Review of "Sea-ice thickness on the northern Canadian polar-shelf: A second look after 40 years"

By: H. Melling

Submitted to The Cryosphere – tc-2021-393

This paper presents a unique set of observations of ice thickness collected in Penny Strait with a moored sonar during winter 2009-10, that are then compared to historic in situ observations manually collected during spring in the 1970s. The paper shows that MYI in this area did not thin over this 50 year gap, which is interesting because MYI in the Arctic Ocean has been shown to thin significantly and decline in areal extent over this time period. The result highlights the dynamic formation of this very thick type of multi-year ice through convergence of the Arctic ice pack against Greenland and the Canadian Archipelago, and its subsequent drift into and through the Archipelago. Overall the paper is well written and provides a unique glimpse into the ice pack of this fairly inhospitable area of the Canadian Archipelago. I have two larger comments and several minor edits/suggestions, but overall this is a very nice paper and I think it will be suitable for publication after these revisions.

Major comments:

1) One of the key results of the paper is that MYI within Penny Strait did not become thinner between the 1970s and 2009-10. I think this is a really interesting result and I commend the authors for working with the dataset to pull out this detail from the noise and variability of the IPS dataset. However, 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. Given that the paper is already quite long, I don't expect the authors to add in more analysis of MYI area within the CAA, but Howell and Brady (2019) and other previous Howell papers have shown that MYI area is declining in the CAA. I think it is critical that the author here reference this in the intro and reiterate it in the abstract and conclusions. Basically, clarify that MYI within the CAA has not thinned because of its source from convergence against the CAA and Greenland, however the presence of MYI within the CAA is declining.

2) My second major ('ish) comment has to do with the 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. The total thermodynamic growth over that time was then added to the observed ice thickness throughout fall, for which the results are presented in Figure 11. My issue is that the time period for modelled ice growth does not correspond to when the ice was observed by the mooring. For example the area of young ice that drifted over the mooring between 900 and 1,300 km (October 15 - ~29), had 6-8 less weeks of ice growth than ice of the same thickness would

have had from September 1. I appreciate that ice growth slows through the season, so the impact may be minimal, but I think this can be addressed by running the thermodynamic model at different time steps, or at least discussed in the methods as a limitation of this "bulk" correction. In addition, 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? Along with this it would be suitable to note that terrestrial snow measured at Resolute may differ from snow on sea ice, particularly young ice that may have formed after some of the heavier snowfalls during early autumn.

Minor comments:
Line (L) 12: consider changing "units" to "floes", I appreciate these weren't large aggregate floes like the first type of ice, but these were still discrete floes.

L 35–36: It's not essential, but it might be worth either updating this to CMIP6, or acknowledging that CMIP6 projects similar conditions in the last ice area.

L 40-46: I appreciate in this section you are focused on observations from this region, but I think it is worth noting that Moore et al., 2019 showed negative trends in ice thickness in the last ice area from PIOMAS. From this you may even be able to add a short bit of text in your discussion section about how models may not recreate the formation of very thick ice within the rubble zones along the CAA and Greenland.

L 60: revise to "… depicts the northern part of *the* Canadian Polar Shelf".

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

L 75-81: It seems like it would be suitable to note here that ice import from the Arctic Ocean into the QEI is increasing due to the transition towards a longer period of ice motion during summer (Howell and Brady, 2019).

L 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.

L 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.

L 127: Suggest adding an "an" in front of IPS.

L 135-136: revise to "… at 45-m depth, *where it was safe from deep keels*."

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

L 228: Switch the "-3" to superscripts.

L 261: correct the value 125, perhaps its meant to be 1350 and continue from the last class.

L257-261 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 believe these classes are marked on Figure 18, so it would be useful to add them here as well.

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

L 428: revise "they" to "they are".

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

L428 – 436 and Figure 18: I don't think its 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.

L 431: revise "Table 4" to "Table 3".

L 455: correct "common".

L 473: Something is missing from this sentence, "… from new ice __?__ the two populations…".

L 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".

L 478: revise to "…thickness for multi-year *ice* in May 2010 was …".

L 497-499: revise to " 46 of the floes *had* at least one 250 m expanse *with an average thickness* over 6m, while 14 floes had an expanse thicker than 8 m".

L 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?

References

- Moore, G. W. K., Schweiger, A., Zhang, J., & Steele, M. (2019). Spatiotemporal variability of sea ice in the Arctic's last ice area. Geophysical Research Letters, 46, https://doi.org/10.1029/2019GL083722
- Maslanik, J., J. Stroeve, C. Fowler, W. Emery (2011), Distribution and trends in Arctic sea ice age through spring 2011, GRL, 38, L13502, doi:10.1029/2011GL047735.
- Stroeve, J.C., D. Notz (2018), Changing state of Arctic sea ice across all seasons, Environmental Research Letters, 13(103001, doi:10.1088/1748-9326/aade56.

David Babb
University of Manitoba