Which satellite? Lagrangian feature tracking?

L 12-13. There should be a basic description of the differences in optical properties between FY and MY sea ice in the introduction.

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L 14-15. This needs to be explained better. Ice conc < 15% but with an age tag – is this rotten/fragmented former MYI? Why is it treated as open water?

P 2928, L 1. Need to explain what the product is and how they get it (i.e. MODIS).

L 6. Why 'up-scaling model', upscaling from what? Why not 'solar heat flux parameterization' or similar.

P 2929, L 17. Explain better what the difference between new MYI and MYI is, and why it is relevant.

P 2930, L 1. Remove comma after 'both'.

P 2931, L 1. Where does Perovich 1996 describe/show this? Do you mean that the increase in transmittance of the sea ice cover at the aggregate scale is roughly exponential? You need a relevant reference to state this.

L 1-3. Do you mean the transmittance decreases as the inverse of albedo while the sea ice surface is melting? And what is < 10 cm? The last existing sea ice is assumed to be < 10 cm thick?

P 2932, L 20. What is the 'scaling factor'?

P 2933, L 8. Try '2011 seasonal cycle of solar radiation...'

L 13. 'Results'? You mean for validating the parameterization?

L 17. How did you get this annual Arctic-wide total heat flux? Is this something you calculated? If so explain exactly how. Or is it a value found in the literature? If so, cite.

L 16-. It could be useful to normalize these values by either the annual maximum transmitted energy or heat flux, or as a percentage of the total heat flux at the ice surface. Something like this could help when you make comparisons between months or regions.

L 23. Most pronounced compared to what? Other monthly increases?

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P 2935, L 2. 'According to...'

L 5. How do you know this is due to lower surface irradiance? Did you test this statistically (regression or ANOVA)? Or is it speculation (if so move to the discussion)?

L 11. This is discussion.

L 15. Important in what context? Radiative right? Not in terms of the conductive heat flux, which is of course an incredibly important component of the 'basal' energy budget in fall and winter. Maybe use radiative energy budget, or radiative energy partitioning instead.

P 2936, L 16-19. These sentences are out of place and confusing. Consequently from what? The previous sentence is about light availability for primary production. You are trying to say that an increase in transmittance will accelerate internal and bottom melt, which in turn will reduce the thickness of the ice and increase transmittance? You must explain these speculations in full.

L 21. More ponds? Or greater pond coverage makes more sense, no?

P 2937, L 1. 'the impact for primary production is expected to be largest', needs a reference.

L 2-4. This sentence needs rewording.

L 5. This section might be more appropriate in the results if it is supposed to be a validation for the transmittance parameterization. Is it a validation or a comparison with published observations/measurements? You mention both.

P 2938, L 10. It is stated 'conclusively' that solar heat input under sea ice depends vastly more on the timing of EMO and MO than EFO and FO. This may very well be the case, but isn't a valid conclusion based on the results presented. The calculated flux depends on the timing of EMO and MO, but only because the timing is assigned such a strong importance in the parameterization, i.e. there is such a strong transition

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in transmittance between winter, EMO and MO.

P 2939, L 10-12. Are Hudson et al.'s measurements of the 'ocean heat flux' not a combined heat flux to the sea ice from the ocean and also from radiative heating of the upper ocean by transmitted solar radiation?

L 18. 'The main reasons...'

P 2941, L 8. Change 'studies' to 'results'.

L 12-15. I don't believe that your results or discussion support this conclusion, because the sensitivity studies are unrealistic.

L 24-27. While the underlying point is surely relevant, your discussion doesn't back this up. See comment 8 above.

Table 2. Why is there pond-covered sea ice in winter (Phase I)? Why is the transmittance for Open Ocean in Phase IV not 1?

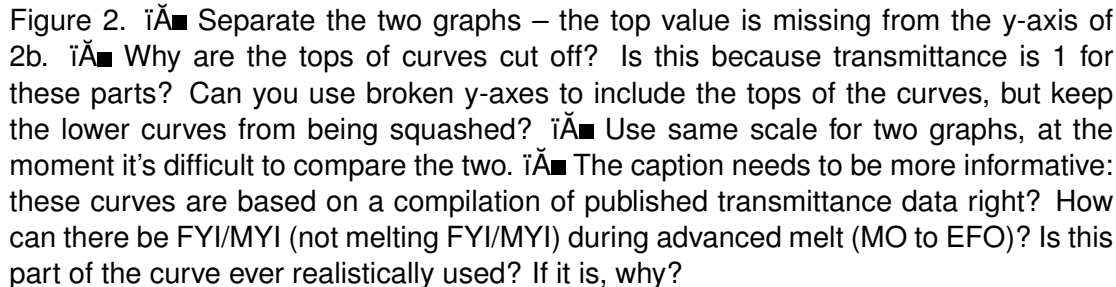
Figure 2.  Separate the two graphs – the top value is missing from the y-axis of 2b. Why are the tops of curves cut off? Is this because transmittance is 1 for these parts? Can you use broken y-axes to include the tops of the curves, but keep the lower curves from being squashed? Use same scale for two graphs, at the moment it's difficult to compare the two. The caption needs to be more informative: these curves are based on a compilation of published transmittance data right? How can there be FYI/MYI (not melting FYI/MYI) during advanced melt (MO to EFO)? Is this part of the curve ever realistically used? If it is, why?

Figure 5. These graphs are not clear – increase line width.

Figure 6. Again not clear. Have you assessed the statistical similarity of the two datasets, e.g. by using correlation analysis? How much of the observed variance is explained by the parameterizations?

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Interactive comment on The Cryosphere Discuss., 8, 2923, 2014.

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