

Ocean Sci. Discuss., referee comment RC1
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Comment on os-2022-16

Anonymous Referee #1

Referee comment on "The role of oceanic heat flux in reducing thermodynamic ice growth in Nares Strait and promoting earlier collapse of the ice bridge" by Sergei Kirillov et al., Ocean Sci. Discuss., <https://doi.org/10.5194/os-2022-16-RC1>, 2022

"General comments":

The authors describe novel results showing visible and potentially invisible polynya formation within the Nares Strait ice bridge using visible and thermal imagery (MODIS), passive microwave (AMSR-E/AMSR-2), and altimetry measurements (ICESat-2). There is little discussion in the literature of these polynyas, the use of ICESat-2 and thermal data for this problem is novel, and polynya impacts on the North Water ice bridge has not been examined. The authors show evidence and make a reasonable argument that these polynyas impact the breakup of the ice bridge. They also make a convincing argument that upwelled Atlantic Water causes the polynyas and potentially the sea ice thinning patterns in Nares Strait and Peabody Bay. Some of the arguments, however, are unclear because of grammatical or organizational errors, or are missing counterarguments that are important to discuss with the reader. The treatment of MODIS and ICESat-2 data is unclear or inaccurate at points (noted below) and more clarity here will make it easier to assess the quality of the results. I appreciated the authors' thorough use of various methods to approach this problem and their clear goal to be transparent about the limitations in the analyses. The needed grammatical/organizational adjustments alone are substantial, and other potentially major revisions are included.

"Specific comments":

Although it does appear that the warm area in northern Peabody Bay is associated with warmer surface ocean heat, at least in years where there is landfast ice covering the entire area, a distinction should be noted in the text about other reasons that warm temperatures may be observed at the surface. Especially important for snapshots and short time periods, AMSR and MODIS surface temperatures will measure warmth merely

because sea ice is broken (more surface ocean is exposed) or recently formed. That can happen because of mechanical wind forcing that has nothing to do with ocean/ice temperature. Further, when looking at snapshots of temperatures when ice is mobile, it is one thing to say the surface is warmer in a location because there is an open ocean surface (the surface is of course warmer if it is open ocean than if it is covered in sea ice) and to say that the open ocean surface is quantitatively warmer than freezing temperatures or than other years (might indicate AW coming to the surface). Differences in figure 10 are more likely to have resulted from synoptic scale variability than an interannual one.

The authors' point that snow depth is really challenging to get an accurate measure of, is well-demonstrated and important for the community.

It is not yet clear to me if the sea ice appears to be thinner in northeast Peabody Bay because of sea ice thinning or winds scouring the sea ice. The ice temperature differences look like they may arise from atmospheric phenomena coming from the northeast corner of Peabody Bay rather than from warming from below/thinning of the sea ice. I think the wording of the modeling work was a little disorganized and could be streamlined to make the main arguments of the model more convincing. Taking a glance at the general weather patterns and wind direction/speed in the northeastern corner of the bay and commenting on that could also bolster the argument for or against AW being the cause for thinning of the sea ice. I am convinced that the persistent polynya at Cape Jackson is originating from AW upwelling.

Do years with earlier ice bridge breakups coincide with a larger or more persistent Cape Jackson polynya? This information could make the linkages clearer between the polynya and ice bridge.

General grammatical and organizational errors throughout with some other writing errors (e.g., inconsistent figure/fig referencing in text). I've commented on some of these specifically in the technical corrections for the Introduction and Methods only, but they exist throughout.

- L105-119 – It is unclear what specific products/levels of data were used for this work. Also, please clarify how MODIS "sea surface" brightness temperature (T_b) provides temperatures of sea ice.

- L122-123 – Segments are calculated using 150 signal (surface) photons. Photon density varies by surface type and can extend to as much as 150 m so the segment lengths listed here are not accurate as described. The ATL07 description should be adjusted to include this.

