2nd round review of "Seasonal cycle of solar energy fluxes through Arctic sea ice" by S. Arndt and M. Nicolaus

Editor decision:

A third reviewer looked over the manuscript. He saw the first version but did not have time to submit a review. He has found the manuscript still needs substantial work and I agree with his comments.

The issue of open water was raised again and it has not been adequately addressed in the revised manuscript. For example in the abstract it is not mentioned that this analysis is for ice only and excludes all leads and open water.
We rephrased a part of the abstract to clarify that the analysis is for ice-covered areas only.

The flux through ice is of little interest if it is not related to that through open water. The justification for excluding open water (line 122) is inadequate. I would suggest that you address this issue much more completely and include in your analysis the seasonal changes in the flux though open water and include an open water term in eq 2. What fraction of the flux entering the ocean is through bare ice, ponds, or open water? The discussion of Perovich (2011) (in section 4.1, not 3.2) does not mention open water or the fact that he finds a much stronger trend in the flux through open water than through ice (his fig. 3).
We certainly agree that heat fluxes through open water are of high relevance for energy and heat balances of the Arctic Ocean. Perovich et al. (2011) and Nicolaus et al. (2012,2013) discuss the importance and dominance of heat flux through open water. They show the obvious effect of changing (reducing) sea ice concentration on transmitted fluxes. The effect of sea ice concentration is also discussed in this study, but it is linear since transmittance is 0.93, constant over all seasons. In contrast, our study focuses on changes in physical properties of the existing sea ice cover and its effect on energy budgets. We find that even those regions, which are permanently ice covered, show significant trends. To highlight this better, we decided to scale our trend calculation with the trend in sea ice concentration. However, we rephrased parts of the introduction of the methods (section 2) and the conclusions (section 5). We hope that this makes our findings clearer and it becomes obvious for the reader why this study focuses on the ice-covered part of the ocean.

I sympathize with how hard it is for a non-native English speaker, but clear and concise language must be used. It is not the role of reviewers to correct English usage. Please have the manuscript carefully reviewed for clarity by somebody well versed in English and the science as well.
A native speaker of English (Benjamin Lange) edited the new version of the manuscript. He is also working on physical and biological properties of sea ice, including measurements of spectral transmission and energy budgets. We hope that this version fulfills the language requirements.

2nd round review of "Seasonal cycle of solar energy fluxes through Arctic sea ice" by S. Arndt and M. Nicolaus

Anonymous Referee #1
Submitted: 23 September 2014, received: 30 September 2014

General Comments

The authors have clearly made substantial improvements to this manuscript. It is my opinion that this study should be published. There is no question that there are limitations to the study, but I think the authors have adequately described them. This being said, there are still numerous places in the manuscript where the language is not clear and I find the meaning questionable.

Minor Comments

p2. line 26: "Arctic sea ice has not only decreased…". The amount, or the extent, or the thickness has decreased, not the ice. "The amount of Arctic sea ice has not only decreased…."
We rephrased that sentence as suggested.

2.30: 'sufficiently well'? what does this mean? By whose standard is the quantification sufficient? Maybe just delete these two words.
We deleted those words as suggested.

2.32: maximum monthly mean solar heat flux where? Below the ice?
We added 'under the ice'.

2.33: "…accounting for an equivalent ice melt of approximately 30 cm per month." well, not really "accounting" because there is no attempt here to relate this estimate to an observed ice ablation. I think it would be better to say 'yielding possible ice melt' or 'potential ice melt'?
We rephrased that part as suggested.

2.34: "…96% of total annual solar heat input occurs from May to August…" If this is just heat input through ice and does not include heat into the open ocean then this statement is wrong as it is currently written.
We added "through ice only" to make our statement clear.

2.37: "…increase of light transmission of 1.5% / year"? This is unclear. Which is meant, "increased transmitted light" or "increased transmission of light"? It matters.
That's right! We meant the "increased transmitted light" and reworded this part for a better understanding.

2.42-44: “Continuing the observed transition from Arctic multi- to first year sea ice could increase light transmittance by another 18%.” This is an open ended statement, so it’s not clear by when the 18% could be achieved.
We specified that the evolution towards a FYI-covered Arctic only results in an additional increase of 18%.

3.59: change “various studies showed” to “various studies have shown…”
Done.

3.66: “was possible” to “has been possible”
Done.

