In the following, the comments by the reviewer are in normal blue characters and our responses to the comments are in cursive and indented. Modifications to the text are shown in quotation marks with bold characters indicating newly added text, and normal characters indicating text that was already present in the previous version. The line numbering in our responses corresponds to those in the revised manuscript with the tracked changes.

RC1: 'Comment on os-2022-16', Anonymous Referee #1, 10 May 2022 reply

"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.

> *We understand this reviewer's concern, but note that in this paper we mainly analyzed the data collected within solid landfast ice sheet from the middle to end of winter season. This automatically excludes such factors as 'broken ice' or 'recent formation' from consideration. Of course, if we are not talking about the mobile ice in the main channel in 2019 or in NOW area.*

The warm temperatures in the channel in Fig.5a are linked to the fact that leads are present in the mobile sea ice drifting south. But the mobile ice is not a topic of our study.

The only exception is the discussion around Fig. 10 where the snapshot MODIS temperatures in early winter are shown. However, that figure is only for pointing at the existing (interannual) variability of surface water temperatures that may indicate the altering ocean heat flux. In respect to this comment, we changed text in Line 469 in Section 4.1 (see our response to your comment to Figure 10):

*"The **MODIS** brightness temperatures, Tb, shown in Fig. 10 **generally** support the **idea that the thermal state of the ocean surface in Nares Strait varies interannually**. In December 2019 (Figure 10b), the high T_b conditioned the ice-free (or covered with thin ice) area **in the northwestern part of Peabody Bay and at the eastern side of Kennedy Channel**. Although the signatures of warmer water **in Kennedy Channel** can also be traced **through leads within the mobile sea ice** in December 2018 and 2020, T_b was observed to generally be lower **and may indicate reduced ocean heat transport towards the surface from below**."*

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

Thank you, but it's really a basic conception. The importance of snow is well known for everybody who deals with the remotely measured sea ice thicknesses one way or another. We just tried to obtain very rough quantitative estimates of the snow impact on the observed anomalies. It may even turn out eventually, that all our estimations of the snow accumulation rates in the region are far from correct. And yes, an accurate measure of snow depths would improve our ability to obtain more accurate results.

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.

*We fully agree that the moderate negative anomalies in the north**west** part of Peabody Bay may be partly (or even fully) associated with a wind effect on snow cover or to a specific spatial pattern of snow depth distribution in general. But it is what we say in Line 488 (Section 4.1): "… it is possible that an unknown spatial distribution of snow may considerably affect the magnitude of anomalies of ice freeboards and our suggestions about the ocean heat impact [on see ice thicknesses]."*

We modified the Lines 497-503 (Section 4.1) and also made small changes in the following paragraph to represent the model results clearer:

"Several simulations with different snow accumulation rates and ocean heat fluxes were run to find an optimal combination of these parameters to match ***the observed modal surface height*** *of 0.26 m* ***near the Cape Jackson polynya (Fig. 3). These simulations were made under consideration that the polynya is kept ice-free during winter by a large (>200 W m-2) ocean heat flux****. ~~It was found that the ocean heat flux at Cape Jackson needed to exceed 200 W m-2 to open the polynya as early as in March.~~ Such large heat flux* ***within a relatively small polynya area*** *seems to be associated with a* ***local*** *upwelling* ***and followed mixing*** *of warm core of the southern branch of mAW rather than with vertical mixing* ***alone."***

However, it is very difficult "to make the main arguments of the model more convincing", because one of the major outcomes of this part is that "Even though the [ice growth] model could reproduce the observed difference of elevations within landfast ice in vicinity of polynya reasonably well, the obtained results should be considered only as an approximation because steady snow accumulation rates and heat fluxes were used, which is likely not representative of their true change in times".

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.

