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

Anonymous Referee #1
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This manuscript describes a significant data synthesis effort for estimating the total amount of sunlight propagated through the Arctic ice cover between 1979 and 2011. No new observations are presented. Sensitivity studies address the effects of uncertainties in key parameters in the observational records. The study was done with care and attention to detail, so the results are useful and of interest to the community.

The study has limitations, of course, and some of these limitations are discussed, but others are not really addressed. I wish there had been some discussion of the fact that ice thickness was not included as an independent variable, of course thickness was not explicitly considered because reliable records (with adequate spatial and temporal resolution) do not exist. This seems to be a significant gap in this work. The manuscript is publishable, except I would like to see some acknowledgement and discussion of the role of an ice thickness distribution for the Arctic and some estimate of the uncertainty created by not having this distribution. I have chosen "reconsideration after major revisions" for this reason.
First of all, we thank the reviewer for his/her helpful comments. As he/she might have recognized, some aspects were commented on in the second review, too.
Nevertheless, sea ice thickness is a key parameter. The revised version contains more explicit sections that deal with uncertainties of different data products. In particular we discuss the potential use of sea ice thickness data (section 4.1: Discussion/ Validation), but as the reviewer sais, it is not possible to include sea ice thickness in this kind of study because of the lack of consistent data sets.

My only other general comment is that there are numerous occasions where the English is awkward, to the point of leading to considerable confusion, in this manuscript. I did not attempt to flag each instance. It would be helpful to have someone read it strictly for the purpose of improving the written English.
We have rephrased several passages of the manuscript with the focus on the improvement of the language. We hope that we now meet the expectations in terms of good English expressions and formulations.

Minor Comments

Abstract line 2: "ice is thinner". So why not consider ice thickness? Clearly there is no comprehensive record, but there is very little discussion on the role of ice thickness on the total heat calculations.
We keep the statement here, but the aspect of using sea ice thickness data is now discussed more comprehensively and in more detail throughout the manuscript.

Abstract line 5: "But until now, it is not possible to quantify: : :" It is not clear here what has changed. Why "until now"? It would probably be better to just say "In this study, we quantify: : :"
We reworded this part as suggested.

Line 8: Please qualify the "annual maximum : : : flux of : : :occurs in June, then also matching the under ice : : : flux" This sentence is very confusing. Is that a daily maximum? Do the authors mean "annual maximum"or "maximum annual"? What is matching the under ice flux? Very confusing.
We clarified this part by rewriting to "maximum monthly mean".

Line 12 -13: increase in light transmission? Thru all ice types? This needs to be more specific.
This refers to an overall mean. It is more detailed in the text (section 4.1: Discussion/ Seasonality and trends of transmitted fluxes), but we consider this as detailed enough for the abstract

Line 18 – 20: Is this claim substantiated or speculative? I see nothing in this study that conclusively leads to this statement!
Since this was too speculative, we deleted this part in the abstract.

p.2926, line 1: "multi-year radiation transfer "– what does this mean?
This sentence was rephrased: for the years 1979 to 2011.

2926, 10-11: "Finally, it was possible to derive trends for the years from 1979 to 2011 for radiation transfer through Arctic sea ice." How is this statement credible if meltpond records only go back to 2000? Is it because the changing date of melt onset is significant enough to make the trend significant? This should be explained more clearly.
We kept the formulation here as it is in order not to get too detailed in the introduction. But the issue is discussed more detailed as before in the discussion part (section 4.1: Discussion/ Seasonality and trends of transmitted fluxes) of the manuscript.

Eqn 2: tau_b should be function of (time, x, y)?
We agree about this and indicated the transmittance as a function of time and space.

2929,12-13: "After EMO, melting FYI and melting MYI are introduced for sea ice not surviving summer melt." What is meant by "not surviving"? I don't understand this statement.
We reworded this paragraph to improve the clarity of the different ice types we use for our parameterization.

2929, 16-17: "FYI surviving the summer melt turns into new MYI after week 36 of the year, and furthermore into MYI at the end of the year." This is not clear.
We reworded this section and clarified the differences of new MYI and MYI due to crucial differences in surface/ optical properties.

2930, 1: Surface Heat Budget: : :
Fixed.

2930, 22-25: "However, at MO, the surface albedo of FYI is only about half that of MYI (Perovich and Polashenski, 2012). Since Nicolaus et al. (2010a) calculated a transmittance of 0.02 for MYI for the day of MO, the transmittance of FYI is assumed as 0.04 following the albedo evolution." I don't follow either of these sentences. Please rewrite for clarity.
We reworded this part.

2930, 27-29: "After EMO, it is considered that the annual sea ice retreat in summer strongly impacts the light transmittance. Thus, melting FYI and melting MYI is separated in the parameterization of tau_i ." Not at all clear. Please rewrite.
We added a short argument to improve the understanding of this part.

2931, 1-4: Since melting has an approximately inverse effect on transmittance compared to albedo, we use a transmittance of 0.4 for the last existing sea ice (< 10 cm) and fit an exponential increase between EMO and the last day of melting for the according pixel." Confusing and needs to be rewritten.
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).

2931, 6-8: “After MO, snow is assumed to melt completely within 14 days (Nicolaus et al., 2006) and pond cover fraction increases rapidly until the maximum pond cover is reached at the end of this phase.” Is this from Rosel data? Or an assumption?
The evolution of the melt pond fraction until the end the melt season is described in Nicolaus et al. (2010a). We added this reference.

2931, 12: continues until ice extent minimum? Really? No, I think freeze up starts at high latitude prior to extent minimum.
Correct, this might differ for different regions. We reworded this with reference to the respective pixel, not the Arctic wide extent minimum.

2932: 20 – 21: What 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.

2933. 12: “for ice covered areas only” Does this mean the calculations are normalized to ice area (i.e., per kmˆ2)? Or is it just total accumulated heat for only all ice covered area?
The trends in transmitted heat fluxes were normalized with 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.

2933, 23-24: “From May to June, the most pronounced increase was found for QT (x,y) (to 9.3105 Jm2) and the transmittance (0.054)..” I don’t think this is intended to mean what it says.
We clarified that the presented values represent the monthly increase, which is the strongest one in June.

2933, 25-26: June was the month of the highest QT (20.91019 J) associated with the highest solar surface irradiance over the entire Arctic Ocean (8511019 J). Not sure I believe this. The ponds in June aren’t really that transparent yet, so I think tau_p isn’t the same all summer.
Indeed, we are not able (yet) to use seasonality in melt pond transmittance. 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 of this seasonal cycle are discussed in section 4.2.
However, the results show the importance of the surface solar irradiance, which leads to highest fluxes in June.

2934, 4-6: In July, QT (x,y) reached its maximum 5 of 9.8105 Jm2 resulting from a maximum mean

transmittance of 0.089. The maximum QT (x,y) reached still about 28105 Jm⁻2 with a QT of 18.41019 J. Please rewrite for clarity.
We agree and edited this section.

2934, 6-8: “The different impact of MYI and FYI, becomes most obvious in July. Also continuation of sea ice melt along the ice edge becomes more important for the underice heat fluxes.” Neither of these sentences make sense to me. Please rewrite.
We edited this section.

Curves in fig. 6 are too faint.
Yes, we realized this after submission. It is improved now.

2938, 15 -16: albedo increase? Or decrease?
Yes! Of course, a decrease in albedo is shown.