3.69: “spatial variability” and “seasonal changes” don’t really belong in this list of specific physical attributes. Spatial variability of what? Seasonal changes of what?
We hope that we understood this comment correctly. Thus, we split this sentence into two, listing surface features and physical aspects first and seasonality and spatial variability in a separately.

6.120-121: It’s fine to exclude open water areas since the authors have now made it clear that they are doing so and why, but the work of Perovich (2007a) should be referenced here.
We added the reference.

7.168: extra “data”
Done.

11.269 – 12.276: I don’t understand this paragraph. What exactly is meant by “inverse behavior”? Is it meant “…increase in transmittance of growing sea ice…”? Why is “aggregate scale” important? Furthermore, I don’t see at all how it relates to Figure 2, where transmittance of 0.4 is not depicted. Also, I don’t see an exponential change between EMO and the last day of melting in this figure. This paragraph needs to be rewritten.
We rephrased this paragraph. The exponential evolution starts actually with the first day of the summer season (phase IV) as shown in the figures.

12.279: Perovich et al. (2002) also commented about duration of snow melt, so it would be worthwhile to consider their estimate as well.
As their work gives a comparable evolution regarding the snow cover, we added this reference.

12.286: The language here implies that the minimum ice concentration is always during phase IV? Ice concentration is tightly tied to dynamics; I don’t understand why it should be tied to thermodynamics.
Indeed, in our purely thermodynamic approach the minimum concentration is during Phase IV, before freeze-onset. The selected approach does not consider dynamic processes and re-location of ice masses (see comments on edge pixels). But to some degree, this is included through daily updates of sea ice concentration fields.

12.287: that undergo comparably small changes?
We refer to temporal changes in optical properties of sea ice, which vary only little during this time of the year, since melt processes renew the surface scattering layer. We re-phrased this and added additional references.

12.288 – 292: Descriptions appear ambiguous. Does "pond covered ice" mean just the pond or the ponded ice cover (as was defined earlier?). Then why do these number get weighted by melt pond fractions?
We agree that the term was confusing. We rephrased this paragraph and put in the transmittances of (pure) melt ponds for FYI and MYI which are then weighted by the melt pond fraction.

15.363: why the word "strong"? Do the authors mean "rapid" snow melt instead?
We rephrased that sentence as suggested.

16.380-383: Fig 4a shows annual average heat input, yes? That's not clearly stated in the figure description here. Also m or cm?
We rephrased this part to clarify the meaning of the figure.
Of course cm is meant; we corrected that.

16.384-385: "The mean total solar heat input per grid cell in the area of the mean sea ice extent was 46 MJm-2." What does this mean?? Per grid cell, but also per m^2? And what does "area of mean sea ice extent" mean?
We rephrased this part. The considered sea ice covered area is the annual mean ice covered area in 2011 for representative comparisons as described in section 2.3 (Methods/ Deriving trends). We added also this link.

16:385-387: "The maximum QT(x,y) occurs at the edge of the marginal ice zone in the Canadian Arctic Archipelago (up to 110 MJm-2 386 /130 cm melt per year) and the East Siberian Sea and Chukchi Sea (up to 80 MJm-2 /94 cm melt per year)." So is this describing what the authors call "ice edge effects"? What is the physical explanation of this?
16.388: What does "excluding ice edge effects" mean?
16.391: Again, I don't understand what the "sea ice edge effects" are?
We agree that the term of "sea ice edge effect" is confusing. Therefore we clarified that we exclude areas characterized by a strong spring sea ice retreat and thus, low sea ice concentration for the presented trend analyses.

17.403-404: where is this increasing variability be seen? Is it just being eyeballed off the figure? It should be quantified.
We added the reference to the figure to make this point clear.

18.443-444: "Speculation of even greater pond coverage might even increase the trends." This doesn't make sense to me. If the pond fraction was even larger than assumed prior to 2000, then seems this would decrease the trend. The third sentence in this paragraph doesn't follow at all.
We totally agree that this was a mistake of us. As that entire paragraph of three sentence is independent of the other paragraphs and does not have a high relevance at all, we decided

to delete it.