It's a really interesting idea, but its realization would be difficult. The quantitative estimate of size and persistency of the relatively small Cape Jackson polynya would require a good set of high-resolution true-color images that are limited to the recent (from 2015) Sentinel-2 dataset only. The longer series of MODIS images (from 2000) don't have a good spatial resolution (see our Figure 2) and these images are also limited to clear-sky periods only. A straightforward attempt to find a correlation between the visual sizes of polynya in Fig.2 and the dates of breakups (Kirillov et al., 2021) did not give any good results.

In addition, the polynya at Cape Jackson is not the only factor that supposedly affects the bridge breakups. The polynyas along the western coast, air temperatures (through sea ice thawing) and wind may contribute to a collapse. We don't think we are able to investigate all this within a scope of this paper.

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.

We did the best to find all mentioned errors in the text. Thank you for pointing at this problem.

- 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 (Tb) provides temperatures of sea ice.

We added the information about the processing levels for MODIS, AMSR and Sentinel-2 products.

In respect to the second part of the comment, in this study we used Tb to highlight a temperature contrast between certain areas, but didn't interpret these temperatures as absolute *temperatures of the ice/snow surface, although MODIS 11µm brightness temperatures are reasonably close to the temperature of sea ice or snow and might be interpreted so.*

We would like to clarify we never intended that either MODIS or AMSR provide information on the temperature of sea ice/snow or water. The brightness temperature is related to the temperature of underlying surface, but they are not the same. Because T_b is dependent on both the actual temperature and the emissivity of the snow/ice, it leads to lower temperatures for satellite measurements such as the 89 GHz channel than the actual temperature values.

Also, see our response to your comment to Figure 5.

- 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.

We added the followed sentence to describe the variable resolution more carefully:

*"**The along-track resolution is variable according to the number of photons returned, but the typical lengths of segments are 15 and 60 m along strong and weak beams, respectively.**"*

- 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.

We modified this part as follows:

*"**At a close distance from the polynya at Cape Jackson (up to 20 km), the ice-free area** was used as a reference level for an estimation of absolute elevations in vicinity of the polynya. Although the absolute surface heights over narrow bridges may **also** be determined via linear interpolation of sea surface heights measured at the opposite sides of bridge (Babb et al., 2022), however, this approach requires ICESat-2 tracks cross a bridge from edge to edge which does not work for the **long and narrow landfast ice-covered bridges that form in** Nares Strait."*

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

Addressing this concern and also following the recommendations of the second reviewer, the new Lines 286-292 (Section 3.2) were added to explain why all these uncertainties are suggested to be small.

*"**A relatively short off-shore extension (about 10 km) of both coastal zones (along the western coast of Nares Strait and along the northern coast of Peabody Bay) eliminates the regional variations of sea level as a factor contributing considerably to the anomalies. For instance, Samelson and Barbour (2008) reported the relatively small spatial gradient of sea-level pressure over the full width of Kane Basin corresponding to about 2 cm of sea level difference with higher levels along the Greenlandic side of the strait. A geostrophic adjustment requires less than 2 cm sea-level drop from Ellesmere Island to Greenland in Kennedy Channel (Münchow et al., 2006). Also, using the tidal gauge records at Alert and at the opposite sides of Smith Sound, Münchow & Melling (2008) reported the across- and along-channel sea-level differences varying in Nares Strait from a few centimeters to about 10 cm, respectively.**

However, these relatively large differences could be associated with the local dynamical effects as all bottom pressure sensors were deployed in shallow bays not far from the areas covered with mobile ice at Smith Sound and at Alert. The actual sea level gradients below the ice bridge in Nares Strait and their input to the observed ICESat-2 anomalies remain unknown, but are thought to be small comparing to the gradients associated with the anomalies observed along the western coast of Nares Strait and at the northern coast of Peabody Bay."

We also changed sentence in Line 157 (Section 2.2) to indicate that more explanations are followed:

"However, the presence of immobilized ice and relatively short along-track coast to coast distances within Kane Basin suggests that spatial variations of these uncertainties are relatively small ***at least comparing to the calculated anomalies (more details are given in Section 3. 2)****."*

- 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.

