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.

The role of oceanic heat flux in reducing thermodynamic ice growth in Nares Strait and promoting earlier collapse of the ice bridge

Sergei Kirillov, Igor Dmitrenko, David G. Babb, Jens K. Ehn, Nikolay Koldunov, Søren Rysgaard, David Jensen and David G. Barber

Overview

Nares Strait is an important oceanic connection between the Arctic Ocean and the Atlantic. It carries a sizeable fraction of the total outflows of Arctic surface and Pacific waters and provides a quick exit for thick old ice leaving the "last ice area". The rates of outflow of both seawater and ice are reduced when the strait is covered by fast ice, a condition of changeable duration that occurs in many but not all winters.

This paper explores factors that may reduce the viability of shore-to-shore fast ice in Nares Strait during winter. The discussion is based upon ice-cover observations acquired solely by satellite-based remote sensing instruments. These include Sentinel-2 SAR imagery (50 m), MODIS "true colour (250 m)", MODIS "mid-infrared brightness temperature (1000 m)", Sentinel 2 "high resolution optical imager (10 m)", AMSR "89-GHz brightness temperature (6250 m)" and IceSat's Advanced Topographic Laser Altimeter System "ice-plus-snow elevation (60 m)". No contemporary "ground-truth" data on the ice cover or the ocean were collected. A 1D sea-ice thermodynamic model was used in an attempt to distinguish the separate contributions of ice and snow to measured elevation. Oceanographic insight was garnered from a run of the Finite volumE Sea ice Ocean Model v2 for the 5-year interval, 2006-2010. The authors provide no indication that the viability of this model has been evaluated in Nares Strait.

The authors draw attention to a small polynya that forms off Cape Jackson on Greenland's coast at the southern end of Kennedy Channel during late winter of years when fast ice covers Nares Strait. They argue that this feature is indicative of a localized upward flux of sensible heat from the ocean to the ice. Using lidar data, they map a larger negative anomaly in surface elevation (ice plus snow) around the polynya._They use the measured elevations to constrain a 1D thermodynamic model of ice-plus-snow, driven by surface air temperature, to explore the complementary influences of ocean heat flux and snow depth in reducing ice-thickness. The "best match" corresponded to snow accumulation at 4-8 cm/mo and a 10-20 W/m2 heat flux from the ocean. Atlantic-derived water found below 150-m depth was proposed as the source of this heat, delivered directly to the underside of sea ice via upwelling. The paper also documents a band of relatively low ice-plus-snow surface elevation along the eastern coast of Ellesmere Island during two years with fast ice, but this was not observed during the one studied year (2019) when ice was mobile throughout the winter. Upwelled Atlantic-derived water was proposed as the source of this anomaly also. In closing remarks, the authors speculate that the zones of thermally weakened fast ice that they have identified on both sides of Kennedy Channel and Kane Basin in late winter weaken the stability of fast ice along the full 550-km length of Nares Strait and promote its earlier collapse in summer.

Assessment overall

The authors make a useful contribution in drawing attention to the influence of oceanic heat flux on the fast ice cover of Nares Strait. Oceanic heat flux has been shown to have a noticeably impact on the fast ice cover of the Canadian polar shelf, particularly in shallow waters with strong tidal currents where small polynyas form (Topham et al., 1983 [JGR 88(C5);]; Melling et al., 1984 [CSR 3(3)]; Melling, 2002 [JGR, 107]; Hannah et al., 2009; Melling et al., 2015). It might indeed be surprising if such features were not found along Nares Strait.

They make ingenious use of information from a variety satellite remote sensors to document ice-cover characteristics and state of motion, to detect polynyas within fast ice, measure ice-plus-snow elevation and map surface temperature. However no in situ data are brought into play. This is regrettable. In consequence, the accuracy, precision and possible bias of elevation and snow-surface temperature measurements for example were not determined, so that confidence in these data and the results derived from them is eroded. Moreover, the critically important separate contributions of ice freeboard and snow depth to elevation are not known, although the authors make a valiant effort to generate "educated guesses" through use of a 1D thermodynamic ice model; unfortunately, the "surface" air temperature used to drive this model was taken from the ERA5 re-analysis which has a 31-grid scale, much too large to achieve a realistic representation of surface weather conditions in Nares Strait.

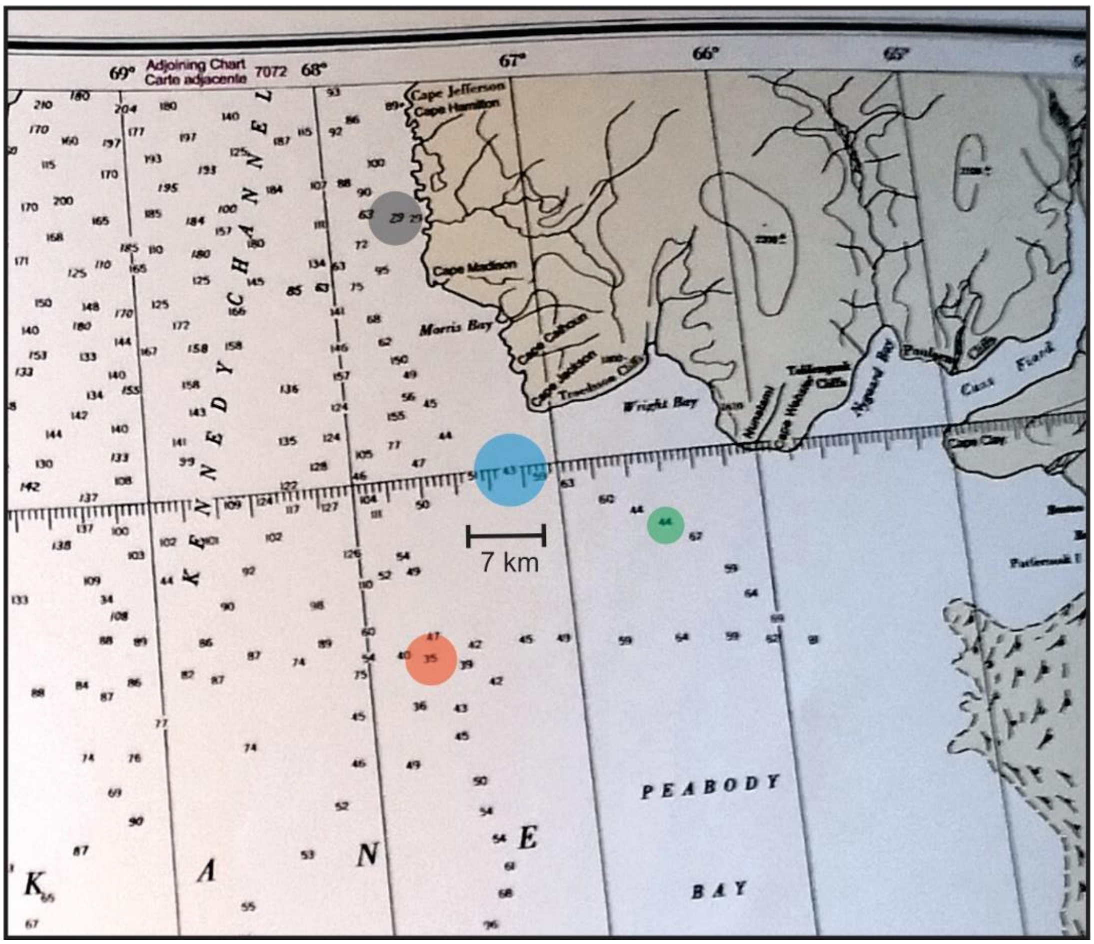
The lack of contemporary or past oceanographic observations at the locations of interest is a serious shortcoming in a paper that strives to attribute polynya formation to oceanic heat flux. The FESOM global ice-ocean model has been harnessed in an effort to fill this gap. However, since this model neither assimilates contemporary ocean observations, nor seems to have been evaluated against existing ocean observations collected nearby, nor to incorporate tides, there is little basis for confidence in the minute (from a global perspective) thin-ice features that it is called upon to "explain".

In the particular instance of the Cape Jackson polynya, the authors could have saved themselves some trouble through a heavier reliance on Hannah et al. (2009). I examined CHS Chart No. 7072 to find a 43-fm (78 m) sounding 4 naut miles to the SW of Cape Jackson. I estimated depth beneath the polynya as half this, since the polynya is centered about 2 miles off the cape. WebTide (https://www.bio.gc.ca/science/research-recherche/ocean/webtide/index-en.php) predicts a 1 m/s spring tide here, so that Hannah's tidal mixing parameter is 2.1. This is comparable to values at polynyas in fast ice across the Canadian polar shelf, where turbulence generated by energetic tidal currents moves heat upward from relatively shallow depth. It seems unnecessary to look to the weaker general circulation to lift warm water from depths 3-4 times greater.

I believe that this paper should be published. However at present, it strives to be too comprehensive, is too speculative and therefore too long. There is valuable information therein and some pioneering use of remote sensing, but these strong points don't shine forth as well as they should. Specific suggestions for changes are listed below.

> *With all respect to the reviewer we have to disagree with the last argument about the tidal origin of the Cape Jackson polynya. There is a principal difference between this polynya and the polynyas in the narrow passages of CAA discussed in Hannah's paper. First of all, the polynya at Cape Jackson is not constrained by landmass. But our main argument would be that there are other shallows in the area with relatively strong tidal currents (see figure/table below and also supplemental figures in the end of the document showing the current speeds predicted by*

WebTide). Although the tidal mixing parameter is indeed higher at Cape Jackson, it is also remarkably high in all other positions with known depths. However, an effect of tidal contribution to sensible polynya formation also requires a continuous lateral heat inflow at depth. The reversible nature of tidal motion cannot provide a consistent heat inflow by itself. In addition, the calculated horizontal excursions over one-quarter of the tidal cycle do not exceed 7 km in all considered positions (colored circles in the figure below). Even at Cape Jackson, where the predicted tidal currents are the largest, such excursion may result in only ~30m (59 to 43 fm) vertical displacement – not very large to upwell much heat. Therefore, even if a strong vertical mixing associated with tidal currents takes place at Cape Jackson, there should be a consistent mechanism “pumping” the heat from depth to the shallow.



Bathymetry in the vicinity of Cape Jackson. The circles show horizontal excursions over one-quarter of the tidal cycle (during spring tide) in several shallow regions based on WebTide current speed predictions.