19.448-462: I am really having a difficult time understanding this paragraph. In particular, what is the “ice concentration effect”? Where does “annual trend of +-1.1% a-1” come from? It does not follow why this comparison “emphasizes the dominance of the albedo feedback mechanism”. This paragraph needs to be rewritten.
We understand this problem and rewrote the paragraph

24.587: adding up to > 100%? Figure only goes up to 50%.
We adjusted the scale of the figure and described the end of the scale with “>50%”. Since we want to keep details, the scale is not extended to 100%.

26.622-623: How is 63% calculated from 1.5%/year for 33 years?
This results from: 1.015^33=1.63 => 63% increase

26.628-630: How is this known? Heat fluxes through the ice haven’t been compared here with heat fluxes through open ocean / retreating ice concentration!
We reformulated this section to make it clearer and find a better comparison of fluxes through sea ice and into open ocean.

26.632: What is meant by “access heat”?
We reworded that.

2nd round review of "Seasonal cycle of solar energy fluxes through Arctic sea ice" by S. Arndt and M. Nicolaus

Anonymous Referee #2
Submitted: 10 September 2014, received: 30 September 2014

General Comments

The authors have made a considerable effort to improve their manuscript, taking into account both reviewer's comments. I therefore recommend that the manuscript should be accepted following minor revisions.

Major Comments

The English is still awkward. This was pointed out by Referee #1 in their original review, but I don't feel that it has been adequately addressed in the author's resubmission. I reiterate that it would be useful for someone to edit the manuscript purely to improve the written English. This isn't only an issue in a few places, but throughout the manuscript.
We agree with all reviewers and could hopefully improve the understanding, clearness and language by the help of a native speaker from our institute.

Minor Comments

Abstract.

Throughout the abstract you should explain your points fully. Often it is very hard to understand what you are trying to articulate (examples are given below). Remember, people are going to read the abstract before deciding whether to read the full paper. It should therefore be simple and clear, otherwise it will put off potential readers.

Line 30. Remove 'sufficiently well'. Remove 'here'.
Done.

Line 32-33. Confusing sentence. Is this '30 cm ice melt per month' only for June? Or an average of 30cm per month for the entire summer? If it's the former, maybe just remove 'per month'.
The mentioned potential ice melt is valid for June only. We rephrased this.

L 37-38. What does this sentence mean? 63% more bottom melt occurs now than it did 33 years ago? Where has this figure come from? What is 'potential' sea ice bottom melt?
Considering an annual increase of 1.5% over 33 years (1979 to 2011) results in a 63% increase: 1.015^33=1.63
Potential bottom melt refers to the potential amount of sea ice that could be melted with the amount of energy, if all would be used for melt only (latent heat). However, we can not directly quantify melt, since also heat advection and warming need to be considered. But this is the potential of the energy. Generally speaking, this is often easier to imagine and to compare than energy units.

L 42. Like I said before, what about the annual budget is it that increases? The whole budget could increase (more energy gains, more losses) without a net surplus or deficit. I think what you are trying to say is that the surplus of radiative energy available to the ice increases by 20%.
We clarified that part as suggested.

For the final two sentences of the abstract you should report either changes in light transmittance or changes in the surplus/deficit of energy. I recommend the latter.
We restructured this part for a conclusive end of the abstract.

Manuscript.

L 164-165. This is from Rosel et al? It is the same std dev for both FYI and MYI?
We added that the mentioned mean standard deviation is valid for both ice types. It is calculated from the provided data set.

L 464. A validation is typically performed to test the reliability of the method and is therefore reported before the major results, not in the discussion. Since this section is more of a comparison between your predictions and limited observations, you may want to change this section to 'Comparison with Field Observations' or something, instead of validation.
We agree that it is possible to include the comparisons with field and the sensitivity studies in the result section. Nevertheless, we think that we would have to rewrite the entire section for a consistent overall picture. Therefore, we decided to keep the structure in the results and discussion section as it is but changes the title of the former “Validation” section to “Comparisons with field data”.

2nd round review of "Seasonal cycle of solar energy fluxes through Arctic sea ice" by S. Arndt and M. Nicolaus

Anonymous Referee #3
Submitted: 30 September 2014, received: 30 September 2014

General Comments

The science contained in this manuscript is publishable, and should be published. However, the manuscript in its present form is very difficult to read, and suffers from both language and organizational shortcomings. I also raise some outstanding questions regarding the science. In my opinion, the article still needs quite a bit of work before publication, and so I suggest major revisions.
We highly appreciate the work of this (third) revision. We found similar aspects as in the other two revisions. Here we try to comment on all of them separately, but also refer to our replies to reviews 1 and 2.

