**Review of "Seasonal cycle of solar energy fluxes through Arctic sea ice" by S. Arndt and M. Nicolaus**

Anonymous Referee #2
Received and published: 12 August 2014

## 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.
We highly appreciate the great work that the reviewer put into revising our manuscript. This work is really excellent and certainly above our expectations. We realize that he / she is really familiar with this field of research and has a great expertise to make most useful comments and suggestions.
We did quite comprehensive modifications based on this review, including some additional sensitivity studies to put more confidence into those results.

## Major Comments

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: : :.'.
We changed the title as suggested to 'Seasonal cycle and long-term trend of solar energy fluxes through Arctic sea ice'.

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).
We followed the suggestion of the reviewer and added an additional short paragraph describing the energy budget of sea ice.

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.
We followed the suggestion and articulated our objectives clear in the last part of the introduction with the focus on the aim of the method and the valid time period for the calculations.

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 hind casts see Flocco et al., 2012.
We thank the reviewer for these notes as we figured out that there is a mistake in the manuscript.

The melt pond fraction before 2000 is set to an averaged melt pond fraction of August 2011 for FYI and MYI from the melt pond fraction data set of Rösel et al. (2012). This is corrected in the manuscript.
As the mean pond area for 2011 matches the mean value for the entire period of 2000 to 2011 very good [Rösel et al., 2012], the usage of the averaged pond fraction of 2011 seems to be valid.

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)?
A more precise knowledge about snow depth would definitively improve the parameterized transmittance. Nevertheless, we consider the suggested AMSR-E snow depth product as not useful for our work, after engaging ourselves in the product by Cavalieri et al. (2012). On the one hand, the product is only available for FYI. On the other hand, it is just available until the beginning of the melt season, thus mainly for the winter season. But as the time after the beginning of the melt season determines the annual budget of solar heat fluxes under sea ice (as later shown), these snow depth data seem not useful for our parameterization.
Nevertheless, we included this discussion part also into the manuscript by describing shortly the non-use of the snow depth data in the end of section 2.1 (Methods/ Input data sets). Furthermore, we discuss the limitations of the current snow depth data in the end of section 4.2 (Discussion/ Comparison with in-situ measurements).

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.
Also the implementation of the sea ice thickness (as well as the snow thickness) would give a more accurate description of the present sea ice cover. As mentioned already in the beginning, we think that the implementation of sea ice thickness would change crucially our presented method. Our aim of a simple parameterization would than pass into a complex radiation transfer model. This would miss the focus and idea of our work.
The limitation of the current sea ice thickness data as well as the limitations of the ice age as indirect proxy is also described in section 2.1 and 4.2, as for the snow depth.

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.
Yes, the trends in transmitted heat fluxes were normalized based on the trends in sea ice concentration. We rewrote the paragraph for a better understanding. In general, all trends were calculated for every month based on the ice-covered area in that respective month in 2011. All annual trends are estimated for the mean ice covered area for the entire year 2011. For both calculations only areas of a sea ice concentration bigger than 15% were considered.
For the main analyses we exclude all open water area as those would clearly dominate the

transmitted heat flux signal. Nevertheless, heat fluxes through open ocean and sea ice are highly relevant for the heat and energy balance of the entire open ocean. Therefore, we calculated and compared these fluxes additionally in the end of section 3.2 (Results/ Light transmission from 1979 to 2011). For all other results we consider only fluxes through the ice cover as these are crucial for the energy and mass balance of sea ice as well as for biological processes beneath the ice cover. We also added an explanation why we mainly focused on under-ice fluxes only in the methods section (section 2).

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.
We thank the reviewer for this precise analysis of our comparison to the work done by Perovich et al. (2011). We agree that the comparison was not deep enough regarding a comparison between a maximal range and a mean value. We fixed this and came to the same conclusions as annotated by the reviewer. The entire paragraph is rewritten.