- L124-131 – Authors mention they can use one method for determining freeboard heights in some instances and use relative heights to produce maps. It isn't clear what analyses in the results use the first method versus the second.

- L135 – Why the uncertainties are small is unclear. References would help.

- L132-141 – The methods here are unclear. It is unclear what (if) the authors are doing about the uncertainties mentioned, implications for the study, and if these uncertainties are problematic enough to prevent being able to use them. See the subsequent comments.

- L137-141 – It isn't clear how this statement is pertinent to your method. Are you mentioning issues with determining snow height depth in general for ICESat-2, adjusting sea ice freeboard for discrepancies in mean snow depth, or stating ICESat-2 data is unusable? This needs more clarification here.

- L149 – What is PHC3 climatology and JRA55-do? It hasn't been introduced. Also, need to write out "high resolution."

- L160-163 – The phrase "We have to admit" can be cut. Additionally, it isn't clear from how this is worded whether this is a significant problem for the study or not, how it will create issues or not, or how you mitigate the issues. My assumption is that this statement should read more like "Despite..., a dearth of bathymetry data in Kane Basin adds uncertainty to the models by... A large number of floating and grounded icebergs that originate from the Humboldt Glacier may also...."

- L178-179 – What are the other parameterizations and why were they ruled out? I see this was mentioned later so it would be good to mention here that this analysis is included in a subsequent section.

- L188-189 – Why are sea surface brightness images not shown? It would be helpful to include these temps in a figure.

- Figure 3 – Some of these images look like they may not have open ocean along the ICESat-2 tracks. Are all of the offsets being calculated by the same method in this figure?

- L233-234 – It is unclear what a "similar difference" in Tb actually means. It would be helpful for this to be quantitative and to be accompanied by a figure.

- The use of MODIS and AMSR temperature data is unclear. MODIS sea surface brightness temperatures were included in the study, but it isn't clear which product and no quantitative analysis were included in the manuscript. Tb was introduced as MODIS band 31 "sea surface" brightness temperatures, but Figure 5 references Tb as a "sea ice" temperature and the figure caption says it is from AMSR-E/AMSR-2 data.

- Figure 5 – Why is the Tb only averaged for March instead of the same timespan as ICESat-2 (Jan-Apr)? The analysis would be stronger for a larger period and would make for an easier comparison to the ICESat-2 results.

- Figure 9 – Error maps should be included here

- There is currently no overlap between the ICESat-2 sea ice heights/AMSR temperatures (2019-2021) and the model results (2006-2010). It might be helpful to extend the AMSR temperatures back to 2006 to provide some comparison and context. It cannot necessarily be assumed that 2006-2010 have the same circulation conditions as 2019-2021.

- Figure 10 – It is unclear how the figure is making the main point the authors assert in L391-392. I believe the authors state that the warmth in Dec 2019 is probably related to the subsequent lack of ice bridge formation, or maybe "altered surface conditions" refers to something else? The ice bridge does form Jan-Apr 2020. So the early 2019 ice bridge failure to form should be unrelated to warm 2019 MODIS temperatures shown. Glancing at these images, this surface warmth was very short-lived and could have easily been associated with winds (synoptic-scale variability) moving sea ice away from the coast and causing sea-ice-free waters. Other years (e.g., 2018) have these same kinds of ephemeral open water conditions for a few days at a time. The authors would need to rule out that the MODIS Tb patterns derive from synoptic-scale variability here to make assertions about interannual variability here.

- L456-474 – Are there more grounded icebergs in north Peabody Bay than in the south? I think this argument could be more succinct with references and this paper should be cited here. Theoretically the iceberg basal melt could create its own polynyas and bring AW all the way to the surface: Moon, T. et al. Subsurface iceberg melt key to Greenland fjord freshwater budget. *Nature Geoscience* 11, 49–54 (2018).

