3rd round review of "Seasonal cycle of solar energy fluxes through Arctic sea ice" by S. Arndt and M. Nicolaus

Editor comment
Received: 04 November 2014

General Comments

I agree with the referees that the manuscript may be published after the minor corrections suggested by them are made. The writing is much clearer now and the referees appreciate that.
My point about the justification for excluding open water (L124) is that the flux through open water is also crucial to physical and biological processes, often more so. You would make it clearer to the reader to acknowledge this at the beginning and point out that although there are important trends in the ice concentration and the resultant solar fluxes to the ocean in open water, you want to focus on an important additional source of variability, the properties of the ice cover, that could be overlooked if ice concentration alone is considered.

Dear Ron,
We thank a lot for this positive and constructive comment. As suggested, we include sentences on the focus of the study on fluxes through sea ice only already in the introduction.

3rd round review of "Seasonal cycle of solar energy fluxes through Arctic sea ice" by S. Arndt and M. Nicolaus

Anonymous Referee #1
Submitted: 03 November 2014, received: 04 November 2014

General Comments

This manuscript is dramatically improved. It reads much more clearly. The authors obviously made a focused effort to address the concerns raised in the reviews. I have read this revision in its entirety and still find sentences that need attention. However, I think these should be straightforward to edit and can be taken care of easily.

We thank the referee for the last minor comments for improving our manuscript.

Minor Comments

172: I don't think "EMO" has yet been defined.
You are right! We added the explanation.

267: "begin of" "beginning of"
Done.

429: "amounted to"… how about "was estimated to be" instead?
Done.

447-448: "This increase in bottom and internal melt is affecting sea ice mass balance." Bottom melt yes, but I am not aware of documented increases in internal melt. The authors need to cite a reference for this statement.
We agree with the comment and focused in the sentence on the documented bottom melt.

467: "through ice-covered sea ice"?
Through ice-covered ocean was meant here. We changed that.

469-472: These two sentences are still not clear. I think the authors are trying to say that since ice is decreasing, heat through the ice is also decreasing, but I am not sure. These two sentences need to be rewritten so the point is clear.
We reworded this part.

576-581: These three sentences are particularly confusing and need to be rewritten.
We reworded this part.

587-588: This sentence makes no sense. Do the authors mean "…studies the effects of altering timing and duration on the heat"?
Yes, we did. We reworded that.

3rd round review of "Seasonal cycle of solar energy fluxes through Arctic sea ice" by S. Arndt and M. Nicolaus

Anonymous Referee #2
Submitted: 27 October 2014, received: 04 November 2014

General Comments

In this review I have paid little attention to minor details of the language, grammar, sentence structure and content, as these were covered in previous rounds. Instead, I provide general comments on the study, with a focus on the major points raised by Reviewer 3.

I still believe the manuscript, in its current form, can be accepted following minor revisions. There is however a caveat. The conclusions of the study are limited by the underlying premise: that the heat flux parameterization is based on readily available remote sensing and reanalysis data, and is therefore heavily simplified. The authors can choose to include a more sophisticated treatment of the relevant physics (i.e., the snow depth, ice thickness, seasonal cycle of melt ponds), or not. However, if they do not, then their results are limited by the simplicity of the parameterization and the reliability of their conclusions is questionable. In other words, the science still merits publication, but the conclusions are not as reliable as they could be if a more sophisticated approach was used.

The same can be said of the fact they purposefully ignore open water fluxes. As Reviewer 3 pointed out, what is the relevance of the flux through ice if it is not related to that through open water? How can the author's calculated heat fluxes be compared with results from other studies or used in a predictive capacity?

In my opinion, the study would benefit enormously from an improved treatment of the relevant physics, especially ice thickness. I also think the fluxes should be calculated separately for both ice and ice plus open water. The authors can then discuss trends in fluxes through the ice only, which have a physical relevance, and for the entire Arctic basin, which have a more practical relevance. As I have said, the authors can choose not to make these revisions, but their conclusions will consequently have limited scientific relevance.

Major Comments

1. The English has been improved considerably in this version.

2. I agree with Reviewer 3 that the study would provide more relevant results if the flux through open water was included. The editor recommends adding an open water term to eq. 2, while the authors argue that the focus of their study is specifically on the impact of the evolving sea ice cover, not the open water fraction, on the heat fluxes. Both arguments are valid, so I would recommend the authors revise their parameterization and report fluxes

for the ice only AND for ice plus open water. In this way the authors can discuss the impact of evolving sea ice physical properties (the main ones appearing to be the date of melt onset and the ratio between FYI/MYI), but ALSO the impact of evolving sea ice properties and changing (increasing?) open water fraction, on the heat fluxes. The editor asks what is the fraction of the flux entering the ocean through bare ice, ponds and open water? This is a very important question, which the authors could provide an estimate for, and by doing so add a significant conclusion to their study.

We thank the referee for another intense review and the suggestions of improvements of our manuscript. Also, we certainly agree with the referee that also fluxes through open water are highly relevant. Nevertheless, we still think that including studies and discussions about additional fluxes through open water would change the focus of the manuscript drastically. The main focus of the manuscript is meant to be on the changing physical properties of sea ice and its effect on the mass and energy budget of Arctic sea ice, but not on effects of the ongoing sea ice retreat in the area. As suggested by the editor, we add already in the introduction the focus of the manuscript on the analysis of fluxes through sea ice covered areas only.
In addition, we agree that it would be great if it would be possible to include more physical basis in terms of the use of sea ice thickness or snow depth data. However, those data are not available yet. There are some data, e.g. from IceSat or CryoSat-2, during the most recent years, but no data are available that allow the discussion of trends, long-term, or seasonal changes. This is why we selected the presented approach. We did include a section on "limitations" in the revised version. From our point of view, this is the best we can do at this stage. However, we encourage any further development of the method as well as comparisons to model simulations. In turn, also the use of model results for such an up-scaling is not a reasonable option for us in this approach.

Minor Comments

Abstract.

L 44. Remove 'only'
L 45-46. Remove this sentence. It's not needed.
We agree and removed both suggested parts.

Manuscript (these comments were included in my previous review, but seem to have been missed out).

L 542-559. These comparisons are fairly weak and add very little to the paper. You should consider removing them.
We think it is worth to keep this paragraph, similar to the statement above, this is the best we can do at this time. We see the benefit of this comparison in showing the fitting range of our results compared to other field studies.

L 561-581. These paragraphs are not validation or a comparison with results, more a discussion of the limitations of the parameterization and possible improvements when new datasets become available. You should consider creating a new 'limitations' section for this material.
See comment above. But we certainly see the point and agree with the referee. However, we hope

that we were able to accommodate this comment though the addition of the a new section "Limitations".

Figure 2b. The 'melting MYI' curve appears to go back in time at the end of Stage IV.
Actually, there was a mistake in the set-up of the figure. We corrected that.