The Cryosphere Discussions

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Comment on tc-2021-245

Anonymous Referee #1

Referee comment on "Influences of changing sea ice and snow thicknesses on simulated Arctic winter heat fluxes" by Laura L. Landrum and Marika M. Holland, The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-245-RC1, 2021

Review of "Influences of changing sea ice and snow thicknesses on Arctic winter heat fluxes"

By Laura Landrum and Marika M Holland.

Understanding ongoing Arctic change is high on the international agenda. Fluxes of heat from the ocean-ice towards the atmosphere are very central for understanding how the Arctic will develop in the future, and "Arctic Amplification (AA)" has triggered interesting discussion among climate scientists for some years now. I think the authors have picked an interesting problem and have done nice work on the analysis. The paper is well written, but it is also quite short. Some interesting questions did pop up during the review, and I think there should be room for more science in a paper in the Cryosphere than in the present version. This is why I have selected "major" revision. The science that is present would only require "minor" revision, but it should be extended.

There is in my view no good reason to break our publications down into "least publishable units", especially when there are relevant processes to sort out.

One general comment is the selected sub-area of the Arctic Ocean. This leads to a quite strong bias towards the Canadian Basin and the Beaufort Gyre (68-80N is only included from 100E to 243E). I understand that the sea ice cover and changes in related heat fluxes are fundamentally different in the "Atlantic" domain, but this is also an interesting contrast. In Fig. 2 e) the contrast shows clearly. But where in the Arctic has AA been the largest in recent decades? Exactly – on the Atlantic side, with the (excluded) Barents Sea as a "hot-spot" area with 3-4 times the global warming (Lind et al 2018). Taking the results here on face value I think would suggest that the future maximum AA would not be in the Barents Sea, but perhaps move north as the sea ice retreats as suggested by Shu et al (2020). I think also including the Atlantic side down to 68N, and looking at the contrasts would be super interesting. You might state that there are no "conductive fluxes" because there is little sea ice. That is true, but the changes in "winter heat fluxes" are interesting in their own regard.

There are also a moderate number of specific suggestions that I would like the authors to address for a final more substantial version of the paper.

Moderate suggestions:

There is a general lack of comparison towards observations. While no observations exist for the future, there are plenty that observed differences in conductive heat fluxes over a variety of ice types and thicknesses. Some observations should be cited, just to confirm that the simulations are in the right ballpark. A good set of detailed observations while we wait for MOSAiC analysis, was the NICE campaign, as utilized in Duarte et al. 2020.

The role of snow. As suggested below in more detail, the paper should be substantiated by presenting results on why there will be less snow on the sea ice in the future.

Open Water. The good point about sea ice heterogeneity largely misses one important aspects; open water. There are a few results presented, but how large portion of open water is "prescribed" or simulated can easily dominate the winter fluxes and total ocean heat loss. Some models sets a minimum open water fraction in the range 2-10%. Prescribing a large open water fraction would probably dominate over using just one ice class, which is the what "a mean gridcell value" essentially means. You should explain how this is done in CESM too.

Specific comments:

Abstract: Also very compact. Hopefully you can add more on a properly extended "Atlantic" side of the Arctic Ocean and comment/speculate on where future AA would be strongest. Also your best "guess" at why the snow is decreasing would be a nice addition to your findings. One major thing missing in the abstract is to state clearly that all results are simulations, and that the CESM is taken as "truth" without any evaluation.

Introduction (Line 21-25): You should include one sentence on the seasonality of AA – which is greater during winter. This guides us towards the summer ice-albedo feedback being less important in my view. You should also state clearly that this ice-albedo-feedback is a summer only phenomena.

Equations have a strange “black box” around them in the downloaded PDF version. They lack numbering, and the sub-scripts are also not good, f.ex. for h_”eff” in Equation (2).

Line 118-122: Include some more analysis and discussion on open water around here.

Line 128: Geographical area. Please indicate the selected area properly in the Fig. 1 b). It is NOT possible to see the longitudes in the figure. You cannot term the area outside your box for “marginal seas” either, you have excluded large areas of the winter sea ice covered Arctic Ocean proper; The Barents Sea and the Greenland Sea. The official southern limit of the Arctic Ocean on the Atlantic side is the Greenland-Scotland ridge, so also the Norwegian and Iceland Seas are sub-seas of the Arctic Ocean, despite having little sea ice cover (IHO, 1953).

Results (line 150-153): You should explain the difference between the changed ‘conductive’ heat flux (9 W/m2) and what you term the ‘ocean-ice’ heat flux (<1 W/m2). I think the essential difference is that you do grow more ice, and only to a small degree is the ocean cooled additionally.

Line 158: The changes in snow cover are interesting. Please include a figure, and also explain why there will be less snow on Arctic sea ice in the future. I actually thought that there would be MORE precipitation??? But perhaps it is due to warming, and more snow coming down as rain? Or is it that more snow land in open water, as freeze-up is delayed? Or does the open water fraction become much larger so more of it blows to sea?

Line 162: “Central Arctic Ocean” is not clear here. To me this name describes the region you have selected; north of Berings Strait and north of Svalbard. Later you seem to define it to be 80-90N. NSIDC uses ‘Central Arctic’ for this region, just be clear.

Line 182: Again here the open water fraction is missing, I think this is as important as getting the thin ice fraction correctly.

Line 191: Heading should be bold.

Line 192-199: Here there are open water fraction results, at least indirectly (SIC 90 – 98%). I think you have space and interest in investigating these changes as well. Contrasting with the areas further south is very interesting. For example, have the areas that lost the winter ice sooner warmed as much as the ‘Arctic Basin’ will between 2060 - 2070? This is what Onarheim et al (2018) postulated as their “winter mode”.

Line 212: Here you use the term ‘Arctic Basin’ which I think is very clear; it is the deep part of the basin around the North Pole approximately south to 80N. I think you should use this term instead of “Central Arctic Ocean”.

Line 225: missing and “, and” after CESM here?

Line 235: I think “and” at the end of the line should be “for”?

Figures are generally nice. I find Fig 3 a) redundant. You do not need to show both the absolute values and the anomalies.

Suggested new References:

Duarte et al (2020). Warm Atlantic water explains observed sea ice melt rates north of Svalbard. JGR, https://doi.org/10.1029/2019JC015662

Shu et al. (2020) The poleward enhanced Arctic Ocean cooling machine in a warming climate, Nature Comm.

Lind et al (2018) Arctic warming hotspot in the northern Barents Sea linked to declining sea-ice import, Nature Climate Change, https://doi.org/10.1038/s41558-018-0205-y

Onarheim et al (2018). Seasonal and regional manifestation of Arctic sea ice loss. J Clim

IHO (1953) International Hydrographic Organization, Limits of Oceans and Seas, Special Publication 28, 3'd edition.