Major Comments

1. Both of the previous reviewers suggested that the English is awkward in more than one place. This remains a problem in this version of the manuscript. The authors really need to have a native English speaker correct grammatical and punctuation mistakes, as well as suggest improvements for clarity. The authors use many introductory phrases and transition words inappropriately and the manuscript as a result is difficult to read for a native speaker who can interpret what is written. It will be even more confusing for non-native English speakers. Much of it can be simplified and made more clear.
We agree with all reviewers and could hopefully improve the understanding, clearness and language by the help of a native speaker from our institute.

2. Open water is not specifically treated here. However, the use of sea ice concentration implies an implicit treatment of fractional open water coverage. Presumably in cases where the sea ice concentration is much less than 100%, the transmittance of light through leads will also dominate the light conditions, as well as the energy absorbed in the water column. It seems there should be a discussion on this point - both in terms of the transmission levels as well as the absorbed energy and therefore melt potential. This presumably would also swamp the signal of changes through the ice itself in low sea ice concentrations. Are trends in transmittance useful (or even appropriate) in areas where the ice has retreated for at least some months of the year? A comparison (as in Nicolaus 2012/2013) of the solar absorption in open water may be useful.
We totally agree that heat fluxes through open water are of high relevance for energy and heat balances of the Arctic Ocean. Nevertheless, the focus of our study is the analysis of changes in physical properties of the existing sea ice cover and its effect. Studies by Perovich et al. (2011) and Nicolaus et al. (2012,2013) show the clear dominance of heat fluxes through open water in the transmitted heat signal (and its trend), and therefore, the obvious effect of sea ice concentration on those fluxes. Thus, we decided to scale our trend calculation with the trend in sea ice concentration. Nevertheless, we added these aspects

more explicitly (starting from the abstract) in order to make this aspect clearer.
To make these points more obvious and clear to the reader, we add rephrased parts of the introduction of the methods (section 2) and the conclusions (section 5).

3. The ERA-Interim data is used for the incoming surface solar irradiance. How do changes in incoming solar affect the budget and/or trends? The authors could do a direct assessment on the incoming solar field (or there may be literature on it already), or they could evaluate the results against a 'clear-sky' solar irradiance. The latter would most clearly get at changes induced by the ice itself, as compared to changes mitigated by changes in incoming solar.
Of course, also the solar surface irradiance has a major impact on the solar heat flux under ice. Considering equation (1) and (2) it is shown that the incoming radiation impacts the transmitted heat flux linearly. Therefore, we decided to not analyze changes in the incoming radiation explicitly. However, we discuss the role of solar irradiance in terms of uncertainties in the section 'Sensitivity studies'.

4. I also agree with the previous reviewer's comment who suggests a test of the parameterization against the melt-pond fractions in Flocco 2010 or Flocco 2012. Though I understand the author's desire to keep this an empirical result based on observed quantities rather than modeled, I would argue that the use of reanalysis data for the primary input dataset is already a model result, and the parameterization is also missing the ice thickness and/or snow depth quantities that might make the whole exercise more straightforward. A comparison to the modeled pond result which may have some consistency between ice thickness, snow depths, and pond fractions might strengthen the point of the authors.
We would like to refer to our replies in the first round of revisions: We think that an implementation of model data would miss the focus of our work. Therefore, we aim to take as much observational data as available. The ERA-Interim (model) data are used, because no other comparable data set is available and even most model studies are forced with (similar) reanalysis data.
We agree that the modeled melt pond fraction by Flocco et al. (2010,2012) can be considered as an alternative approach. Therefore, we extended the discussion on alternative approaches by this aspect.

5. L88-102 The description of the contents of the paper still does not capture what I see as the main story. I would suggest that the authors make a simple summary here of what they accomplish in this paper. Though a matter of semantics, it is not really a 'new method;' rather it is an estimate of an annual budget for shortwave radiative transfer through sea ice and into the water column, based on on existing (and incomplete) data sets.
The 'new' part of the parameterization involves the melt-pond fraction only, correct? Or do the authors include the classification of the ice surface conditions? I'd suggest that it is one and the same, and might emphasize not that it's 'new' but is an extension of Nicolaus 2012/2013 as a first attempt that to quantify changes through the annual cycle.

