Influences of changing sea ice and snow thicknesses on simulated Arctic winter heat fluxes
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We thank the reviewers for their helpful and constructive comments. Both reviewers point out that the manuscript could be significantly strengthened by adding more content and context through comparisons in three areas: 1) observations, 2) other models, and 3) other Arctic regions that are experiencing changing wintertime heat fluxes due to sea ice concentration losses.

We have expanded the paper and added more context in these three areas. We make more comparisons with observations, and published results from observational studies. We compare recent results from both the most recent versions of the CESM (the CESM2) as well as other modeling studies for context. We added two figures (and removed another) and discussion comparing the results we have presented for the high latitude Arctic ocean to net surface heat fluxes, sea ice concentration changes and surface temperature changes in regions of the North Atlantic that are already seeing very large changes in sea ice and temperatures particularly in the fall, namely the Barents and Kara Seas. In addition we expand our discussion of the role of snow on sea ice and explain why snow on sea ice is expected to decrease in a warming world even as precipitation increases, and how much remains to be explored w.r.t. the distribution of snow on sea ice and how that impacts surface heat fluxes.

More specific responses to the reviewers are given below.

Reviewer 1

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 Beufort 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.

Our focus is primarily on wintertime surface heat flux budget over ice-covered areas, and hence we have presented results from this region. Changes in conductive heat fluxes due to thinning sea ice dominate the surface heat budget until the sea ice thins enough to subsequently start retreating. As sea ice concentrations fall, the surface heat flux quickly becomes dominated by fluxes from exposed open water.

We've added a new section (3.2 Relative contributions of conductive heat flux changes to total surface heat flux changes in a warming climate), two new figures (Figures 4 & 5) results and discussion from regions that are experiencing wintertime sea ice loss (or will sooner than the high latitude Arctic ocean) to supply context, broaden the discussion and expand the paper. The new figures (Fig 4 & 5) how maps and regionally averaged timeseries of net, ice and ocean surface heat fluxes, sea ice concentrations and Arctic Amplification along with discussion of regional differences (Barents Sea, Kara Sea and our original area of the Arctic Ocean). Although the highest rates of AA are in October in the Chukchi Sea (Chung et al., 2021), the North Atlantic currently shows the highest AA on an annual or cold-season basis and is a location of rapid changes and tremendous oceanic variability – and we've expanded our discussion and results to include these two seas in the North Atlantic. We also discuss how AA is non-stationary and may increase in regions experiencing sea ice loss before decreasing again as the region becomes ice free (e.g. Holland & Landrum, 2021).

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.

We are responding to these broader suggestions by expanding analysis and discussion to include the Barents and Kara Seas and elaborate more on the CESM, improve our explanations of the simulation of sea ice in the CESM and include more comparisons with other models and observations.

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.

This is a good point. The results in the original submission are based on an ensemble of simulations from one climate model. We include more comparisons with observations when describing the model (section 2.1) and also in our discussion (e.g. lines 358-365), and cite more references.

We provide additional information on the differences in snow simulated within different models as well as comparisons with observations. Snow distributions in the CESM-LE (used in this submission) tend to have a larger range than those that seen along the Operation IceBridge surveys and higher variabilities, values and persistence than the newer version of the model, the CESM2, which tends to underestimate snow depths compared to those inferred from satellite freeboard estimates (Webster et al., 2020). Sea ice in the CESM1 is also more persistent than in the CESM2 and may contribute to some of the differences in the snow depths. We will add some of this discussion in the paper to add context to observations as well as some discussion of how our results might differ in other models. We will also discuss how field campaign data (including new MOSAiC data) can enable better quantification of observed snow depth distributions.

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.

Yes, we've now added further discussion of snow, the role of snow, and the distribution of snow (in addition to the comments above on snow distributions), including previous work that has indicated that in observations (Webster et al. 2014) and projections of change (Hezel et al., 2012) snow on sea ice declines due to the loss of a platform for accumulation in the fall months.

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.

