

General

I have found it helpful to receive these two thorough and thoughtful of the original manuscript. I thank them. My plans for the manuscript in response to these comments are outlined below.

Reviewer 1

Major

- 1. I think it is important throughout the paper to note that while MYI is the same thickness there is overall less MYI in the CAA. I think without this clarification the paper only tells part of the story and could be misconstrued.**

The reviewer makes a valid point that the paper's conclusions might be misconstrued. Clearly I have not made it sufficiently clear that the paper is focused specifically on ice thickness and specifically on the old-ice component of sea ice cover of the Canadian High Arctic. However as noted by this reviewer, there has been a decrease in the area of multi-year ice on the Canadian polar shelf, which signifies a decrease in the volume of MYI there, notwithstanding the present null result for thickness.

I will modify the paper's title to reflect the focus on MYI thickness and heed this reviewer's suggestion to note and reference in the abstract, introduction and conclusions the now reduced concentration of MYI in this area.

- 2. Adjustment of ice thickness with the thermodynamic growth model: In the methods it says that the model was run from September 1 to mid-May. My issue is that the time period for modelled ice growth does not correspond to when the ice was observed by the mooring.**

Without sufficient thought, I started the ice-growth calculation on September 1 which is when the mean daily temperature drops below 0°C. Although Resolute Bay typically freezes on about the same date, the CIS charts showed no new ice in the vicinity of Penny Strait in 2009 until two weeks later. Fig. 10 shows a calculated 14 cm of growth for 2-m ice from 1 September to 15 Oct and 7 cm for 4 m ice. Therefore, I agree with the reviewer that starting date for the calculation was chosen carelessly. However, the slow growth of thick ice and the not-so-robust method of adjusting ice thickness to late winter do not justify the complexity applying a different correction for each date of observation in 2009. I propose re-calculating the correction from additional starting dates of 15 September & 15 October to the quantify the difference by winter's end of corrections based on different starting dates.

- 3. Can the time series of snow depth and air temperature used to drive the thermodynamic ice growth model be presented either in a figure or as supplementary material?**

It is probably not desirable to have these data displayed in the manuscript. They are readily available on the Environment Canada website (<https://www.canada.ca/en/environment-climate-change/services/ice-forecasts-observations/latest-conditions/archive-overview/thickness-data.html>). It is probably sufficient to provide the URL, but I can plot them for inclusion as supplementary material if the reviewers consider this useful.

Along with this it would be suitable to note that terrestrial snow measured at Resolute may differ from snow on sea ice.

Yes of course. However, the snow-depth data that I use was measured on the sea ice close to the weather stations and at the same place that the ice thickness was measured each week. The long-term Canadian ice-observing program has been noteworthy in its foresight to include a snow-depth component.

Minor

Line 12: Consider changing "units" to "floes. A good suggestion. Thank you.

Lines 35-35: It might be worth either updating this to CMIP6.

Good suggestion. You can see that my manuscript has been awaiting completion for some time!

Lines 40-46: I think it is worth noting that Moore et al., 2019 showed negative trends in ice thickness in the last ice area from PIOMAS.

I take your point, but my purpose in this paragraph is to note the dismal lack of in situ data in the mischievously named "The Canadian Hole". Without such data, we don't really know whether PIOMAS or satellite altimeters are credible or not. I prefer not to trigger a squabble here about whether they are or not.

Line 60: I will provide the missing "the".

Line 77-79: Is there a reference for these drift speeds?

For Penny Strait, I will provide a cross-reference to fig. 7 and the associated text. I can note that the DFO data for the other two straits are presently unpublished.

Line 75-81: Ice import from the Arctic Ocean into the QEI is increasing ... (Howell and Brady, 2019).

This is a relevant citation, which I will include. Thank you.

102-106: I think in this sentence it is also worth noting the dramatic loss of MYI from the Arctic Ocean and its retreat to this area along the CAA and Greenland. Maslanik et al., (2011) or more recently Stroeve and Notz (2018) show the loss of MYI in the Arctic Ocean that then corresponds to the reduction in ice thickness.

I will work these references into the noted paragraph. Thank you.

Lines 106-110: This justification for the research is focused on oil and gas activities, but I think it would also be worth noting that MYI from this area is subsequently transported southwards to the Northwest Passage, where it affects shipping. In the short term it seems like shipping is more critical than oil and gas extraction.

Yes, and I hope in the long term too. Inclusion of this point will be worthwhile.

Line 135-136: My omission of a phrase is acknowledged.

Figure 4: What is the time period for the median ice concentration in this figure?

I can add to the caption "Epoch 1969-1998". Since the features to be noted here are the exit streams, which remain unchanged, I consider it unnecessary to update to the most recent epoch, but can do so if this reviewer disagrees.

Lines 228, 261: Typos to be corrected.

Lines 257-261 and Figure 8: Can the different ice classes be shaded in the figure so that it is more clear how they differ from each other?

I will add the requested shading. Thank you.

Line 290: Please provide a reference for the assumed values of thermal conductivity of sea ice and snow. Agreed.

Line 427: Typo to be corrected.

Lines 428-429 and Figure 18: Why was the value of 4 m chosen to represent a navigational hazard?

This choice is arbitrary because hazard in practical terms depends upon the temperature of the ice (roughly speaking, the season), the design of the vessel and how it is operated. 4 m was chosen to be appreciably higher than the 3 m linked to the 3-knot-continuous-speed capability of Class 10 icebreaking ships in summertime ice. The most powerful icebreaker in service is the Russian Artika, nominally rated at about Class 9.

I will note the basis of my choice of 4 m choice in the revised text.

Lines 428-436 and Figure 18: I don't think it's an area but rather "a length of the floes cross-section associated with ice thicker than 4m". Table 3 shows the Chord Lengths of floes, which I think is what should be used in the text around Figure 18 and the figure itself.

I apologize for my lack of clarity because the index is in fact an area. It is the sum of the integrals of thickness over distance for all segments of the floe thicker than 4 m. I will work to make this clear in the text and captions.

Lines 431, 435, 473: Typos to be corrected.

Line 478: Worth clarifying that wintertime ice accretion was estimated from an ice growth model. Suggest you revise to read, "... then seasonally adjusted for modelled estimates of wintertime ice accretion".

Good point. Thank you.

Lines 478, 497-499: Typos to be corrected.

Line 499: I would encourage the authors to write a more impactful ending of the paper, perhaps a short paragraph commenting on the future of MYI in the CAA as the Arctic ice pack continues to decline. Will thick MYI continue to exist when the sea ice is confined to the CAA and last ice area in the not too distant future?

This is good suggestion. I propose responding to it by moving the two paragraphs spanning lines 482-490 to the end of this section and reworking them to provide an opinion on the two issues of interest that this reviewer has raised.