The tidal mixing parameters. The colors correspond to the positions from the figure above

	TideMarker 1	*TideMarker 2*	*TideMarker 3*	*TideMarker 4*
Depth, fm	*43*	*35*	*44*	*29*
Spring tide, m/s	*0.48*	*0.34*	*0.24*	*0.36*
Horizontal excursion, km	*6.8*	*4.8*	*3.4*	*5.1*

Mixing parameter	*2.85*	*3.20*	*3.79*	*3.06*

However, we have to admit that our suggestion that upwelling brings warm deep water from Peabody Bay ***directly to the surface*** *may be too challenging. In combination with vertical mixing, it may be sufficient to upwell this water just closer to the surface – to the bottom layer over the "ridge" dividing Peabody Bay and the central channel of the strait. We changed the sentence in Line 536 (Section 4.1) accordingly.*

"This heat may ***either be*** *upwelled* ***over the mid-basin ridge closer*** *to the surface ~~(leading to formation of sensible heat polynya at Cape Jackson)~~* ***and/****or transported upward* ***to the lower surface of sea ice (or to the ice-free polynya)*** *by vertical mixing."*

Comments (major)

1. The authors have chosen to refer to the fast ice that covers the full 500-km of Nares Strait during many winters as the "ice bridge": The terminology is confusing because a long strip of fast ice does not resemble a bridge. I believe that most readers will consider the bridge to be the arch that forms the boundary between fast and mobile ice, most often in southern Kane Basin. As in masonry, the arch is strongest geometry for a load bearing structure because it is everywhere under compression, thereby exploiting the stress-state where sea ice is strongest. I recommend that the authors devise a different term to refer to locations within the fast ice "above" the arch. For example, at Cape Jackson, "more than 200 km north-east of the bridge".

(This is also an answer to the reviewer's minor comments to lines 67, 94 and 130)

We understand this reviewer's concern about the terminology. However, even from a simple geometric point of view, arch is not an areal object but a line. In this research, we follow the terminology that was used in our previous paper (Kirillov et al., 2021; JGR) where we specifically point at the difference between an ice arch and an ice bridge: "…instead of using the term "arch", we prefer to use the term "bridge" for this structure in general and use "arch" to describe the characteristic dome-like shape of the bridge's leeward (southern) edge". We considered a bridge as an object connecting two opposite shores. From this point of view, "ice bridge" seems to be a good term for what we observe in Nares Strait. Historically, the landfast ice in Nares Strait was used as a migration route for Inuit. Mathieu Plante, an expert in sea-ice rheology who reviewed that paper, specifically mentioned that "it seems that there is no consensus on the terms ice arches and ice bridges" and agreed (supported) with our way of distinguishing them.

It might also be worth mentioning that there are some examples of the real bridges with width exceeding their length considerably.

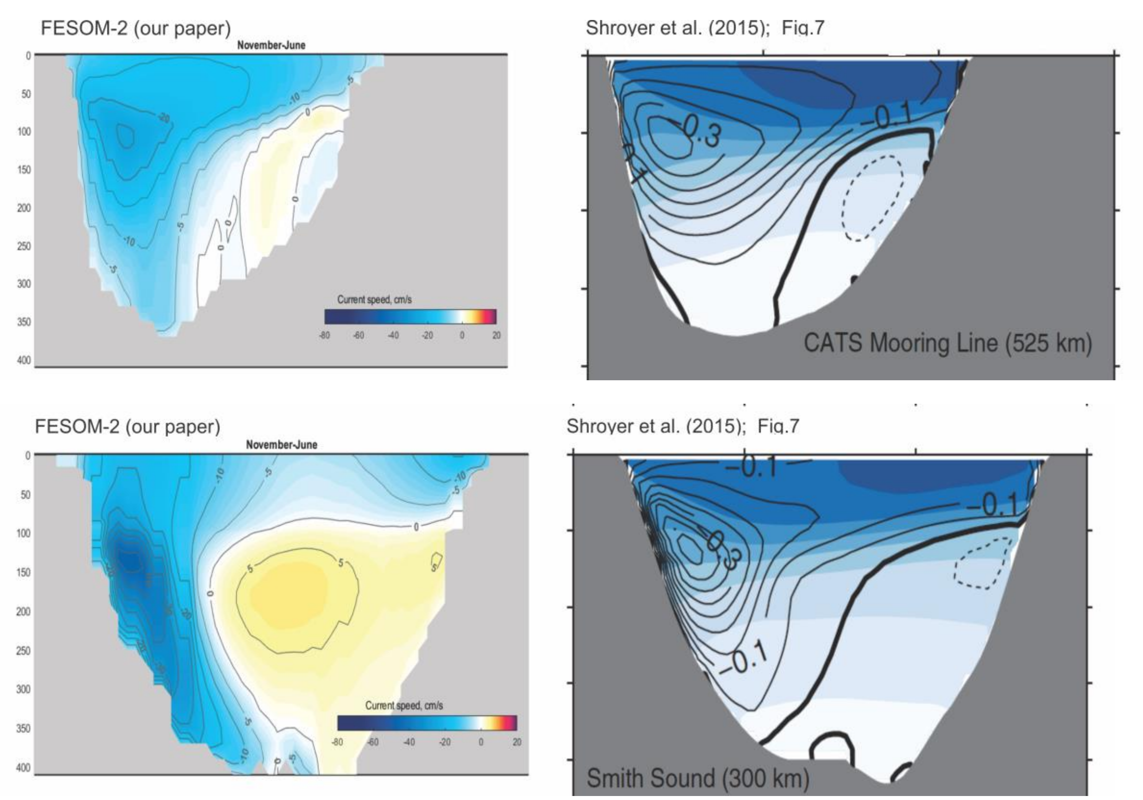
We added the following footnote in Introduction to address this concern:

*"****In the absence of established consensus on the terminology, hereafter we prefer to use the term "bridge" for landfast ice blocking Nares Strait instead of "arch" which is used to describe the characteristic dome-like shape of the bridge's leeward (southern) edge.****"*

2. Line 148, “The model was driven by the atmospheric reanalysis fields from JRA55-do”: This was probably not a good choice. Samelson and Barbour (2008) concluded that a grid 10x finer than that of JRA55 was required to correctly represent weather conditions in Nares Strait. I recommend adding a discussion the capability JRA55 to represent the mesoscale meteorology of Nares Strait, a channel much narrower than 55 km in width for much of its length, bordered by high terrain and characterized by a strongly stable atmospheric boundary-layer – the Arctic inversion – during the freezing season. This could be perhaps achieved via comparison of simulations by the ERA55 and Polar MM5 models.

We agree that hi-resolution atmospheric models better represent weather conditions in the narrow Nares Strait with a strong impact of steep surrounding topography. However, a relative impact of wind forcing on water dynamics in the strait (not at CATs transect only) remains generally unknown and it’s the along-channel sea level gradient that is thought to be a main factor controlling southward ice and water transport (Munchow and Melling, 2008).

Unfortunately, without running additional model experiments to investigate the effect of more realistic regional wind forcing on circulation in the strait, we can only try to address this reviewer’s concern by comparing the results of our <u>model of opportunity</u> (FESOM-2) and results previously reported by Shroyer et al. (2015) who used wind forcing from the high resolution regional atmospheric model (after Samelson and Barbour, 2008). The figure below represents the along-channel velocities at two transects: across Kennedy channel at CATS mooring line and across Smith Sound. Both simulations show generally similar vertical structure of along-channel flow and fairly resembling current speeds. Slight differences might be attributed to different time interval used for simulations (2006-2010 in our paper, and 2005 in Shroyer et al., 2015).



*To underline the fact of good correspondence between FESOM2 results and results obtained by Shroyer et al. (2015) and, therefore the suitability of using the low-resolution atmospheric forcing, the following sentence was added to the text (Line 177, Section 2.3): "**Despite using a low resolution JRA55 product that is probably unable to reproduce orographic strengthening of wind within the relatively narrow Nares Strait (Moore and Våge, 2018), the modelled current velocities in Kennedy Channel and in Smith Sound fairly coincide (not shown) with velocities obtained by Shroyer et al. (2015) who used the Polar MM5 regional atmospheric model, which has a finer horizontal resolution of 6-km.**"*

3. Line 166, "The 6-hourly records of 2 m air temperature, wind speed and humidity in Kane Basin were taken from the ERA-5 global reanalysis database": The 31-km grid of ERA5, comparable to the width of Nares Strait, does not come close to resolving the channel and very its steep surrounding terrain. I suspect that much of the strait's "sea surface" resolved on a 31-km scale will actually be above sea level and therefore "terrestrial". It is very difficult to accept that ERA5's 2-m air temperature values hold much credence for simulating ice growth in the real world. Since sea ice dissipates the upward flux of latent heat from wintertime freezing by long-wave radiation from its top surface, would you not be better using satellite-derived surface radiation temperature to model freezing? Please strive to persuade readers that my viewpoint is invalid.

We agree that the (relatively) low spatial resolution of any global atmospheric reanalysis makes it difficult to consider their products as good proxies of atmospheric conditions in Nares Strait (e.g. Moore and Vage, 2008). However, we would like to explain here our logic and justify feasibility of using ERA5 data in our ice growth model.

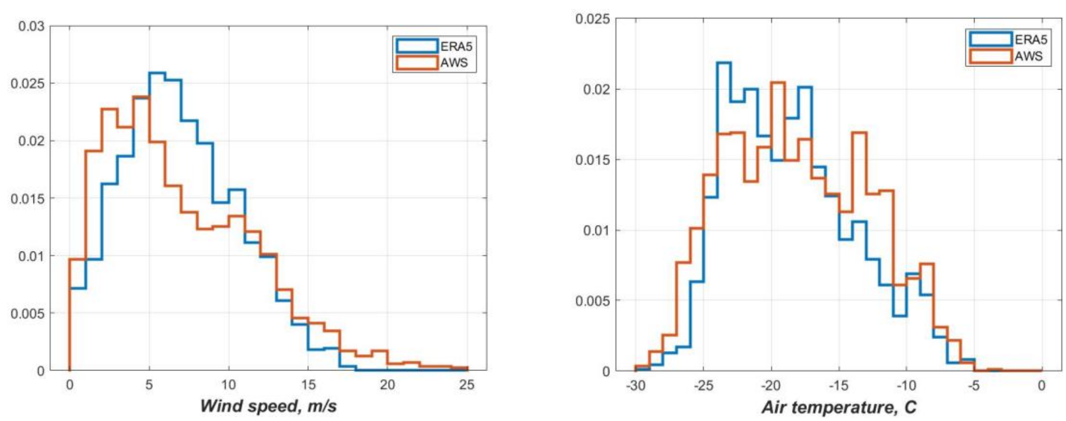
The key goal of using ice growth model in our study was not to obtain an absolute ice thickness in a certain position, but "to investigate a possible joint effect of snow and ocean heat flux on the observed spatial variations of surface heights in the vicinity of Cape Jackson" (i.e. over a distance of ~10 km or so). Despite a possible bias in ERA5 data in Nares Strait, a spatial variation of any meteorological parameter over such a short distance is negligibly small. Therefore, any local anomalies of ice thicknesses here are thought to be controlled by spatial variations of ocean heat flux rather than varying meteorological parameters. It means that although our estimates of absolute sea ice thicknesses presented in Figure 6b and 6c could have been biased, the difference of elevations presented in Figure 6d (and it is specifically mentioned that this plot is particularly important) are exposed to these errors to considerably lesser extent.