See our previous comment and the changes made in the text.

- 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.

To make this part clearer, we changed the text as follows:

"Another type of uncertainty related to estimating ***sea ice thickness from the ICESat-2 elevation anomalies*** *is connected to* ***the presence of snow and, what is more important, to the generally unknown spatial and temporal variability of its accumulation rate****. The existing climatology or estimates of snow depths in the Arctic either do not cover the study area (Warren et al., 1999; Kwok et al., 2020b) or* ***are*** *too coarse to provide* ***a good spatial coverage*** *in Nares Strait (Rostosky et al., 2018; Glissenaar et al., 2021). Using DMSP/SSM/I-SSMIS brightness temperatures, Landy et al. (2017) reported >0.3 m mean snow depth in Kane Basin. However, as we will show later, this height seems to be overestimated and the more modest snow depth of 19±2 cm in Peabody Bay obtained from mean March-April AMSR2 data (Tedesco & Jeyaratnam, 2019) is thought to be a more reliable estimate* ***of the snow thickness in this area. The effect of the spatial variability of snow depth on the elevation anomalies remains unknown and is not accounted for in this study.****"*

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

We have specified that PHC3 is the ***ocean state*** *climatology in that sentence. However, we don't think that an additional explanation is required for both PHC3 and JRA55. It's evidential that the atmospheric reanalysis data is used as an external forcing.*

- 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…."

Cut as requested.

Unfortunately, we don't have a good answer to this question and we cannot do anything to mitigate a problem with the uncertain bathymetry. So, we could just state its existence.

We changed these sentences following your recommendations. Thank you for spending time to find a better wording!

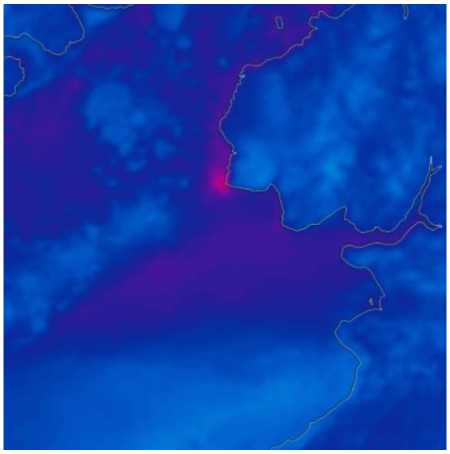
- L178-179 – What are the other parameterizations and why were they ruled out? I see this was mentioned later so it would be good to mention here that this analysis is included in a subsequent section.

We changed the sentence to indicate where those parameterizations are exactly applied:

*"The most often used parameterization of ice growth under average snow conditions in the Arctic was introduced by Lebedev (1938) in a form of $h(m)=0.0133\times FDD^{0.58}$, although other possible parameterizations also exist (Bilello, 1961) and were considered **in Section 3.3**"*