The CESM does not prescribe open water within its ice model (or a minimum lead fraction). Sea ice is allowed to grow and decline based on both thermodynamics and dynamics. Sea ice open water formation is simulated and affected by ice dynamics and deformation and by thermodynamic processes like basal and lateral melting. The resulting concentrations of sea ice can range from exactly 0.0 to 100.0 % (although values of 100% are rare due to lead formation). This is true in the discrete thickness layers as well – the concentration of sea ice within a given thickness category ranges from 0-100% depending on thermodynamics, dynamics, and the transfer of sea ice from one thickness category to another.

We've added information in the model description to better explain the sea ice model within CESM and to clarify that the open water fraction is not prescribed but instead is simulated and affected by a number of factors.

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.

We have expanded the main body of the submission to include more comparisons with observations and discussions about how these results may differ depending on the model and mean state, and comparisons to changing fluxes on the "Atlantic" side of the Arctic. We've added to the abstract, have included "simulated" in the title and expanded model comparisons to other models and observations throughout the document.

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.

Yes. We added more discussion of AA including seasonal and transient nature in the new section 3.2 in the manuscript.

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).

We have changed the equation .

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

We've expanded our discussion of AMIP, PAMIP simulations and added a statement (line 141) clarifying that these runs specify open water and sea ice. We further discuss changes in net heat fluxes due to thinning sea ice and snow vs open water in the new section 3.2.

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).

The regional definitions we use are those used by the NSIDC in regional masks of data and discussed in Parkinson and Cavalieri 2012, Cavalieri and Parkinson 2008, and Parkinson et al. 1999 and we now state and reference these. We include another panel in Figure 5 showing Kara, Barents and Arctic Ocean regions. We removed "marginal seas" and show comparisons (for net surface heat fluxes as well as ice cover and AA) between the winter ice covered area of the Arctic Ocean along with those of the Barents and Kara seas to create better context for the results of this paper.

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.

The ocean heat flux in this figure refers to the turbulent heat flux between the ocean and the overlying sea ice and indicates that the increases in conductive heat flux through the ice are not due to increases in ocean-ice heat flux due to warming of the ocean below the sea ice. We rephrased our description in the text to better clarify.

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?

Precipitation is expected to increase in a warming world – including in the Arctic. The depth of snow on sea ice, however, is expected to decrease (across many model simulations). The primary reason for this is that even though there will be more precipitation, sea ice will be forming later in the fall and thus a later start to the accumulation of snow on the sea ice as the snow will initially fall into the ocean rather than on sea ice (e.g. Webster et al. 2014; Webster et al. 2018). This mechanism has also been found in projections of snow on sea ice (Hezel et al., 2012). In addition, as the temperatures warm, the rain/snow season will increase/decrease (e.g. Webster et al., 2021). We've expanded our discussion about snow to include this and these references. We show maps changes in the effective thickness (h_{eff}) in the main paper (Figure 3) and maps of changes in the components of h_{eff} (SIT, h_s and $k_{ratio}*h_s$) in Supplemental Figure 3.

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.

See above comments re. regions, maps, region names.

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

See above comments.

Line 191: Heading should be bold.

Done.

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".

We will add a section (3.2) comparing wintertime net surface heat fluxes over the (largely) ice-covered Arctic with the Barents and Kara Seas – regions that are experiencing sea ice loss. Sea ice loss in these seas leads to greater heat fluxes from open water to the atmosphere – and these regions serve as good examples of how the net surface heat fluxes from the ocean become dominant contributors when sea ice concentrations are decreasing. They also help show how when sea ice concentrations remain high (typically above ~90%) and change very little, sea ice thicknesses then contribute more to changes in net surface heat fluxes than the ocean (e.g. new Figure 4). Furthermore, amplification of wintertime regional surface temperatures compared with global averages show slight increases in the high latitude Arctic Ocean whereas the remain about the same or start decreasing over the Kara/Barents seas in the latter half of the 21st Century. We mention this, and add discussion about Arctic amplification, it's seasonality, regionality, and anticipated changes in AA over the 21st.

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".

We will clarify regions, names, etc. (see comments above).