To underline this aspect, we added the followed sentence to Line 210 (Section 2.4):

*"**Although the spatial resolution of ERA5 reanalysis data is relatively low to resolve orographic effects in the narrow and steep Nares Strait (Moore and Våge, 2018), the key goal of the ice growth modeling was not obtaining the absolute ice thicknesses, but reproducing the spatial variations of combined ice and snow surface heights in the vicinity of the polynya. From this perspective, even though the modelled absolute ice thicknesses calculated with using ERA-5 data may be meteorologically biased, the accuracy of meteorological data seems to have a considerably smaller effect on the investigated spatial differences of ice thickness compared to unknown snow accumulation rate and possible spatial variations of ocean heat flux.**"*

Another aspect that we would like to point at is that the magnitudes observed anomalies of surface elevations around polynya (ICESat-2 data, Figure 3) were very similar in 2020 and 2021. It implies that interannual variations of air temperature and/or wind speed may insignificantly affect the maximum ice thickness at the end of winter and that it's the ocean heat flux that seems to determine the thinner sea ice around Cape Jackson. Therefore, using just <u>realistic</u> meteorological parameters is believed to be fine for reproducing the ice growth during winter. The unknown snow accumulation rate and its wide range used in the model is believed to have much stronger effect compared to the coarseness of ERA5 grid.

To demonstrate that ERA5 parameters are realistic, we generated here two histograms showing probabilities of winter (December-April, 2014-2018) air temperatures and wind speeds measured with Automatic Weather Station set on Hans Island (red line) and from the nearby node of gridded ERA5 product during the same period. AWS on Hans Island is installed at 168m elevation (mean pressure 991 hPa), so for comparison we used the closest 1000 hPa level from ERA5 dataset. It was found that, despite some differences between reanalysis and measured data distributions, the means of both parameters are not largely biased. For instance, the mean measured wind speed was 7.1 m/s that is almost equal to 7.0 m/s in ERA5. For the mean air temperatures these numbers are -18.0C and -18.4C, respectively.



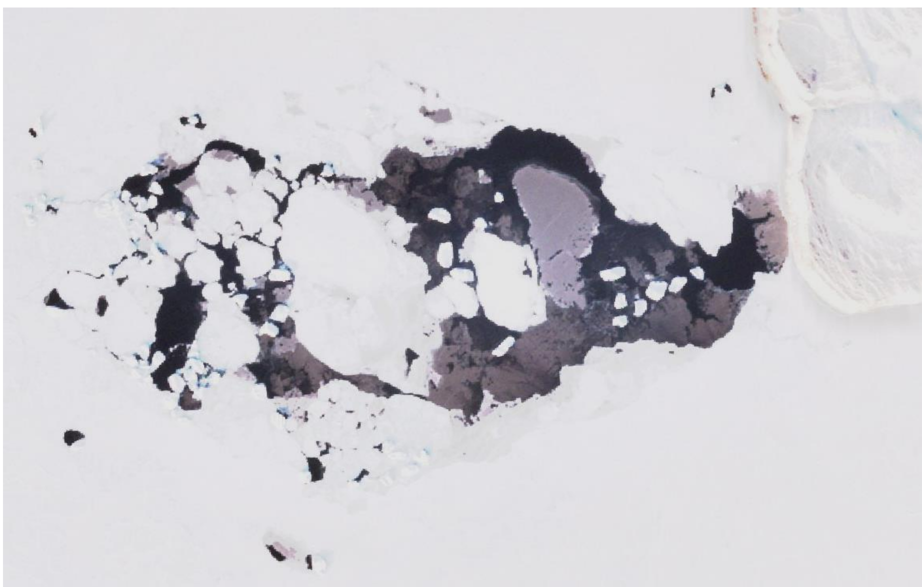
Summarizing our response, we would like to say that, although we generally agree that ERA5 reanalysis is not the best dataset for reproducing meteorological data in Nares Strait, we found that ERA5 wind and temperatures are only slightly biased with observations. The effect of such biases on the local thinning of sea ice is negligibly smaller then unknown snow accumulation rate and possible spatial variations of ocean heat flux – the parameters which effect was investigated with the ice growth model.

4. Line 415, "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": The message intended here is unclear from the present text. Revision is required.

I don't believe that it is plausible for a polynya to suddenly melt itself into existence – oceanic heat fluxes just aren't large enough. The more likely role of oceanic heat flux is keeping the ice relatively thin (and relatively weak), so that more powerful mechanical (fracturing, rafting, flooding and downstream advection, etc.) and thermodynamic (radiation) processes can do their work. Principal among the former are the stresses exerted by wind, wind-waves and tidal current on already thin ice. Once these open a polynya, new ice (mainly frazil and nilas) created by high rates of heat loss (> 200 W m-2) from the surface will continue to be removed by current, while insolation and downwelling short-wave radiation may deliver appreciable heat directly to seawater. This "tag-team" approach to creating a polynya in fast ice, involving both dynamics and thermodynamics, has been discussed by Topham et al., (1983. JGR 88).

Actually lifting warm water 100 m or more to the surface to open a sensible-heat polynya takes a large input of kinetic energy to the ocean, which cannot occur with a continuous cover of fast ice. It is only possible when strong winds act on mobile ice or open water. There is a paradox because necessity for strong winds is the same requirement for the opening of latent heat polynyas; the distinction between the purported "two types" of polynyas is not clear (see discussion in Melling et al, 2001). Moreover, because the wind must be integrated over a large expanse of mobile pack ice to accumulate enough kinetic energy to drive upwelling, the formation of small polynyas in fast ice, such as that off Cape Jackson, via this mechanism is unlikely.

The estimated heat flux was obtained based on the fact of presence of open water near Cape Jackson in early March. Below, we give an example of one of the earliest (March 16, 2020) good Sentinel-2 images showing polynya in more details. One may clearly see the presence of thin new ice along with the areas of open water. If water surface is ice-free, 200 W/m^2 does not seem to be extraordinarily high.



The mechanical processes maintaining water at Cape Jackson ice-free suggested by reviewer can work to a certain extent only. Occurring within very small, landfast-ice constrained area, none of these processes may prevent the polynya to eventually become covered with ice unless the ocean heat flux is strong enough to melt it. From this perspective, the situation at Cape Jackson is thought to be different from the polynya in Pioneer Channel discussed in Topham et al, 1983. The authors showed that the polynya was kept ice-free by a removal of new ice by strong wind (and, we suppose, by extremely strong tide with up to 1.2 m/s current speeds) that pushes newly formed ice beneath the landfast ice sheet. If such process had taken place at Cape Jackson, the *continuous accumulation of large amounts of frazil/new ice from polynya below landfast ice sheet would have resulted in much thicker surrounding ice close to edge (at least in certain directions). However, ICESat-2 data shows a more less linear increase of ice thickness from ice-free polynya area in all directions (Figure 3).*

*To address the reviewer's concerns, we changed the sentence in Line 497 (Section 4.1) as follows: "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.**"*

*We also added the followed sentence in Line 230 (Section 3.1): "**The appearance of open water near Cape Jackson in March is also evident in a few high-resolution Sentinel-2 images obtained in 2020 and 2021 (Fig. 3).**"*

*And we also changed the sentence in Line 358 (section 3.3): "**Within the polynya, in order to have open** water in May the heat flux should reach 70 W m^{-2}, **while a heat flux** above 200 W m^{-2} **is required to form an ice-free polynya** ~~to let polynya form~~ in early March"*

Addressing the other reviewer's comment, we would like to say that we never attributed upwelling near Cape Jackson to wind forcing. The kinetic energy is available from the consistent inflow of AW through Smith Sound. This flow has nothing to do, but overflow the ridge separating the semi-enclosed trough in Peabody Bay from the main Nares Strait channel. We understand that it's difficult to prove this suggestion without in-situ current measurements, T/S data and water sampling, but our whole paper is a combination of lines of indirect evidence that all together support the hypothesis of upwelled heat.

*In respect to the concern about "lifting warm water 100 m or more", please see our comment in your Assessment overall. We changed the sentence in Line 536 (Section 4.1) as followed: "This heat may either be upwelled **over the mid-basin ridge closer** to the surface ~~(leading to formation of sensible heat polynya at Cape Jackson)~~ **and/**or transported upward **to the lower surface of sea ice (or to the ice-free polynya)** by vertical mixing."*

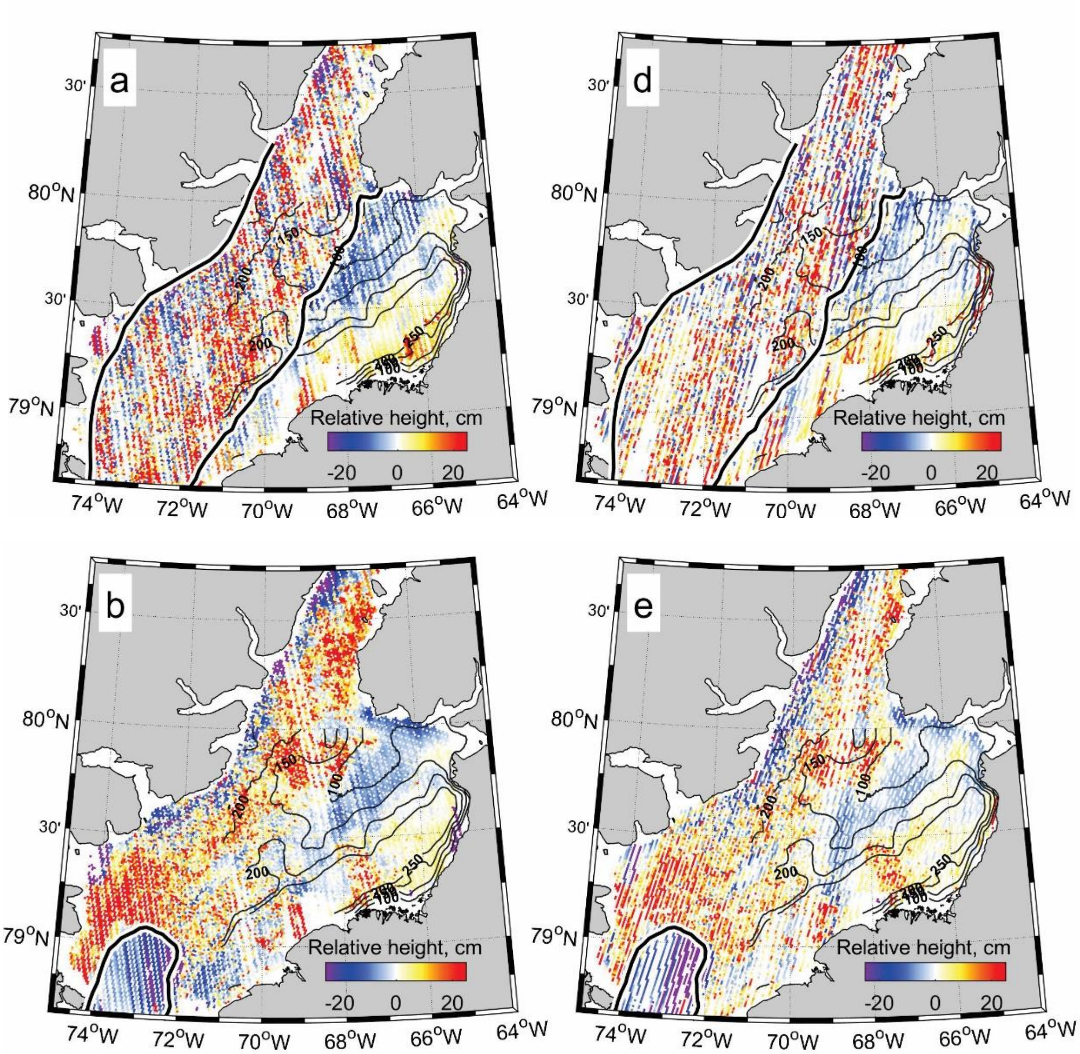
5. Lines 447-473: The speculation about iceberg melting provides an intriguing diversion, but it doesn't add much to the concepts central to this paper. I suggest that it be removed from the paper.