One suggestion for this paragraph might be:

"In order to improve the understanding of the ongoing change in sea ice conditions and 89 the associated impact on the partitioning of solar energy, we provide an estimate of the monthly shortwave radiative transfer through sea ice for the entire Arctic for the years 1979 to 2011. We use a definition of 6 types of sea ice over the annual cycle, define 6 distinct time periods of insolation conditions, and include the temporal and spatial variability of melt ponds to extend and generalize the up-scaling method of Nicolaus et al. (2012) and (2013)."

The rest of the details in the paragraph made it seem like the authors were accomplishing a list of 4-6 different things. Again it can be kept much simpler and clearer.

We thank the reviewer for this detailed comment on the introduction of our manuscript and involved the suggested phrasing for a much better understanding of our presented work.

Organization:

Methods: I suggest introducing the parameterization first (Sec 2.2), followed by the datasets.
We agree that it makes sense to put the data set description in the end of the method section.

I also suggest introducing eq 2 first, and introduce eq 1 as the exception due to the lack of data for melt ponds.
We think it is useful to have at first Eq. 1 followed by Eq. 2 as Eq. 1 illustrates the general connection between the input variables.

It's not clear how the transmittances in fig 2 for melting MYI and melting FYI relate to the terms in Eq 2.
The transmittance of melting sea ice is treated similarly as for all other ice classes. Dependent on the point in time in the melting processes the transmittance value on the exponential curve is chosen.

It's not clear why the authors mention open water in Fig 2 caption, when it is not included in the equations 1 or 2 (though it probably should be).
We added the transmittance of open water for completeness but we agree that this might confuse due the neglect open water fluxes.

I suggest that Sec 4 not be labeled its own section, and instead become subsections for section 3. 4.2 should not be labeled 'Validation', since it is not. Perhaps 'Comparison with direct observations'. Any further discussion can be wrapped into the last section.
We agree that it is possible to include the comparisons with field and the sensitivity studies in the result section. Nevertheless, we think that we would have to rewrite the entire section for a consistent overall picture. Therefore, we decided to keep the structure in the results and discussion section as it is but changes the title of the former "Validation" section to "Comparisons with field data".

Minor Comments

Abstract: L42 - The annual budget of what increases by 20%? One of the reviewers also raised this point. I assume you mean the annual transmittance of solar radiation to the water column increases by 20% given a two week increase in melt season.
We rephrased this sentence as suggested.

Fig 1. The text discussion of the 6 ice types is at odds with the figure diagram with the 6 labeled seasons. Please clarify in the text and figure.
Sorry, but we do not understand this comment. There are different ice types, which are named, as well as different seasons with their names and numbers (Phases).

Fig 2. What happens to the MY transmittances after season IV? What is the maximum transmittances that the curves approach? Including them by a discontinuous scale might be a way to represent it.
To clarify the maximum transmittance of 0.4, we added another sentence in the description of melting FYI/MYI.
We did not include a discontinuous scale but rather cut off the top part because we think the linear or exponential evolution of the different ice classes is sufficient described in the manuscript.

Why does the MYI split into a 'melting MYI' and a constant MYI in season IV? (Same for FYI?) It is not explained what the significance of this is.
The distinction between "surviving" and melting FYI and MYI (and reason for this) is given in the section Methods/ Definition of sea ice types and Methods/Transmittance of pond covered sea ice. To clarify the reason of distinction by the expected differences in optical properties for the mentioned ice classes we rephrased the corresponding part in the latter section.

Trends: Given the assumption of constant melt-pond coverage for prior to 2000, it is difficult to see how trends can be meaningful for 1979-2011. If the authors want to include these trends, it is suggested that they be reported also for the period 2000-2011. It is not clear why pre-2000 values are set to 2011 mean values, instead of to 2000 mean values.
We think it is not useful to use a mean value for melt pond coverage of only one year. That's why we decided to take the mean value from 2000 to 2011 into account. During previous analyses it also became apparent that we do not see any jump or any discontinuous trend evolution between known melt pond fractions and our calculated mean values (before 2000).

The problem with comparison to the Tara drift measurements for August (L 481-484) is not clear - is it only in the water column? Or within the ice? Is the depth of the sensor too far from the ice interface?
We clarified that the mentioned biological processes are present within the ice and the underlying water column. The sensor was hanging about 1.4 m under the ice .