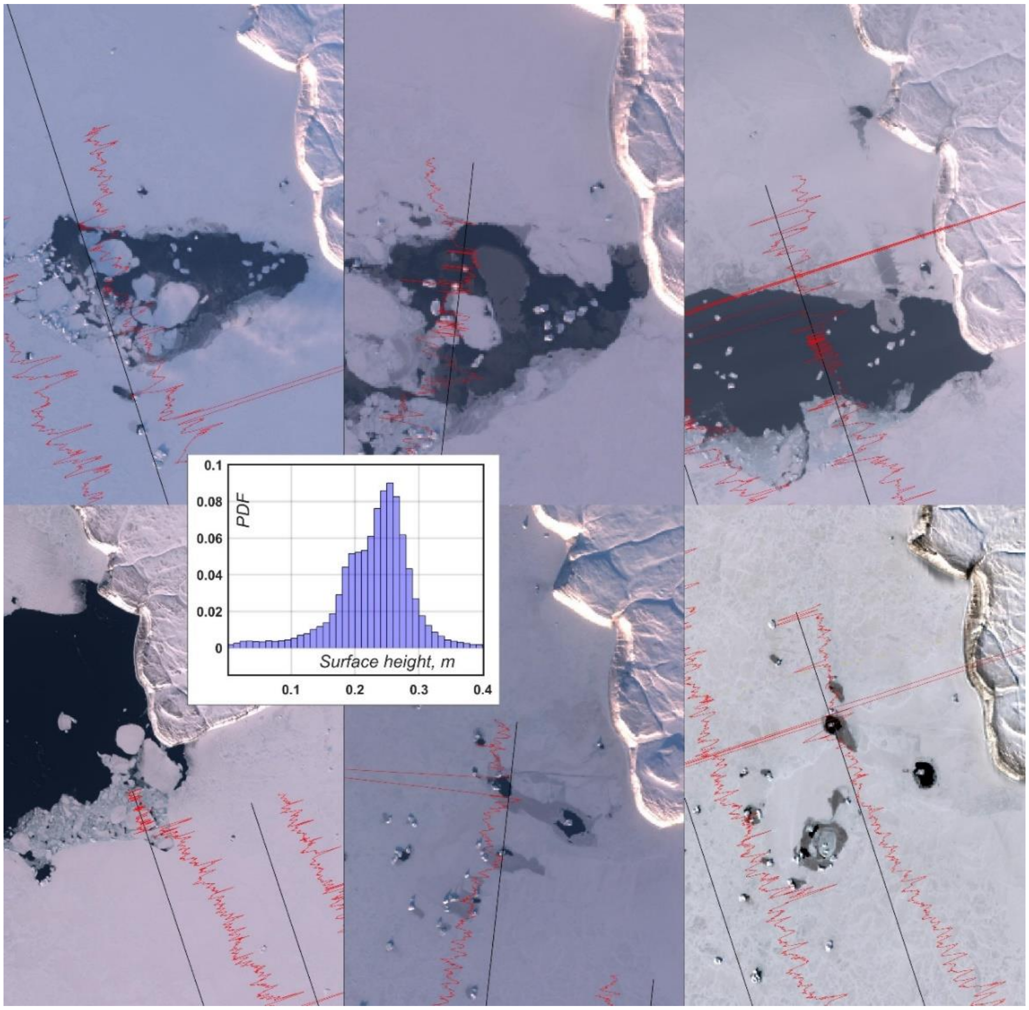
- L188-189 – Why are sea surface brightness images not shown? It would be helpful to include these temps in a figure.

We did not show these images because "…polynya signatures are less clear in those images because of their lower spatial resolution" (Line 234, Section 3.1). As an example, see the image below. It shows T_b on 29 Jan 2021 (the ice bridge in place). One can see that there is a pronounced difference in the area around Cape Jackson compared to the surrounding ice, but the quality of this (one of the best!) image is relatively low to be used in the paper. In addition, it is difficult to determine whether the area around Cape Jackson is ice-free or covered with thin ice in these thermal images.



- 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?

It's not true. The enlarged portion of Fig.3 is shown below to prove that open water (or very thin ice with negligibly small freeboards) is present in all panels. Please keep in mind while looking at these panels that the timing of the overlaid Sentinel-2 images and ICESat-2 tracks don't match and may differ by a few hours.



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

This sentence was changed as follows:

*"The difference in surface height anomalies between the southeastern and northwestern parts of Peabody Bay is ~~supported~~ **correlated with** ~~by~~ a similar difference in the observations of T_b."*

However, we think that the next two sentences along with referencing Fig.5 clearly explain what we mean under this similarity. The quantitative estimates of the T_b difference are not given because the brightness temperature don't correspond to physical temperature of the ice surface, though they are strongly correlated.

- 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.

This uncertainty seems to be the result of a few cases related to using wrong definitions in some parts of the text.