We agree with the reviewer. Although the process of iceberg base dissolution may support the idea of a steady ocean heat transport in the bottom layer of Peabody Bay, we don't have any solid evidence of its occurrence. Therefore, we have to admit that it adds little to the main idea of our paper. This part was removed completely.

6. Section 4.2, “The formation of ice thickness anomalies along the western coast of Nares Strait”: This is indeed an interesting feature. It did occur to me that is might possibly be an artifact in the southern half of the strait to the authors’ referencing of elevation to the mean value measured along the full length of the strait. Has the possibility been investigated that the higher sea level in the north that drives the current might explain this anomaly?

Although using the along-track anomalies generates interesting results, we do understand all its weaknesses. It can be used only at short distances where dynamical/steric variations of SLH are relatively small. The narrow Nares Straits is believed to be such a place, but even here the along-track and tracks-to-tracks changes of sea level may affect the obtained results.

However, the wedge of negative anomalies along the western coast is certainly not related to this weakness. The observed coastal zone is too narrow and has a very strong local gradient of elevations for being explained by spatially varying reference level. It’s also worth noting that the pattern and magnitude of anomalies are very similar for descending (~along the strait) and ascending (~across the strait) ICESat-2 tracks (see figure below).



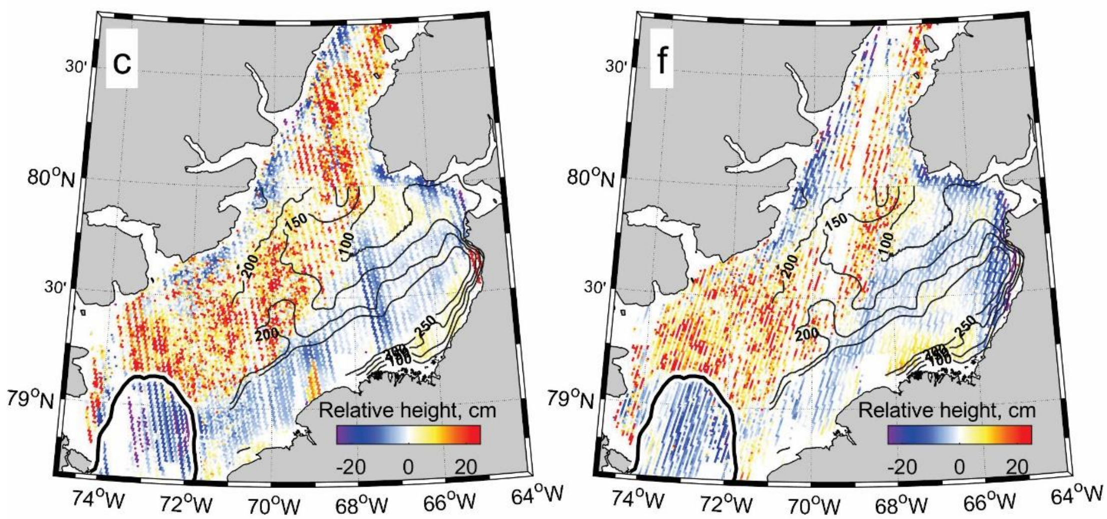


Figure. The ascending (a-c) and descending (d-f) along-track anomalies of ATL07 heights averaged over 1.5x1.5 km cells in January-April 2019 (a, d), 2020 (b, e) and 2021 (c, f).

We changed the Line 277 (Section 3.2) for making the latest aspect clearer:

*"This discrepancy is partly attributed to the averaging of data **from both ascending and descending tracks (ICESat-2 repetition cycle is 90 days) that were** used to compute the **mean** $\tilde{h}$ **in each 1.5x1.5 km cell**, but **it is** a combining **of both these tracks** with different along-track regional means **together that seems to** result in **some** smoothing of spatial anomalies presented in Fig. 4. **Note, however, that both the patterns and the magnitudes of anomalies are very similar when calculated from only descending (~along the strait) or ascending (~across the strait) tracks (not shown).**"*

Alternatively, could it be a manifestation of the undercurrent that hugs the Ellesmere coast when fast ice covers the strait? It is interesting that the elevation anomaly has roughly the same 10-km width as the undercurrent mapped by Rabe et al (2012; Fig. 4b). A connection to the dynamic relief reflecting geostrophy in this flow is implausible; it is only about 10% of the measured 20-cm drop in ice-plus-snow elevation adjacent to the coast. The inverse barometer effect, which might also contribute to lowered sea level in response to higher SLP at the western shore (not resolved at this scale by ERA5) is also too small: Sea-level pressure is only higher by 2-4 mb on the Ellesmere side of the strait (Samelson and Barbour, 2008: Fig. 8). Nonetheless, I recommend noted both possibilities as having been explored.

We appreciate the reviewer's help in attempting to attribute the observed anomalies to factors other than ocean heat flux. We agree that the suggested factors should not have a large effect on the observed anomalies. The inverse barometer effect is believed to form a smooth gradient of sea level across the entire strait, not a large gradient within the narrow 10-km wedge and a relatively smooth pattern over the rest of the channel. A connection to a geostrophic balance is also implausible. The mentioned dynamic relief of the geostrophic flow is less than 2 cm across the channel (Munchow et al., 2006), but sea level rises towards the coast of Ellesmere coast though it has to be opposite to explain the negative anomalies in ICESat-2 data.

Also, see our answer to the minor comment to Line 133-134) and the proposed changes in the text.

7. Line 484, "We suggest that the observed negative anomalies are attributable to the heat upwelled from the underlying mAW": I believe that this suggestion has merit, but that the details are incorrect.

The flow structure beneath fast ice in Nares Strait depicted by Rabe et al (2012; Fig. 4b), displays a jet of roughly 10-km width against the Ellesmere shore, centered at about 80-100-m depth. The baroclinic adjustment of the ocean to this jet (not shown) involves downwelling below the core of the flow and upwelling above. This leaves no mechanism to raise mAW through the core of the undercurrent to the surface on this side of the strait. Indeed, the cross-strait circulation that compensates for downwelling of mAW on the western side is upwelling on the other side, near Greenland!

However, upwelling does occur above the core of the jet. This would bring Pacific Winter Water as much as 0.2C warmer than the surface-freezing temperature (see Melling et al. 1984 Cont. Shelf Res 3) to the base of the surface mixed layer (see Melling et al. 1984 Cont. Shelf Res 3). This sensible heat in this Pacific Water could provide a heat flux to the underside of the sea ice via entrainment into the turbulent surface mixed layer. The needed turbulence kinetic energy could originate in part from brine-driven convection (ice growth) in the mixed layer and in part from shear between rough immobile ice and the rapid tidal flow. Melling et al. (2015) estimated an oceanic heat flux to the base of ice as 15 W/m2 under similar circumstances in Penny Strait, which would be sufficient to melt about 0.5 cm/d from 3-m ice (with 10-cm snow) at -25C. It should be noted that the submerged jet, and the upwelling above it near the western shore, do not exist when the ice is moving, so that the oceanic flux would be much reduced in years without a fast-ice cover.

*The word "upwelled" was definitely used there by mistake. We didn't really imply that it's <u>upwelling</u> that is responsible for heat transport towards the surface along the western coast of Nares Strait. The next sentence in the text clearly showed what we meant. We changed "**upwelled**" to "**transferred**" and also mentioned Winter Pacific Water as a possible source of sensible heat transferred to the base of the landfast ice along the western coast. Citations of Münchow et al. (2006) and Jones et al. (2003) were added.*

However, we agree with the reviewer that the mentioned upwelling above the core may facilitate the heat from the Pacific Water layer (which warmer temperature may just reflect an upstream mixing with the underlying mAW layer; see our answer to the major comment #12) reaching the base of sea ice in winter. The followed sentence was added in Line 581 (Section 4.2):

*"We suggest that the observed negative anomalies are attributed 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 southward current in winter induces upwelling above the core that may shift the upper thermocline water closer to the surface along the Ellesmere coast (Rabe et al., 2012; Shroyer et al., 2017) and, as a result, forms more favorable conditions for a larger heat transport to the bottom of sea ice here**."*

We deleted few sentences further in the text:

~~"The temperature in the northern branch of mAW in Kane Basin is about 0.3 °C higher compared to the temperature in the southern branch (Fig. 9c, d). In combination with higher current velocities within the subsurface jet (Fig. 9a, b) this would result in increased shear instabilities within the flow and higher upward heat flux that may have considerably stronger impact on ice growth compared to the northwestern Peabody Bay.~~"

and modified sentences in Line 607:

"Transformation of these currents over steep topography generates baroclinic semidiurnal tidal wave that may considerably enhance vertical mixing through benthic stresses and shear instabilities (Davis et al., 2019). From this perspective, the fact that most of the western polynyas first appear near prominent headlands (Fig. ***11****) generally support the idea* ***that the enhanced heat fluxes along Ellesmere Island are attributed to the*** *topographically controlled instabilities associated with the mean current and reversible tidal flow.* ***Another mechanism that may enhance the heat flux in the area is associated with the sub-ice turbulence generated by interaction of tidal flow and the very rough under-ice topography (Ryan and Münchow, 2017). In combination with the upwelling of the upper thermocline water along the western coast in winter (Shroyer et al., 2017), this mechanism may result in a considerable increase of vertical heat flux towards the bottom of sea ice.****"*

8. Lines 504-505, "Transformation of these currents over steep topography generates baroclinic semidiurnal tidal wave that may considerably enhance vertical mixing through benthic stresses and shear instabilities": I suggest that the steep cliffs on the western shore, indicative of deep water close to shore, make turbulence and internal waves generated in the benthic boundary layer irrelevant to the ice far above. However, I believe there is a good possibility to generate strong turbulence, mixing and entrainment through the action of the tidal flow (Pite et al., 1995. JPO 25) on the very rough under-ice topography of Nares Strait (Ryan & Munchow, 2017).

I suggest that the authors give some thought to this alternate, and I believe more plausible, explanation for the source of ocean sensible heat.

See our response to the previous comment and also the suggested changes in the text.

Not for the paper, but we want to point at on thing related to the idea of sub-ice tidal mixing that makes us confused. The semidiurnal tide forms a standing wave pattern in Kane Basin with the lowest tidal velocities in its central part (Davis et al., 2019). If so and if the negative anomalies along the western coast are tidally driven, they should have become smaller around Cape Frazer. However, we don't clearly see it in Fig. 4b-c.