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 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?
Estimating the overestimated solar heat input to the ice during summer due to biota in and beneath the sea ice is a good point concerning the comparison with the Tara data. As we added to all heat flux calculation the consequent melt rate in cm, anyway, this overestimated heat flux or rather melt rates are clearly underlined in the adjusted manuscript.
However, these overestimation calculations can not be generalized towards a spatial and temporal upscaling as the biota strongly depends on nutrient availability.

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.
We thank the reviewer for the hints to underline the relevance and focus of the sensitivity studies. Now, we included the (known) uncertainties of each data set in data description (section 2.1). But

more importantly, we performed additional sensitivity studies and restructured this section. The sensitivity studies are now directly related to the reported uncertainties as well as on the calculated ones (from described validations). As a consequence, we now present three studies/experiments:

- Sensitivity study 1: Changes in timing and length of melt season based on (a) data uncertainty, (b) uncertainty due to weekly ice age data, (c) uncertainty derived from field data
- Sensitivity study 2: Changes in sea ice age distribution: This data set contains no uncertainties (see section 2.1). Consequently, this study is based on ongoing discussions towards an only FYI-covered Arctic se ice area. Unfortunately it is not possible to discuss other trends or distributions of FYI/MYI since this would require additional assumptions on the spatial distribution of the different ice types.
- Sensitivity study 3: Changes in melt pond fraction based on (a) uncertainty of data set, (b) estimated uncertainty due to the neglected seasonal cycle.
- The discussion of changes in sea ice concentration and surface solar radiation fluxes are neglected due to a linear effect of this variables on transmittance.

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.
We thank the reviewer for the widespread ideas of improvements of our parameterization. As mentioned in the beginning, we think that an implementation of model data would also miss the focus of our work. Nevertheless, we fully agree that an involved seasonal cycle of the melt pond transmittance would increase the accuracy of the given calculation. Due to the limited existing field data it is currently not possible to include a seasonal cycle in. We hope to improve this as soon as we have field data for another part of the season.
The limitations in our study due to the missing seasonal cycle are also discussed in section 4.2, as for the snow depth and sea ice thickness.

## Minor Comments

Abstract. Line 17. What about the annual budget increases?
Sorry, but we can not follow this comment. This sentence seems clear to us: The annual budget increases by 20% for a two-week earlier melt onset.

L 18-20. Is this speculation? This has not been proven in the paper.
Since this was rather speculative, we deleted this part in the abstract.

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

P 2925, L 18. 'Obtained' seems like the wrong word.
Obtained is replaced by implemented.

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?

Yes, because of observations only during August. The sentence is reworded.

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.
A brief explanation of all involved studies (TransArc, Tara, SHEBA) has been included at the first respective reference.

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?
We clarified that the previous study by Nicolaus et al. (2012,2013) is only performed for the summer season 2011, without any seasonal cycle of surface properties.

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

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.
We clarified the usage of the nearest neighbor resampling method.

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.
There is no consistent uncertainty for the data product but different approaches given in Lavergne (2011). As sea ice concentration impacts the result linearly (as shown from Equation 2), we did not analyzed that uncertainty in detail with an other sensitivity study but keep it in mind for the relevance for the entire study in the discussion part.

L 10. Which satellite? Lagrangian feature tracking?
We added the information of using satellite data of ice movement information from different sensors; without listening the exact sensors/ satellites.

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

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?
We selected this threshold to be consistent with the commonly threshold for sea ice extent and similar studies. Those marginal ice zones show greatest uncertainties.

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

L 6. Why 'up-scaling model', upscaling from what? Why not 'solar heat flux parameterization' or similar.
We followed the suggestion and changed the section title to 'Solar heat flux equations'.

P 2929, L 17. Explain better what the difference between new MYI and MYI is, and why it is relevant.
We clarified the importance of the additional ice class of new MYI due to crucial differences in surface/ optical properties.

P 2930, L 1. Remove comma after ‘both’.
Fixed.