*We deleted "MODIS ~~sea surface~~ brightness temperature" in the Data and Methods section. In the Fig.5 caption, the "ice surface temperature" was changed to "**brightness** temperature". We incorrectly used "ice" just because most of the strait is landfast ice-covered. Thank you for finding and pointing at these issues. We also went through the text to make sure that there are no other mistakes related to using "(ice/surface) temperature" where "brightness temperature" should be.*

Both MODIS and AMSR-2 brightness temperatures were used in our study: Figure 5 shows the AMSR-2 brightness temperatures ("AMSR-E" is a remnant from the earlier version that included that dataset - deleted) and Figure 10 - the MODIS brightness temperatures.

The MODIS brightness temperatures in Fig.10 are for showing the contrast of surface water temperatures between different regions and point at their synoptic and/or interannual variability that may alter the ocean heat fluxes. This figure just visually supports the information mentioned earlier and doesn't require quantitative estimates.

On the other hand, the AMSR-2 brightness temperatures in Fig.5 are for showing the correlation between the robust (all-weather permitting, though low-resolution) data on the thermal state of the surface and the observed anomalies of ICESat-2 elevations. The MODIS brightness temperatures can capture this linkage in individual images, but they are less convenient because of their strong dependence on cloudiness.

- Figure 5 – Why is the T_b 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.

Although there is no large difference between using only March compared to averaging over a longer period, we changed the timespan to January-March. April was excluded because the rising air temperatures (see Fig. 6a) and incoming shortwave radiation in the end of the winter lead to a significant increase of T_b and, as a result, reduce the spatial contrasts.

- Figure 9 – Error maps should be included here

Figure 9 represents the results of numerical simulation with the global oceanic model. There is no way of estimating and showing errors 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.

We used the model of opportunity and were not able to choose a different simulated period.

However, the main intention of the model was to demonstrate the circulation under the ice bridge. Since the main factors controlling water dynamics in this region (the along-strait sea-level gradient and the prevailing northern winds) don't vary a lot interannually, we reasonably suggest that the patterns shown in Fig.7-9 are generally valid and fairly represent (modeled) water dynamics and thermohaline state of Nares Strait in winter.

- 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.

*The main and only purpose of Fig. 10 was to demonstrate the existing spatial and interannual variability of surface water temperatures in the beginning of each winter. Taking into account the reviewer's phrase "Glancing at these images, this surface warmth was **very short-lived** …", we suggest that this figure was misinterpreted somehow.*

To make the message clearer, we altered the text in Lines 469-475 (Section 4.1):

*"The **MODIS** brightness temperatures, T_b, shown in Fig. 10 **generally** support the **idea that the thermal state of the ocean surface in Nares Strait varies interannually.** In December 2019 (Fig. 10b), the high T_b conditioned the ice-free (or covered with thin ice) area **in the northwestern part of Peabody Bay and at the eastern side of Kennedy Channel**. Although the signatures of warmer water **in Kennedy Channel** can also be traced **through leads within the mobile sea ice** in December 2018 and 2020, T_b was observed to generally be lower **and may indicate reduced ocean heat transport towards the surface from below**."*

- 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).

We didn't analyse the amount or density of icebergs in different parts of the bay carefully, but most of them seem to be grounded at the eastern flank of the mid-basin underwater ridge in Kane basin.

The mechanism of dissolution suggested in our paper doesn't suggest an intensive basal melt and, therefore, strong freshened plume dynamics that may deliver warm bottom water to the surface.

Anyway, this part of the paper was removed according to the other reviewer's recommendation.

- 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.

It's an interesting point. We didn't consider this effect closely, but the reviewer is right – we have to highlight that upwelling of water above the southward jet in winter may also contribute to the formation of thinner ice along the western coast in winter. Thank you for bringing this up.