9. Line 523, "weakens the cohesion of landfast ice against the shoreline in Kane Basin": The tidal cycles in sea level ensure that the ice sheet is always fractured at the coast, not bonded to it. However, the word cohesion implies that the authors consider that bonding of ice to the shoreline is important. This line of thought runs contrary to decades-old discussions of fast ice in deep water, where it is the formation of ice arches across channels which stops the movement of ice behind them, not shear strength at the shoreline. The upwardly convex shape of a masonry arch is the key feature that allows it

to resist downward loading; the shape ensures that all the stone in the arch is under compression, the stress state in which it is strongest. Indeed the stress is highest within the wedge-shaded stones of the arch and much less above them. Pack ice also is strongest in compression and much weaker in shear. Although there are likely several arch-shaped load-bearing features distributed in the fast ice along the length of Nares Strait during any winter, much of the fast ice cover will be in a low state of stress; cohesion at the shoreline is probably unnecessary for fast-ice stability, although its confinement by irregularly shaped shorelines may constrain it from moving locally. Conversely, weakening of that confinement by melting at the coast may allow it to shift around in response to wind and tide. It is quite common to see the ice in Kennedy Channel become mobile between arches at its northern and southern ends long before the collapse of the arch in Smith Sound allows the ice in Kane basin to do the same. The same phenomenon is seen annually in Prince Regent Inlet. I don't think that the authors' argument for up-channel polynyas hastening the break-up of fast ice further down-channel has much merit, as presently written. It is possible, of course, that phenomena may be correlated in time because of the influence of a third circumstance not identified.

We understand this reviewer's concern about weakening the cohesion and its potential impact on ice bridge break-up. There is no doubt that it's the arch in Smith Sound that keeps the entire Kane Basin bridge in place. Indeed, we used the fact that breaking and moving of ice in the middle of bridge often occurs before the arch collapse. And suggested that it may facilitate the following collapse because mobile sea ice in the middle of bridge may gain more kinetic energy (from wind, mean flow fluctuations or tidal current) and put additional load on the arch that is becoming less and less strong in summer due to gradual thawing.

We agree that more explanations are needed in the text. We changed the Line 628 (Section 4.3) as followed:

"Based on the results presented here, we ***suggest*** *that thinner coastal ice, formed under conditions of enhanced oceanic heat flux, weakens the cohesion of landfast ice against the shoreline in Kane Basin.* ***We can further speculate that such weakening may facilitate*** *an earlier ice bridge break-up (comparing to a supposed no-polynyas situation)* ***as it leads to formation of patches of mobile ice in the middle of the ice bridge in Kane Basin. While shifting around, this ice may gain some kinetic energy from wind and tide and eventually result in additional dynamical load on parts of the bridge that still remain in place.****"*

10. Line 528-529, "*This break-up appeared to release internal stresses in the ice bridge and led to concomitant ice cover break-ups in the main channel*": This statement appears to rely upon a knowledge of the dynamical state of the ice cover. Nothing is known about stresses. In reality all you have access to is evidence of deformation (in the form of cracks) and of motion. Also see comment #9.

We agree with this criticism. The sentence was changed as follows: "This break-up appeared to ***initiate the further fracturing of*** *ice cover in the main channel."*

11. Line 531: This paragraph gives the impression that the polynya has played a role in the breakup, but really all that you demonstrate is that the breakup was correlated with expansion of the polynya.

Perhaps the expansion of the polynya is just one event in the process. A more robust discussion, with a more useful take-away, would review the other factors in play, as listed in Line 520. If such completeness is thought to be beyond the scope of the paper, perhaps it should be covered in a separate paper. See comment #9.

We added new sentences (Line 628, Section 4.3) explaining how polynyas and thinner ice may facilitate the bridge breaking-up (see our response to comment #9).

Although we agree that the investigation of a complex possible role of polynya(s) in bridge collapse requires additional research, we think it is worth at least mentioning such a hypothesis even based on a simple correlation of timing of polynya(s) development and bridge collapse. And we noted honestly that (Line 646) "although our hypothesis that polynyas facilitate ice bridge break-up in Nares Strait is speculative, we would like to emphasize the observations that the first movements of the immobilized ice cover occurred in areas with negative ice thickness anomalies during winter and where polynyas are observed."

*We also changed the title of Section 4.3 to "The **inferred** role of thinner ice in Kane Basin in ice bridge break-up".*

12. Line 539-540, "The only oceanic heat source available to maintain such a polynya through winter is the modified Atlantic Water": This is not true. It may be the warmest source, but it is not the one closest to the ice. My comment on Line 484 raises the possibility that the less conspicuous warmth of the Pacific Water might be more influential than you give credit for. I recommend that you re-think the paper with this in mind.

Although we don't support the reviewer's idea that colder Pacific Water plays a greater role as a source of sensible heat transferred to the sea ice, we agree that this source has to be mentioned. Our major initial mistake was related to not ignoring this water mass, but to suggesting that its heat is associated with a thermal impact of underlying Atlantic Water. Based on summer observations, Jones and Eert (2006) showed that surface mixed layer occupied ~90-100 m of the water column at the western side of Kennedy Channel. From this depth, where the fraction of Atlantic Water was 20-30%, water temperature started to increase. It made us think that the elevated temperatures in the Pacific Water layer below the surface mixed layer (within the upper thermocline) is simply attributed to this AW fraction and the upstream mixing with the underlying AW core.

Without specialized experiments and in-situ observations, we can only give credit to Pacific Water as another potential source of ocean heat limiting ice growth, but not provide solid proof of its greater input compared to mAW.

Addressing this concern, we changed the text in Lines 386 (Section 3.4), 526 (Section 4.1) and 581 (section 4.2) as follows:

*"This flow occupies the entire water column and consists of 3 distinctive layers; i) cold brackish polar mixed water within the upper 50-60 m, ii) the **upper thermocline coinciding with** halocline **that is** observed at 70-110 m and (iii) the relatively warm underlying modified Atlantic Water (mAW) which originated in the North Atlantic and was transported a long way from Fram Strait*

into the AO and to Northern Greenland (e.g. Melling et al., 2001). ***The first two layers mainly consist of water of Pacific origin (Jones et al., 2003; Jones and Eert, 2006).****"*

"The only available source of ocean heat in Kane Basin during winter is associated with the relatively warm modified Atlantic Water penetrating into the basin from the Lincoln Sea (northern branch) and Baffin Bay (southern branch). ***The inflow from the Lincoln Sea may also transport some heat with Pacific Water below the surface mixed layer. However, this heat may just reflect an upstream mixing with warm underlying mAW. For instance, based on the data collected in Kennedy Channel, Jones and Eert (2006) showed the fraction of Pacific and Atlantic Water in the upper part of thermocline at depth 90-100 m of about 70-80% and 20-30%, respectively.****"*

"We suggest that the observed negative anomalies are attributable to the heat ***transferred towards the base of the landfast ice*** *from* ***either the upper thermocline mainly consisting of Pacific Water in this area (Jones et al., 2003) or*** *the 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, to form favourable conditions for a larger heat transport to the bottom of sea ice here."***

And also in the Abstract and Summary:

"This work provides new insight into the Nares Strait ice bridge, and highlights that ***an impact of*** *warm****ing*** *modified Atlantic and/or Pacific Waters entering the Strait* ***may*** *contribute to its [bridge] further decline."*

"The ice thickness anomalies along the western coast were considered to be associated with heat released ***either from the upper thermocline water of Pacific origin*** *or from the* ***underlying*** *mAW that carry relatively warm water southward from Lincoln Sea."*

Comments (minor)

Line 27: Shokr et al. (2020) is a weak reference for the role of the along-channel sea-level in driving flow down Nares Strait. Münchow & Melling, J Mar Res 66, doi.org/10.1357/002224008788064612 would be much better.

The reference to Münchow and Melling (2008) was added.

Line 30-31, "The ice bridge also helps prevent the loss of the thick, old ice from the Last Ice Area": The paper cited (Moore et al, 2019) is not helpful in substantiating this statement; it has very little to say about Nares Strait. To my knowledge, there has not yet been a study demonstrating that ice loss from the LIA, as distinct from ice export through Nares Strait, is reduced during years when an ice arch forms there. Nares Strait is only one of four pathways (and the narrowest) via which ice leaves the LIA – the others are to the NE via Fram Strait, to the SE through the QEI and to the SW to the Beaufort Sea. It is quite plausible that a blocked Nares Strait simply creates a diversion of ice to one of the other pathways, most likely Fram Strait. You need a citation that demonstrates convincingly that this is not so.

The reference to Moore et al. (2019) here was used here only as a reference to the Last Ice Area, not to confirm the statement in the beginning of this sentence. To address this uncertainty, we just moved the reference into the relative clause and specified that the loss <u>*through Nares Strait*</u> *was meant: "The ice bridge also helps prevent the loss of the thick, old ice* ***through the strait*** *from the Last Ice Area* ~~*(Moore et al., 2019)*~~*, located north of Ellesmere Island and Greenland* ***(Moore et al., 2019)****, by hindering its transport south, …"*

Line 38, "… peak in the fraction of sea ice with a draft between 2.6-2.8 m": It is important to note here, as was in the cited paper, that this range in draft was computed on the assumption of no snow cover, which may bias values appreciably high. Also, a referenced estimate of the empirical accuracy in draft estimates from CryoSat freeboard should be included here.

We added that these estimates were made under no-snow assumption. However, 2.6-2.8 m range represents a mean characteristic that is not directly related to the accuracy of individual ICESat-2 readings. The same for the recent paper – we used the elevation anomalies averaged over 1.5x1.5 km cell (Fig.4) that means ~200 readings per a single pair of ascending and descending tracks or even more for repeated tracks. With a nominal accuracy of ICESat-2 measurements of few centimeters, the accuracy of the calculated elevation anomalies is at least ~15 times smaller (few millimetres).

The followed sentence was added in Line 153 (Section 2.2):

*"**Even though the accuracy of individual ICESat-2 readings is relatively high (less than 5 cm, Brunt et al., 2019), the accuracy of the averaged anomalies calculated with this method is estimated to not exceed a few millimeters.**"*

Lines 46-47, "That bridgeless years only occurred during last 15 years underscore a general shortening of bridge existence period and point to changes …": It would be appropriate to clarify that this statement refers to the absence of an ice bridge at Smith Sound (think) and not to the much smaller number of years when there was no bridge anywhere between Baffin Bay and the Arctic Ocean.