Line 225: missing and ", and" after CESM here?

Yes, done.

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

Yes, changed.

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

Agreed. We eliminated one of the panels in Figure 3 (now Figure 6) and show just the absolute values.

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

Reviewer 2

This study uses an older version of the NCAR climate model to project an increase in wintertime conductive heat flux through the Arctic sea ice of 7-11 W/m2 by mid 21st century due to ice thinning and in spite of surface warming which would favor a reduction in this flux. The implications drawn from this are that atmosphere models, used for climate change, should specify sea ice thickness changes and that a representation of the sub-gridscale sea ice thickness distribution is needed for accurate representation of the effect. The paper is clear and convincing. I have several suggestions for providing additional context and strengthening the conclusions:

1) *The increased conductive flux is consistent with the finding from Keen et al (2020) of increased wintertime basal sea ice growth -- the heat produced by this growth being balanced by the conductive flux -- even as the conductive flux is reduced during the rest of the year. Keen et al find a large intermodel spread in the winter basal growth increase encompassing no increase as a possibility. The current study, which uses a single model, may underestimate the uncertainty range on the conductive flux increase. One way to connect the results of the two studies would be to perform the Keen et al basal growth analysis with CESM1 (CESM1 did not participate in the Keen et al study) and compare to the results of that study. This would give some idea of where the CESM1 ensemble fits into the larger multi-model range.*

In the most recent version of the CESM2, the sea ice model incorporates a new "mushy-layer" prognostic salinity profile with sea ice as a combination of frozen water and salty brine. This mushy-layer representation results in thermodynamic changes within the sea ice including heat conductivity, freezing point calculations, porosity, melt pond drainage and snow-to-ice conversion. The sea ice thermodynamic scheme in CESM1 uses an observationally based fixed and prescribed salinity profile ("BL99"; Bitz and Lipscomb, 1999). Comparisons between pre-industrial CESM simulations identical except for the thermodynamics schemes indicate that the mushy-layer thermodynamics result in increased sea ice thicknesses as well as concentrations particularly in the Arctic, and somewhat larger/smaller snow-on-sea-ice volume in the Arctic/Antarctic (Bailey et al., 2020). The largest increases in Arctic ice thicknesses are along the Canadian Archipelago and the East Siberian sea, whereas the greatest increases in sea ice concentrations as well as snow thicknesses are in the North Atlantic area (Greenland, Norwegian and Barents seas). The sea ice mass budget in the Arctic has a slightly larger seasonal cycle in the BL99 due to increased wintertime congelation growth (due to thinner ice pack and thus higher conductive heat loss) along with increased summertime melt (due in part to less snow and more meltponds and therefore lower albedos).

The CESM2 includes many changes to other components in the climate model. Despite the changes introduced by the mushy-layer thermodynamics in the sea ice model of CESM2 (which alone lead to thicker and more extensive Arctic sea ice), Arctic sea ice in the CESM2 tends to be thinner, less extensive and less persistent than in the CESM1-LE (DeRepentigny et al., 2020).

While direct quantitative comparisons to Keen et al (2018) are challenging due to differences in forcing protocols between CMIP6 and those used in the CESM-LE, we have added discussion in the conclusion about the Keen et al results and that, like Keen, we expect fundamental behavior across models to be similar and also uncertainty across models may change the timing of influence of winter sea ice and snow changes on conductive heat fluxes.

2) Concerning the cautions in the current study about using a single ice thickness to compute the conductive flux, it is notable that all 7 of the current-generation climate models participating in the Keen et al study employ a sub-gridscale 5-level representation of sea ice thickness categories. Although it is true that the PAMIP protocol allows atmosphere models to employ a fixed sea ice thickness, the use of a variable thickness is encouraged (Smith et al 2019). It could be noted that significant action has already been taken on the main recommendations of this study.

Yes, most current climate models employ sub-gridscale thickness distributions in sea ice, and yet very little remains known about snow depths on sea ice, how well snow depth distributions are simulated and how bias in snow depths and distributions impact simulated winter polar heat budgets. Snow depth distributions have also received less attention in AMIP type runs. The results here indicate that snow depths and distributions may play significant role in wintertime heat budgets over sea ice.

