Review of Version 3 “Seasonal cycle and long-term trend of solar energy fluxes through Arctic sea ice” by S. Arndt and M. Nicolaus

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.

Minor Comments

Abstract.

L 44. Remove ‘only’

L 45-46. Remove this sentence. It’s not needed.

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.

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.

Figure 2b. The ‘melting MYI’ curve appears to go back in time at the end of Stage IV.