In this clarified context, it should then be noted that there was one winter (1995) in the 1990s with no arch at Smith Sound – in 1995 the arch formed at Hans Island – and one (1993) essentially like 2007 with no arch anywhere; "essentially" because an arch in Smith Sound that year lasted only 10 days (Vincent 2019). With a 30-year perspective, the record looks less amenable to interpretation via trend: there is a cluster of 2 of 3 years with no arch at Smith Sound in the mid-1990s, then an 11-y period with annual arches, then a cluster of 3 of 4 years with no arch in the 2nd half of the 2000s, then a 6-y period with annual arches, then a cluster of 2 of 3 years with no arch in the second half of the 2010s. Disregarding clustering and estimating the probability of no bridge in any year from the data as 7/31, one uses the Poisson Distribution to estimate the likelihoods of the observed gaps between no-bridge winter – that is having 2 no-bridge years in 2 years, 2 in 3 y, 2 in 7 y and 2 in 12 y. These are 6.4%, 11.7%, 25.7%, 24.4%. The low values for the small gaps suggest there is clustering in play; the relatively high values for the large gaps suggest that such wide gaps are not unexpected, so that bridging despite weak clustering, looks like a Poisson process. On these grounds I suggest a re-examination the statistical confidence of the statement in lines 46-47, which is based on such a short time series.

Thank you for bringing all these details up. We agree that 1993 and 1995 have to be also referred as bridgeless years according to the data reported by Vincent (2019). At least for Kane Basin. We changed the corresponding lines and also specified that we are talking about Kane Basin in this paragraph:

"Analysis of 16 bridge formations during the past two decades2001-2021 revealed that consolidation occurred at cold air temperatures (less than -15°C), around neap tide, and during a cessation or even reversal in the prevailing north-northeasterly winds in the strait. However, the bridge ***in Kane Basin may have*** *failed to form* ***even under atmospheric and oceanic conditions that are favourable for consolidation (Kirillov et al., 2021).*** *Based on AVHRR satellite data from 1979 to 2019, Vincent (2019) reported on a recent trend towards later formation and earlier breakup of the ice bridge.* ***The fact that the ice bridge failed to form only two times during the first two decades of observational records (in 1993 and 1995; Vincent, 2019) and six times during last two decades (in 2007, 2009, 2010, 2017, 2019 and the last bridgeless winter 2022)*** *underscore a general shortening of bridge existence period and point to changes in environmental conditions."*

Lines 48-49: I think that the date-based approach of Vincent (2019) is probably a more robust approach to a short 30-year time series than is the counting of the rare occurrences without arches, which the authors have used here.

See the changes made while answering the previous comment.

Line 54-56, "it is the sensible heat polynyas … that are more common in the Canadian Arctic (Hannah et al., 2009)": The authors appear to mis-quote Hannah et al. (2009), who state "… are widely distributed across the Canadian Arctic Archipelago"; Hannah at al. are clear that these sensible heat polynyas are features within fast ice in this region. Their map (Fig. 1) shows that the latent heat flaw-leads and polynyas that form along the perimeter of the fast ice are actually more widespread across the Canadian Arctic waters and occupy much more area.

*This criticism is fair. We used bad way of saying that sensible heat polynyas are commonly met in the Canadian Arctic. We re-wrote this sentence as followed: "****Beyond the NOW and other latent heat polynyas, there are several sensible heat polynyas that form within the landfast ice cover of the Canadian Arctic that are associated with warm subsurface waters opposing ice growth*** *(Hannah et al., 2009)."*

Line 67: Refer the reader to Fig. 1 for the mapped location of Cape Jackson.

Done as requested

Line 67 et seq., "… at Cape Jackson in the central part of the bridge": The terminology is confusing. I believe that most readers will consider the bridge to be the arch that forms the boundary between fast ice and mobile ice in southern Kane Basin. It follows that the central part of the bridge is the "top" of the arch, halfway across the strait between Greenland and Ellesmere. However in this sentence, the authors are referring to a location in fast ice more than 200 km “above” the arch. I recommend that the authors devise a different term to refer to locations within the fast ice “above” the arch. Simplest in this example would be “… at Cape Jackson, more than 200 km north-east of the bridge”.

Please see our response to the first major comment.

Line 93, “… maintaining water at Cape Jackson ice-free during winter”: The reality is ““… maintaining water at Cape Jackson ice-free at times during winter”.

Changed as requested.

Line 94 et seq., “under the bridge”: See comment re line 67. I recommend using the phrase “beneath the fast ice” for the reason already given.

See our response to the first major comment.

Line 100: Line 67: Refer the reader to Fig. 1 for the mapped location of Peabody Bay.

Reference was added.

Line 130 et seq., “crossing the bridge”: See comment re line 67.

See our response to the first major comment.

Line 132, “Although ATL07 data are manifested to be adjusted for geoidal/tidal variations and inverted barometer effects”: The correction for the inverted barometer effect is probably only accurate in wide deep ocean basins where the long ocean wave which is the ocean’s response to changing atmospheric pressure can move as fast as, and in the same direction as, the SLP anomalies moving at 20-25 m/s. I suspect that the correction will not work well in a long (550 km) narrow (35 km) strait. I urge the authors to find and reference research that provides a discussion of the accuracy of the inverted barometer correction in confined coastal waters.

This is a good point. Although the depth of the main channel (>200 m) allows long wave to travel with the speed of more than 45 m/s, we generally agree that the inverted barometer effect may work not very well in narrow Nares Strait. However, we don’t think it somehow affects the obtained results. Even if the strait would have reacted “normally” to changing SLP (as in wide deep ocean), the width of the strait is too small for spatial SLP variations having a large effect on the cross-channel sea level difference. And all observed large coastal anomalies are either too small (at Cape Jackson) or too narrow (western coast) for being attributed to this factor. See our response to the major comment #6 for more details.

Line 133-134, “… may still contain unknown uncertainties related to the regional synoptic variability of sea level associated with wind forcing and/or with ocean dynamics”: With respect to the atmosphere, I recommend replacing “wind forcing” with “strong wind, <u>air-pressure</u> and ocean dynamical effects on the mesoscale (10-30 km)”, referencing Samelson and Barbour (2010).

We don’t really think that varying <u>air pressure</u> may considerably affect the amplitude of observed ICESat-2 anomalies, even if the inverted barometer effect is not adjusted well in Nares

Strait (see our response to the previous comment). The main discovered anomalies (at Cape Jackson and along the western coast) are relatively small/narrow for being affected by any of mentioned uncertainties from our point of view. But they have to be mentioned, of course.

We changed the sentence accordingly.

With respect to the ocean, Münchow & Melling (J Mar Res 66) provide estimates of the anomalies of sea-level height relative to the mean. These have amplitudes as large as 10 cm along-channel and a few cm/s across-channel. These along-channel value is large enough to contribute appreciable fortuitous NE-SW varying anomalies in thickness that are computed relative to an along-track (approximately along-channel) mean. This source of error requires discussion.

With all respect to the reviewer, we don't agree with this point of view. The reference to the sea-level differences obtained from the tidal gauges deployed at Alert and in the interiors of the fjords at both sides of Smith Sound can't be used as a good argument. All those gauges were installed in a relative vicinity to the mobile ice areas and, therefore, may contain a large portion of variability attributed to the dynamical effects associated with local wind and currents. One can see this variability in the Münchow & Melling's Fig. 14 as a continuous alteration of differences between positive and negative values. The magnitude of sea-level gradients below the central part of the ice bridge and its temporal variability in winter remains unknown.

However, it's not the main reason why we would like to reject the reviewer's concern. We have already mentioned that from our point of view the spatial configuration of the observed ICESat-2 anomalies doesn't admit any other explanation rather than the local ocean heat impact. Those areas are either too small (at Cape Jackson) or too narrow (along the western coast, aligned along-channel) for letting other factors explain the large gradients of anomalies observed in these areas.

The distance between individual ICESat-2 tracks is 3.6 km. It implies, that MOST of 1.5x1.5 km cells accumulate data from a single ascending and a single descending track. MOST, because there are some tracks that were repeated twice: ICESat-2 repetition cycle is 90 days and we used 120-day (January-April) period when calculating anomalies in Fig. 4. Even from this perspective, the random variation of cross- and along-channel sea-level differences (from Münchow & Melling) would have resulted in random elevation anomalies along adjacent tracks (=cells) made at different dates. However, the observed anomalies demonstrate a fair correlation between nearby cells and, moreover, the spatial pattern of anomalies was found to be very similar when calculated by using only descending or ascending tracks (see our response to the major comment #6). Such result would be highly implausible, if the along- or across-channel sea-level gradients (with their altering directions) determine the observed elevation anomalies.

To address this concern and show why we think sea-level gradients or SLP could not explain or even contribute much to the formation of anomalies, we added the followed sentences in Line 286 (section 3.2):

*"**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 level at Greenlandic side. 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."

Line 139-140, ">0.3 m mean snow depth in Kane Basin. However, as we will show later, this height seems to be overestimated". Reference to Samelson and Barbour (2010) is again appropriate, since the extremely strong winds common in Kennedy Channel and the vicinity of Cape Jackson (see also Melling, Oceanography Mag, 2011) may indeed provide a strong disincentive for the accumulation of snow.

Thank you for this comment. However, here we are talking about the central Kane Basin and Peabody Bay. We don't think the orographic effect plays the same role in snow accumulation rates as it does in Kennedy Channel.

Line 159, "… generally have good agreement with the mooring records": It is necessary to provide an assessment that is more specific in relation to the comparison of model with data in relation to the cross-channel scale of flow features, their positions cross-channel and in depth and their intensity. Can the countercurrent on the Greenland side be simulated?

To address this concern, the followed sentences were added in Line 180 (Section 2.3):

"It was found that both vertical and cross-channel distributions of temperature/salinity and current velocities in FESOM2 simulations generally have good agreement with the mooring records in this region (Münchow & Melling, 2008; Rabe et al., 2010; Münchow, 2016). ***For instance, the model reproduces well the shift of the southward jet towards the Ellesmere coast (at ~1/4 of the channel width) and also the existence of countercurrent on the Greenlandic side, although with lower velocities. The mean modeled temperatures and salinities both demonstrate the presence of cross-channel gradients towards Greenland that become stronger at depth that is in a good accordance with observational data. In addition, the model fairly reproduces the uplifting of isohalines and isotherms over western slope in winter.***"

We can confirm that the countercurrent on the Greenland side was fairly reproduced by FESOM-2. Similar to the simulation results of Shroyer (2015), this flow is only presented as undercurrent in winter. See the figure in our response to the major comment #2.

Line 182, "MODIS imagery confirm that a polynya is present every winter at Cape Jackson": The sentence that follows that quoted indicates that the following is more precise: "MODIS imagery confirms that in every winter when fast ice fills the strait, a polynya appears at Cape Jackson late in the season".

Thank you for helping to make this sentence clearer. It was changed as followed: "MODIS imagery confirm that ***every winter when the ice bridge is formed in the strait*** *since the MODIS observations began in 2000**,** a polynya* ***appears at Cape Jackson late in the season****".*

Line 190, "may indicate either the ice-free surface or thinner ice": Clarification, "may indicate either the ice-free sea surface, locally thinner ice, locally thinner snow or both the latter".

Changed as requested. Thank you for this comment.

Line 205, "… If 50% of the 0.26 m surface elevation is attributed to a snow layer …": The occurrence of very strong, very turbulent winds off sea capes is well known to mariners. Cape Horn and Cape Farewell, at the southern tip of Greenland, are perhaps the most famous. See Winant et al. (1988) J. Atmos. Sci. 45. Such conditions would be very effective at scouring snow from the surface of sea ice and moving it downwind. It is therefore quite plausible that both ice thickness and snow depth become thinner on approach to Cape Jackson, as the density-stratified oceanic and atmospheric flows accelerate in response to submarine and subaerial topography blockage, respectively. IceSat may be sensing environmental response to both these effects, not just to one or the other.