- L482-489 – This paper should support the notion that AW may come closer to the surface in the west in wintertime: SHROYER, E., PADMAN, L., SAMELSON, R., MÜNCHOW, A. & STEAS, L. Seasonal control of Petermann Gletscher ice-shelf melt by the ocean's response to sea-ice cover in Nares Strait. *Journal of Glaciology* 1–7 (2017) doi:10.1017/jog.2016.140. However, this study under review did not find warmer ice/ocean temperatures along the western edge of the strait and the polynyas in the west only open in early summer once the melt season has begun. It would be good to have a

few more sentences of discussion on this.

“Technical corrections”:

- Where paragraphs start and stop are unclear at times. Please add a space between paragraphs.

- Figures – isobaths in all figures need to span the entire length of the channel if they are included. They randomly stop in some parts of some figures.

- L11 – controlling should be controls

- L12 – earlier than what? Earlier in the year?

- L15 – semicolon should be a comma

- L25 – “into **the** North Atlantic”

- L26 – What is the direction of the sea level gradient?

- L27 – “situation” might be better as system

- L29-30 – Awkward, needs to be reworded

- L33 – icebergs

- L35 – Kennedy Channel not introduced

- L47 – during **the** last 15 years underscores

- L47-48 – a shortening of bridge annual or seasonal formation

- L54-56 – Awkward sentence

- L61 – What is latent NOW?

- L62 – lesser extent

- L63 – Might want to restate this as “importance” rather than being interesting.

- L65-66 – Awkward last half of the sentence. Is this meant to say settlements existed here because there were hot spots? Why would that be the case? Are you meaning hot spots as in open water areas (sensible heat polynyas) or do you mean it’s warmer here?

- L67 – Sensible **heat** polynya - please fix throughout

- L88 – of **the** polynya

- L89 – **the** ocean state

- L91-93 – Awkward phrasing

- L93-95 – Run on, use “and” a lot in this sentence

- L98 – **the** observational evidence of **the** polynya

- L100 – It would be helpful for Peabody Bay to be introduced before this. Cannot start sentence with “And”.

- L105 – This first sentence needs to be more descriptive. What are the datasets for?

- L125 – Vicinity

- L127 – ICESat-2, not ICESat

- L128 – what is that. Also, need to state why the method doesn’t work for Nares Strait. The next sentence seems to imply there are no leads, but it needs to be clearly stated at least in the previous sentence. Also, remove the second ‘therefore’ in the next sentence

- L132 – I’m not sure what manifested to be adjusted means.

- L135 – “Basin suggests”

- L138 – “seem to be” should be “are” or “may be” but if you have the data, I would guess you know and this statement should be more firm.

- Figure 2 – Need to mention that this is MODIS imagery

- Figure 3 - ICESat-2 transects are hard to see and contextualize here because there are no y-axes, portions of the lines disappear with the dark background, and plot orientations vary with each snapshot. These need to be larger and may need a y-axis.
 - Figure caption needs to mention what the black and red lines are.
 - It would be useful to mark where sea ice thinning around the polynya begins. It isn’t clear in most of these plots because of how narrow the plots are.
 - The 50 cm scale bars are the same for all so can be removed from all but one plot.

- L205 – It is unclear what 1.56 and 1.14 m ice thicknesses are associated with/changed from?

- Figure 5 - The rainbow color map is unintuitive for representing temperature differences; please use a more intuitive and color-blind friendly scale (e.g., monotonic or smooth diverging).

- L234-235 – This sentence kind of comes out of nowhere and it is unclear why this is important.

- Figure 5 – It would be helpful for brightness temperatures to be converted to Celsius to be more intuitive.

- These scales appear to be incorrect. I would expect temperatures to only vary by a few 10s of degrees, not more than 100.

- L270-278 – Are these modeling results from this paper or from something else?

- L317 – What depth is subsurface referring to?

- Figure 7 – Red and green are not color-blind friendly. Please use another color other than green.

- L406-408 – Good point.

- L410 – knee deep and 2.25 m where?