The followed sentence was added in Lines 581-586 (Section 4.2):

*"We suggest that the observed negative anomalies are attributable to the heat **transferred towards the base of the landfast ice** from **either upper thermocline mainly consisted of Pacific Water in this area (Jones et al., 2003) or warm** underlying mAW. **The baroclinic adjustment of the ocean to the intensification of the southward current in winter induces upwelling above the core that may shift upper thermocline water closer to the surface along the Ellesmere coast (Rabe et al., 2012; Shroyer et al., 2017) and, as a result, forms favourable conditions for a larger heat transport to the bottom of sea ice here.**"*

Technical corrections:

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

The whole text is formatted in accordance with the rules of Ocean Science journal with using of its Word template.

- 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.

Thank you for this comment. However, the isobaths stop not randomly, but on purpose. In some regions, the bottom topography is complex and dense contours would have resulted in a worse displaying of useful data (e.g. along the western coast, in Kennedy Channel and Smith Sound). The isobaths in these regions are shown in Fig. 1 and we think it's more than enough for general understanding of the regional bathymetry.

The only purpose of plotting the selected isobaths in Fig.4 and Fig.5 is to show the relative positions of the observed elevation and temperature anomalies relative to the central ridge in Kane Basin.

- L11 – controlling should be controls

Changed. Thank you for finding this.

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

Yes, the bridge tends to form earlier in the season. Corrected.

- L15 – semicolon should be a comma

Corrected.

- L25 – "into the North Atlantic"

Corrected.

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

The sea level decreases from Lincoln Sea towards Baffin Bay, but we think this explanation is redundant. The southward flow can be maintained by this particular direction of gradient only.

- L27 – "situation" might be better as system

We checked both ways and think that "situation" works better here.

- L29-30 – Awkward, needs to be reworded

The sentence was changed as follows:

*"An average **annual** ice export of ~141 km^3 ~~per year~~ **in years when the ice bridge exists** is about half of that exported during **bridgeless** years ~~when the ice bridge fails to form~~ (Kwok et al., 2010)."*

- L33 – icebergs

Changed.

- L35 – Kennedy Channel not introduced

*Changed as follows: "Under-ice sonar measurements in Kennedy Channel **in the northern part of the strait** showed the modal peak of ice…"*

- L47 – during the last 15 years underscores

This part was re-written in respect to the comment from the other reviewer.

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

In the modified text, it became more clear that we meant a shortening of the bridge existence duration in this sentence.

- L54-56 – Awkward sentence

This sentence was changed as follows:

*"Despite NOW is believed to be one the most studied polynya, ~~it is~~ the sensible heat polynyas associated with **an impact of warm subsurface water** ~~relatively warm modified Atlantic water (mAW) underlying the cold surface mixed layer that~~ are **also well-known feature in the landfast ice-covered** ~~more common in the~~ Canadian Arctic (Hannah et al., 2009)."*

- L61 – What is latent NOW?

*Changed to "…latent NOW **polynya**…".*

- L62 – lesser extent

Changed. Than you for correcting.

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

*The reviewer is right. "**Important**" seems to be a better way to highlight our interest to those polynyas.*

- 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?

*We meant **biologically productive** hot spots, of course. Changed as follows:*

*"Those polynyas … seem to be **highly biologically productive** ~~local hot~~ spots as they evidence for nearby prehistoric settlements …"*

- L67 – Sensible heat polynya - please fix throughout

Fixed.

- L88 – of the polynya

Corrected.

- L89 – the ocean state

Corrected.

- L91-93 – Awkward phrasing

Sorry, but we could not understand what awkwardness the reviewer meant. The sentence seems to be fine, but we made some changes to make sure that the message is clear:

*"However, **these observations were** limited mainly to Kennedy Channel and **may not apply to** the entire strait, **limiting their use in** identifying the processes maintaining **ice-free conditions** at Cape Jackson **at times** during winter."*

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

To avoid this, we changed the sentence as follows:

*“This study intends to partially fill these gaps **by examining** the ice-ocean interactions **that occur** under the bridge during winter, **and subsequently examine their influence on the** formation of polynya at Cape Jackson and other yet unknown invisible polynyas in Nares Strait.”*

- L98 – the observational evidence of the polynya

Corrected.