Thank you for this comment. However, in this paragraph we just demonstrated how much the snow layer could contribute to the ice thickness if the observed 0.26 m anomaly <u>away of the polynya at Cape Jackson</u> is partly associated with the snow.

Further in the Section 3.3, we also used 1D ice growth model to investigate the effect of snow (and ocean heat) on ice growth. But we also applied different snow accumulation rates to simulate the ice growth at some distance from polynya. In the polynya, with very strong winds suggested by the reviewer, no-snow assumption was used. We made some changes in the text to get rid of these unclear parts and to underline that no-snow was used for modeling the evolution of ice thickness in the polynya.

Lines 218-238 & Fig. 4, "along-track anomalies averaged over 1x1 km squares": On the "basin-wide scale" discussed here, the anomalies, calculated relative to mean height of any ascending or descending track crossing the bridge between 55-76°W and 78.25-82.5°N, may well be contaminated by a varying along-channel gradient is sea-surface height – see comment on lines 133-134. It is appropriate that the authors acknowledge this source of error and discuss its impact on results.

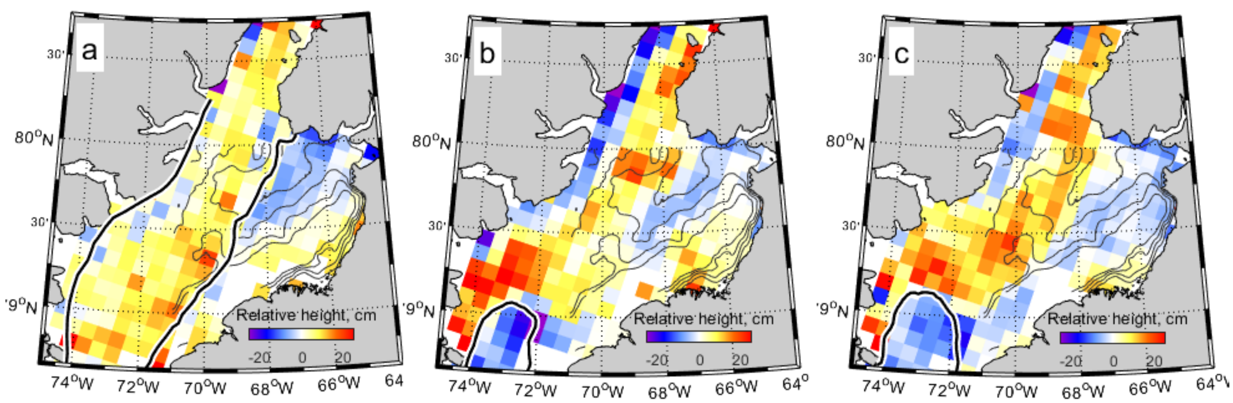
We already addressed this concern while answering the major comment #6.

There was also a mistake throughout the paper. We used 1.5x1.5, not 1x1 km, mesh. The 1x1 km corresponded to the old version of Fig.4. Corrected everywhere.

Lines 221-232 & Fig. 4, "In the main channel, the anomalies are highly irregular and form a speckled pattern, whereas the anomalies in Peabody Bay form a consistent pattern with positive anomalies in the southeast and negative anomalies to the northwest": It is unclear, with the continually moving ice of 2019, why the elevation anomalies are not smoothed out via averaging over time. The small scale of the speckle in elevation in 2019, not so different from that in the years with immobile ice is difficult to understand. Please explain.

For averaging, we used 1.5x1.5-km cells in order to keep data from the individual ICESat-2 tracks (~3 km apart from each other) separated. Therefore, time averaging mainly means averaging of a pair of ascending and descending tracks. See our answer to the major comment #6 and the corresponding changes in the text for more details.

Using coarser mesh gives smoother pattern of anomalies in the main channel in 2019, but also results in vanishing gradients in the areas with large anomalies and strong gradients (see the anomalies calculated at 10x10 km mesh in 2019, 2020 and 2021 in the figure below). We preferred to show the latest.



A similar speckled pattern of h was observed over the landfast ice in Peabody Bay in 2020 (Fig. 4b), but not in 2021. What is the application in these instances?

There is no particular application of these instances because they don't affect the main feature – the negative anomaly along the northern coast of Peabody Bay. The sentence was deleted. Thank you.

Lines 233-234, "The difference in surface height anomalies between the southeastern and northwestern parts of Peabody Bay is supported by a similar difference in the observations of Tb" : In what sense do we interpret "is supported by"? Do you mean "is correlated with" or is there some physics behind the claim of support?

Yes, "correlated with" seems to be a better way of saying what we meant. Changed.

Line 234: Interpretation of AMSR brightness temperature. Please clarify whether the values depend on emissivity (ice type) as well as on surface temperature (of snow, of ice, or of somewhere between?).

AMSR brightness temperature values depend on a combination of emissivity, surface temperature, and surface roughness (reflectance). To underline the difference between the actual surface temperatures and used brightness temperatures, we added the followed sentence in Line 131:

*"**Note neither AMSR2 nor MODIS brightness temperatures are 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 this study, we used T_b to highlight a temperature contrast between adjacent regions, but didn't interpret it as absolute temperatures of the ice/snow surface.**"*

Line 235: Should "southwest" be changed to "southeast"?

Yes. Thank you for finding this mistake. Changed to "southeast".

Line 280, "we applied the 1-D thermodynamic ice growth model": Things like thermal coefficients, snow density, short and long-wave radiation, cloud cover do matter. Please provide a quick overview of the properties of this model, or an equivalent citation.

*Changed to "…we applied the 1-D thermodynamic ice growth model **with the same parameters as in Kirillov et al. (2015).**"*

Lines 282-284. "We used 4 cm mo-1 snow accumulation rate to reach a modest snow thickness of 14 cm at the end of winter that is reasonably close to 19±2 cm obtained with AMSR2 data for Peabody Bay": As mentioned earlier, snow accumulation matching that in Peabody Bay may be unlikely. Ice off Cape Jackson may be blown clear of snow by frequent extreme winds in winter (see Samelson and Barbour, 2008: Fig. 6). It would be appropriate to mention this possibility.

We don't think there is any issue here. We used different snow accumulations to simulate the sea ice growth <u>away</u> from the polynya at Cape Jackson (Fig.6b) and <u>no-snow condition in the polynya</u> (Fig.6c). Also see our answer to one of the previous comments on this.

*For clarity, we changed this sentence as follows: "**Away of polynya,** we used 4 cm mo^{-1} snow accumulation rate to reach a modest snow thickness of 14 cm at the end of winter that is reasonably close to 19±2 cm obtained with AMSR2 data for Peabody Bay (not shown)."*

Lines 288-289, "For having ice-free water in May, the heat flux should reach 70 W m-2 and be above 200 W m^{-2} to let polynya form in early March": These estimates presume that there is no advection of newly formed ice downstream and beneath thicker pre-existing level ice and, I believe, that there is no insolation.

We already addressed this concern while answering the major comment #4.

In respect to insolation, 1D ice growth model takes the incoming shortwave radiation into account (Kirillov et al., 2015). However, the estimated 200 W m^{-2} were obtained for early March when the sun just starts rising above horizon at this latitude.

Lines 440-441, “Although the northern branch is warmer and, being considerably faster, transports more heat compared to the southern branch …”: Unfortunately, the northern branch is partially blocked from entering eastern Kane Basin by a shallow (70-90 m) spur extending more than 100 km southwest from Cape Jackson. The deepest crossing is relatively shallow, a 220-m sill at 79 40’N close to the Ellesmere shore. Moreover, because of geostrophic adjustment in the Arctic outflow, the warm mAW is at it deepest on the western side of the basin. To make a convincing argument about the temperature of the water that gets over this sill, more careful thought is needed. Where does the mechanical energy to lift water of the sill come from? I don’t believe that a numerical model unvalidated in Nares Strait is a substitute for data needed to substantiate an hypothesis. Perhaps the authors could strengthen their case by exploring what the model has to reveal about the energetics of the phenomenon that they propose?

We don’t understand this reviewer’s concern. We never mentioned that the northern branch penetrates into Peabody Bay (Fig.7) and somehow affects the observed anomalies there. In this sentence, we just underline the fact that even though the southern branch is colder than the northern one, it is warm enough to have a thermal impact on the sea ice. However, understanding why this concern appeared, we changed this paragraph considerably as follows:

“The only available source of ocean heat in Kane Basin during winter is associated with the relatively warm modified Atlantic Water penetrating into the basin from the Lincoln Sea (northern branch) and Baffin Bay (southern branch). ***The inflow from the Lincoln Sea may also transport some heat within the upper thermocline layer consisting of Pacific Water. However, this heat may just reflect an upstream mixing with warm underlying warm mAW. For instance, based on the data collected in Kennedy Channel, Jones and Eert (2006) showed the fraction of Pacific and Atlantic Water in the upper part of thermocline at depth 90-100 m of about 70-80% and 20-30%, respectively.*** *Although the northern branch* ***of mAW*** *is warmer and, being considerably faster, transports more heat compared to the southern branch,* ***this water is thought to not to be present in Peabody Bay and can be mainly found in the western part of Nares Strait (Fig. 7a-b)****. According to FESOM-2 simulations, the mean temperature of* ***the southern branch of*** *mAW core in the central Peabody Bay is -0.15 °C or ~1.75 °C above freezing with a maximum observed at depth below 200 m. This heat may either be upwelled* ***over the mid-basin ridge closer*** *to the surface (leading to formation of sensible heat polynya at Cape Jackson)* ***and/****or transported upward* ***to the lower surface of sea ice (or to the ice-free polynya)*** *by vertical mixing…”*

Lines 456-457, “However, it is noteworthy that all these iceberg chains are located within the region with pronounced negative anomalies of ice surface heights in 2019 and 2020”: Qualitatively, from the insets on Fig. 7, I estimate that the bergs cover only perhaps 10-20% of the sea surface; they could create point sources of turbulence kinetic energy through interactions with current, but are likely too sparse to form an area-wide source to explain the sea-surface anomalies which are manifest on the scale of the entire basin. Moreover, the warm seawater contacting icebergs at depth has plenty of opportunity to transfer its heat directly to the bergs, rather than hoarding to create havoc on the sea ice. The authors’ hypothesis is plausible, but it needs appreciable quantitative physics to convert it into an explanation appropriate to uplift from 100-250 m depth.

Even covering only 10-20% of total area within the mentioned chains, these icebergs represent a dense fence with no strait passages allowing flow to pass these chains undisturbed. One may *suppose that the flow jostles through the icebergs, change direction of streams and form a complex highly variable dynamics in this area.*

However, following the reviewer's recommendation (the major comment #5), this part was completely removed.

