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

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## 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/m$^2$ 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.

## 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".
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.
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.
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.

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.
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?

   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.
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/$m^2$ 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.

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.

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.

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.

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.

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.

## 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.

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.

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.

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.

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.

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.

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

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".

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".

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.

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

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

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.

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, air-pressure and ocean dynamical effects on the mesoscale (10-30 km)", referencing Samelson and Barbour (2010).

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.

Line 139-140, ">0.3 m mean snow depth in Kane Basin. However, as 140 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.

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?

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".

"… within the bridge": See comment re line 67.

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

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.

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.

Lines 231-242 & 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.

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?

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?

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?).

Line 235: Should “southwest” be 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.

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.

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.

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?

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.

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.

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.

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.

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.

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.