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

We added the reference to Fig.1 where Peabody Bay is indicated and got rid of “And”.

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

We modified this sentence as follows:

*“In this study, we used remote sensing data from different satellites **to demonstrate the presence of thinner ice and ice-free polynyas in Nares Strait**.”*

- L125 – Vicinity

Thank you for finding this typo.

- L127 – ICESat-2, not ICESat

Changed.

- L128 – what ïƒ 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

We could not understand what does “what ïƒ that” stands for.

In respect to the rest of the comment, we changed the sentence as follows:

*“…this approach requires ICESat-2 tracks crossing a bridge from edge to edge what does not work **in the long and narrow ice bridge in** Nares Strait.”*

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

*We removed the word “**manifested**”.*

- L135 – “Basin suggests”

Changed.

- 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.

Changed to “are”.

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

The mention of MODIS imagery was added.

- 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.

Unfortunately, any attempt to make the elevation even slightly larger results in overlapping of data from adjacent tracks. The figure caption was extended to mention how to interpret the data from each individual track:

*"Figure 3: The ATL07 elevations **(red lines)** from 3 strong beams overlaid the Sentinel-2 image of polynya in March-May 2020 and in March-April 2021. **The elevations are plotted as absolute deviations from the ground tracks (black lines) in direction normal to the tracks.** The open water was used as a reference level to calculate the corresponding offsets for each subset. **The red scale bar in each panel correspond to 50 cm elevation.** The inset histogram demonstrates the probability distribution of all heights shown in the panels a-f."*

Figure caption needs to mention what the black and red lines are.

Done. See the previous comment.

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.

We really don't know how to do what the reviewer asks for. The elevations start to raise right from the polynya and increase gradually until they flatten about 10 km away from polynya. However, it's difficult to choose the exact position where ice thinning starts.

The 50 cm scale bars are the same for all so can be removed from all but one plot.

We prefer to keep it in each panel because it helps to see a relative amplitude of elevation by comparing it to a nearby scale.

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

These thicknesses correspond to the different presumed snow-to-ice freeboard ratios. We added comma, to clearly separate two parts of this sentence.

- 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).

The color map was changed for better reception by color-blind readers.

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

We don't really understand why the reviewer does not see the connection with the previous sentence. However, after addressing the other reviewer's concern, these two sentences were changed as follows:

*"The difference in elevation anomalies between the southeastern and northwestern parts of Peabody Bay is ~~supported by~~ **correlated with** a similar difference in the observations of T_b. The mean AMSR temperatures in March for all three years are shown in Fig. 5 and highlight the*

*presence of warmer (thinner) ice in the northwest compared to colder (thicker) ice in the south**east**~~west~~."*

We hope it helped make this part more correct and clearer.

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

Kelvin scale is a traditional way of presenting brightness temperatures.

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

It is correct that surface temperature would vary across a smaller range than 100 degrees. However, we clarify that 89 GHz AMSR brightness temperatures are not indicative of surface temperature alone, but measure the radiance of microwave radiation that is expressed in units of temperature (K) of an equivalent blackbody. Therefore, brightness temperatures are influenced by a combination of surface temperature, emissivity, and reflectance of the surface. In Figure 5, these variables are influenced by the different types, thicknesses, and surface properties of sea ice, accounting for the wide range in scale values.

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

The results presented in these lines are not from the model, but from the empirical relationships connecting ice thicknesses and amount of freezing-degree days (see the method description in the last paragraph in Section 2.4). All these estimations were obtained in the frame of this paper.

- L317 – What depth is subsurface referring to?

Below 100 m. This is the depth used to divide the water column into two layers in Fig.7. Reference to Fig. 7b was added to this sentence.

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

The green color was changed to grey.

- L406-408 – Good point.

Thank you

- L410 – knee deep and 2.25 m where?

The honest answer is we don't know. The "southeastern part of Peabody Bay" is the most precise positioning that could be drawn from Dr. Kane's report published in 1856.