PAMIP protocol do encourage using variable ice thickness yet the majority (just over 90%) of PAMIP simulation protocols call for constant sea ice thickness specifications. Most reanalysis data products continue to estimate fluxes using constant sea ice thicknesses (the exception being the Arctic System Reanalysis, Bromwich et al., 2018). Furthermore, the protocols that allow for changing sea ice thickness do so at a gridcell level and do not allow for subgridcell thickness distributions, and will thus underestimate wintertime conductive heat fluxes and overestimate changes in heat fluxes where sea ice is thinning. In addition, PAMIP protocols and reanalysis products use either very simplified snow (accumulations only through precipitation) or non-existent snow routines, with resultant flux estimates show large wintertime heat flux biases when compared to the recent N-ICE 2015 observations (e.g. Bromwich et al., 2018; Graham et al., 2019). Yet snow – highly reflective and highly insulating - plays an outsized role in Arctic heat budgets. Reanalysis products show consistent warm bias in winter sea ice surface temperatures (e.g. Batrak & Muller, 2019; Graham et al., 2017; Graham et al., 2019; Lindsay et al., 2014), with recent work attributing this to misrepresentation of snow on sea ice (Batrak & Müller, 2019).

We point out these arguments in our revised version of the manuscript.

3) The authors note that observed sea ice volume has decreased by about â…” since 1958-1976. This observation should be useful for validating the model. Does CESM1 reproduce this thinning? The effective thickness seems to have decreased much less over this period (Fig. 1c).

Comparisons between the CESM-LE and the PIOMASS sea ice thickness reanalysis product show that the model and PIOMASS agree well on both summer and winter rates of sea ice volume change (Labe et al., 2018). CESM-LE and PIOMASS also tend to agree on regional mean thicknesses and variabilities throughout much of the Arctic with the exception of the Canadian Archipelago and coastal Greenland, where the CESM-LE overestimates sea ice volumes compared to PIOMASS (and PIOMASS underestimates volumes compared to buoy and submarine data). We have added these comparisons in our description of the model (section 2.1)

4) Since AMIP runs and reanalyses are available over the post-1958 period it should be possible to directly compare the Arctic conductive flux and warming under fixed sea ice thickness with the more sophisticated CESM1 treatment. The authors could directly assess whether variable thickness/thickness distribution leads to increased conductive flux and increased warming. Perhaps there is even a historical AMIP run available with the atmospheric component of CESM1? It would be interesting to plot conductive fluxes and surface temperature from this model alongside the coupled model results in Fig. 1a.

We have moved the figure showing changes in surface heat fluxes and temperatures in AMIP-style runs using the CESM and the accompanying discussion from the supplement into the main article (now Figure 1 and discussion in section 2.2). Figure 1 shows how in these atmosphere-only runs with specified boundary conditions, heat fluxes decrease and temperatures increase – as expected when changes in sea ice thickness are not allowed and in direct contrast to the increases in conductive heat fluxes seen in the CESM1-LE. We propose pulling this figure out from the supplementary material and including it in the main article and extending the discussion accordingly.

5) Would the underestimation of conductive heat flux with a single thickness representation of ice thickness be, to some degree, self-correcting in an interactive simulation? Since the single thickness ice grows more slowly, it produces less thickness over the winter season than the multiple thickness representation favoring a larger conductive flux, all else equal.

To some degree, yes, but not exactly. The results here show that differences in conductive heat fluxes between grid-cell mean thicknesses and those using a thickness distribution will be larger when the sea ice is thicker. Yes, at some point the underestimation of the conductive heat flux will decrease as the ice thins – the timing will depend on the initial state. Previous results show that the mean climate state changes with the introduction of a thickness distribution (ITD) of sea ice with thicker sea ice, larger ice growth rates and warmer surface temperatures in simulations using an ITD (Holland et al., 2005).

Additional References

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