Lines 460-461, "However, the melting in this case is associated not with latent heat flux from water, but with dissolution controlled by solute transfer between water and ice-ocean interface (Woods, 1992)": I don't understand this point. I believe that a transfer of sensible heat to the iceberg is still required to free individual water molecules from the crystal lattice as dissolution proceeds. Please check whether you are citing Woods' work correctly.

The reviewer is right - it was not written correctly. Of course, it's the heat flux that eventually melt the glacial ice. We just meant that the dissolution process and its rate are controlled by salt exchange. This part was completely removed, so there is no need of correction.

Lines 496-497, "The stronger vertical mixing associated with the shear instability of the subsurface southward jet along the western coast … . This statement is speculative and not supported by observations. It is trivial to show with data in the Rabe papers that the gradient Richardson Number in the shear layer above the jet is about 2.2, almost 10x the threshold for shear instability. The most plausible sources of turbulence kinetic energy are in the wintertime mixed layer, namely shear in tidal currents at the base of rough sea ice and, less important with thick ice, brine-driven convection. Both can be estimated. I recommend that the authors do so.

The Lines 611-615 (Section 4.2) were changed to address this concern. See our response to the major comment #7.

However, with all respect to the reviewer's opinion, we don't think that geostrophic velocities from Rabe et al. (2012) represent appropriate dataset for estimating Ri numbers. The good choice would be using the data of individual pings from 75kHz ADCP deployed at KS02 in 2003-2006. However, to our best knowledge, the highest frequency domain analysed and published based on those records was limited to the tidal variability (Munchow and Melling, 2008). But even if we had access to those records, we think such analysis is beyond the scope of the current research and it should be investigated in a separate paper.

Lines 506-507, "generally support the idea of topographically controlled instabilities associated with the mean current and reversible tidal flow": I don't think it necessary to speculate about submerged topography generating instabilities. Headlands, by partially blocking along-shore currents, are notorious for strong tidal currents, and under-ice topography in Nares Strait is very rough.

Our main objection to this reviewer's comment is that headlands can be found in many places at both sides of Kane Basin and Kennedy Channel, but it is only the western coast where negative anomalies of elevations (and, likely, ice thicknesses) are observed. We still persuade the idea that it is the southward subsurface flow amplified by semidiurnal tide, that generates the instabilities along its pathway. However, we gave credit for the mechanism related to the interaction of tidal flow and under-ice topography (see our response to the major comment #7)

Line 511, “probably through the local upwelling”: What is the basis for “probably”. I don’t believe that there are any soundings in Flagler Bay, so the existence of a sill is speculative.

It is a speculation based on a general understanding how the polynya in Flagler Bay can form. We could not figure out any other mechanism maintaining this polynya rather than a tidal upwelling, but said “probably” exactly because of the absence of information about water dynamics and bathymetry there.

This part was deleted in the new version of the text.

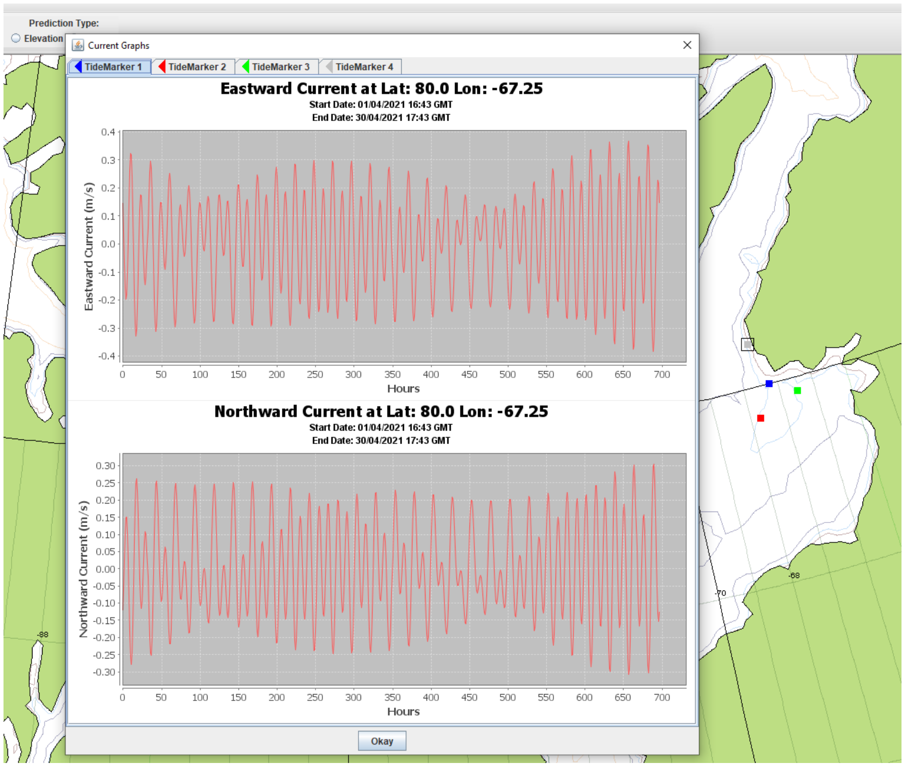
Lines 543-544, “Münchow (2011) reported a very similar warming in the southward branch of mAW of 0.23 °C/decade”: Actually Münchow et al. (2011). This paper provides very weak evidence of long-term warming because the period of observation was only 6 years. The present authors have taken the liberty of extrapolating this to 10 years, and then referring to a supposed “as further warming of mAW progresses” – all this without having made a bullet-proof case for an influence of mAW on the sea ice of Nares Strait. It is one thing to have mAW affect glacial ice at the same depth, quite another to postulate an influence on sea ice at the surface hundreds of meters above. I suggest to the authors that the present evidence to make this projection is not statistically robust.

In respect to “influence of mAW on the sea ice”, we agree that all our paper is built on the basis of indirect lines of evidence. Unfortunately, there is no possibility to provide a bullet-proof evidence without specialized direct measurements in the zones with large anomalies. However, it is the number of those lines that made us think that it is mAW that influences sea ice at both sides of Nares Strait, though through different mechanisms.

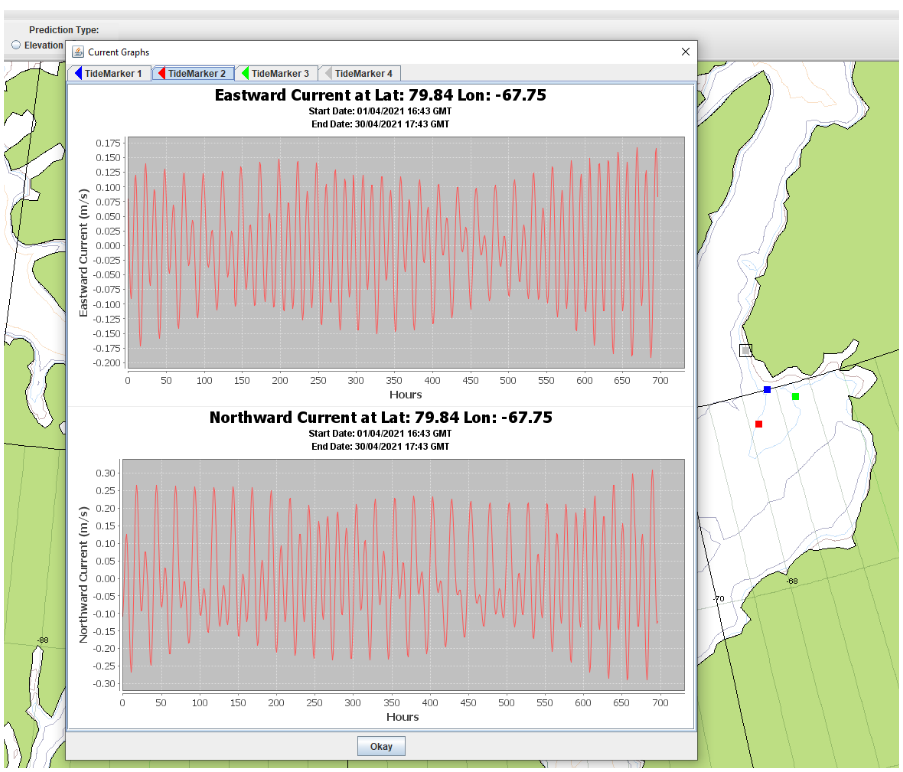
We admit our mistake with using results of Münchow et al. (2011) who showed the positive trend of the mean cross-channel temperatures of 0.027 °C/year in 2007-2009 mooring records only. We changed this part and have reduced the emphasis on the future projections. However, we can’t simply reject it because, if our suggestion about the impact of mAW on landfast ice is right, the warming of this water is important to mention.

SUPPLEMENTAL FIGURES

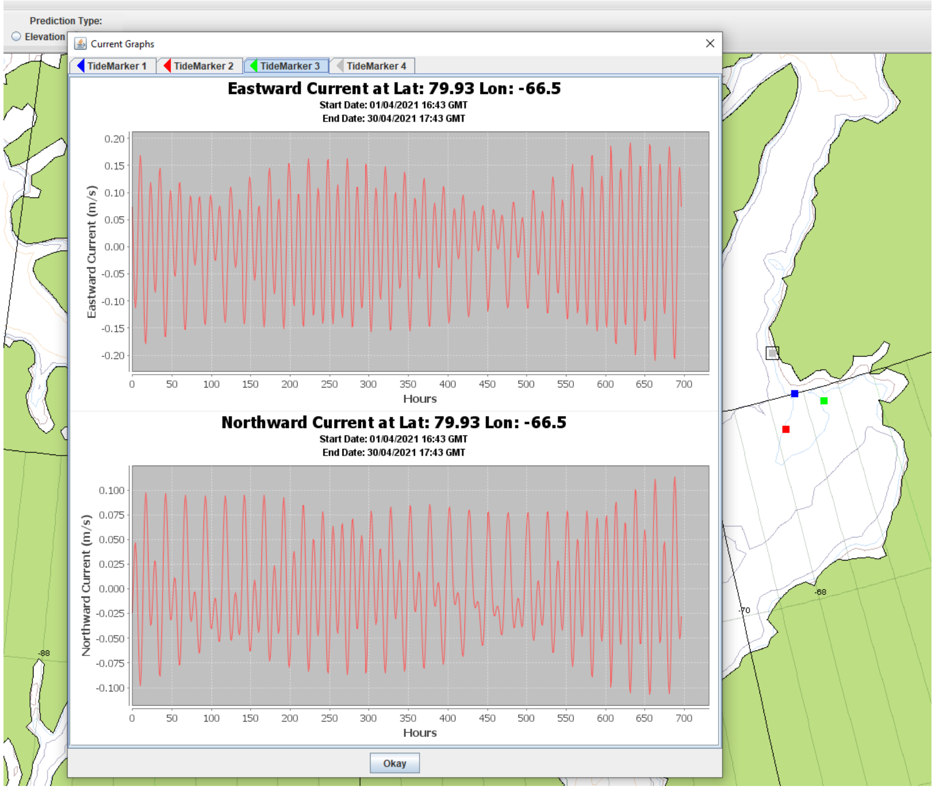
TideMarker 1



TideMarker 2



TideMarker3



TideMarker 4