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?
We rewrote this section and clarified the usage of the inverse behavior of albedo and transmittance in the initial growth phase, based on the studies by Perovich (1996).
We removed the 10cm statement, which we adapted from the Perovich (1996) study. We agree that there is no 10cm thickness threshold. In particular in our study, which is not able to discriminate different thicknesses directly, no such threshold is applied. Hence, this statement is not necessary.

P 2932, L 20. What is the ‘scaling factor’?
The scaling factor is meant as the ratio between tau_b (summer,x,y) and tau_i(summer,x,y). We clarified this also in the manuscript.

P 2933, L 8. Try ‘2011 seasonal cycle of solar radiation: : :’.
Fixed.

L 13. ‘Results’? You mean for validating the parameterization?
The use of 2011 for later comparisons is clarified.

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.
The annual Arctic-wide total heat flux is calculated by applying Equation 3 (Section 2.2). The corresponding reference is added.

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.
We refrain from normalizing our results since comparisons to other studies are getting more complicated. We signalized the differences by obvious different units (and magnitude).

L 23. Most pronounced compared to what? Other monthly increases?
Yes. We added “monthly” to clarify this part.

P 2935, L 2. ‘According to: : :’
Fixed.

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)?
We moved it to the discussion part.

L 11. This is discussion.
We moved it to the discussion part.

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.
Done.

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.
We reworded this sentence as suggested to make the chain of argumentation clear.

L 21. More ponds? Or greater pond coverage makes more sense, no?
Yes! We reworded this part.

P 2937, L 1. 'the impact for primary production is expected to be largest', needs a reference.
We include a reference for this statement (Wassmann and Reigstadt, 2011).

L 2-4. This sentence needs rewording.
The entire sentence is deleted due to an additional subsequent paragraph.

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.
We agree with the reviewer and changed the section title to "Validation" with respect to the following texts.

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 in transmittance between winter, EMO and MO.
We are not sure about the main message of this point. To clarify our statement, we added the coincidence of the beginning of the melt season and the maximal surface solar heat fluxes as reason for the crucial impact of EMO/MO on the transmitted energy.

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?
Indeed, Hudson et al use a combined heat flux. We removed this comparison, because otherwise it would get difficult to compare those numbers.

L 18. 'The main reasons: : :'
Due to rewording the entire section, this is not necessary anymore.

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

L 12-15. I don't believe that your results or discussion support this conclusion, because the sensitivity studies are unrealistic.
Our major revisions and new calculations on the sensitivity studies hopefully conclude this comment.

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

comment 8 above.
This part is adjusted due to the change in the discussion part above, as suggested in comment 8. The summary is now also based on the general statement on internal/bottom melt.

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?
According to Simpson and Paulson (1979), the albedo of open water is estimated as about 0.07. Applying this to the transmittance of the open ocean, we assume a transmittance of open water of about 0.93.
In Table 2 and Figure 2 we also included transmittance values of pond-covered sea ice in winter to illustrate the entire seasonal cycle of the components.

Figure 2.
Separate the two graphs – the top value is missing from the y-axis of 2b.
Fixed.

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?
The top part is cut off because it is obvious (and described) that for the different ice classes just follow their described linear or exponential evolution. In addition, there would be just throughout straight line at 0.93 for the transmittance of open water. We add this information in the caption but do no think that it is necessary to include it in the figure.

Use same scale for two graphs, at the moment it's difficult to compare the two.
Fixed.

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?
We add information to the caption.
There is FYI/MYI during the advanced melting between MO and EFO because this describes sea ice surviving the summer melt and will not completely melt during this season. FYI surviving the melt will turn afterwards to new MYI. The MYI stays as MYI.

Figure 5. These graphs are not clear – increase line width.
Fixed. Besides, we redesigned this figure by combining figure (a) and (b) to only one figure.

Figure 6. Again not clear. Have you assessed the statistical similarity of the two datatsets, e.g. by using correlation analysis? How much of the observed variance is explained by the parameterizations?
No, we did not look into this, we only did some point-to-point comparison here.
We increased line width.