The role of oceanic heat flux in reducing thermodynamic ice growth in Nares Strait and promoting earlier collapse of the ice bridge, version 2

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Overview

I have restricted my present comments on the revision to the author's diligence in addressing my original comments. However, I admit that I did find the authors' responses to seemingly valid comments by the other reviewer unsatisfactory at times. For example:

> Other reviewer's comment: "There is currently no overlap between the ICESat-2 sea ice heights/AMSR temperatures (2019-2021) and the model results (2006-2010). It might be helpful to extend the AMSR temperatures back to 2006 to provide some comparison and context. It cannot necessarily be assumed that 2006-2010 have the same circulation conditions as 2019-2021".
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> Authors' responses:
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> 1. "We used the model of opportunity and were not able to choose a different simulated period."
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>   *There is nothing wrong with saying this, but the future readers of the paper need to know this unfortunate reality too; therefore the gist of this response should be added to the text.*
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> 2. "However, the main intention of the model was to demonstrate the circulation under the ice bridge. Since the main factors controlling water dynamics in this region (the along-strait sea-level gradient and the prevailing northern winds) don't vary a lot interannually, we reasonably suggest that the patterns shown in Fig.7-9 are generally valid and fairly represent (modeled) water dynamics and thermohaline state of Nares Strait in winter."
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>   *I very much doubt that "the along-strait sea-level gradient and the prevailing northern winds do not vary a lot interannually", whatever "a lot" means. However, since this purported lack of variation is crucial to the authors' line of reasoning, the paper must provide citations or include data and discussion to persuade the reader that this is so. Without documentation, the statement is unacceptable.*

I originally provided three overall comments, supported by more detailed comments cross-referenced to specific places in the first draft. The overall comments were:

- The authors neglected the established role of tidal currents in maintaining polynyas in the fast ice of the North American High Arctic.
- The accuracy, precision and possible bias of ice/snow-surface elevation and temperature measurements were not determined, because no in situ data were brought into play. Without error analysis of uncertainty, readers' confidence in the results obtained from satellite remote sensing is eroded.
- 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 that has been harnessed in an effort to fill this gap seems not to have been evaluated in this tiny – from a global perspective – area. Readers' skepticism about the study' results is therefore likely to be high.

Here I am taking the same approach in assessing the authors' responses. I provide an overall assessment of the revision in the next section. Subsequently, I provide my original comment (black font) followed by my assessment of the authors' response in blue font.

Assessment overall

Concerning "The authors neglected the established role of tidal currents in maintaining polynyas in the fast ice of the North American High Arctic".

The authors' begin a semi-quantitative (and incomplete) speculation to argue that tidal currents can't work to create a polynya at Cape Jackson, but later fall back on a more plausible hypothesis: "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".

Correct! This is the hypothesis developed in the citations to which I directed the authors in my first review: Polynyas beneath fast ice in the Arctic need two things: 1) "warm" (probably at least 0.05C" above freezing) water locally at depth, almost certainly of higher-than-surface density; 2) a strong local source of turbulence kinetic energy. In Nares Strait, baroclinic adjustment of the southward mean flow causes shoaling of warm water into shallower areas on the Greenland side, providing the shallow warm-water source beneath the polynyas. The strong local source of turbulence kinetic energy is almost certainly shear in the benthic and under-ice boundary layers of the strong tidal current. Tidal current, contrary to the authors' contention but well-known to mariners, is commonly accelerated around headlands like Cape Jackson; a narrow strait is not required. The generated turbulence erodes the stratification in density between the seabed and the surface and diffuses oceanic sensible heat, if present at depth, to the under-surface of the ice. An internal tide generated by the tidal flow across bottom topography, often serves to enhance to warm-water uplift in such areas. The strong tidal current additionally plays a role in sweeping newly formed frazil and nilas ice beneath or onto adjacent fast ice, except perhaps during neap tides and very cold conditions.

So, the authors are on the right track, but the suggested modest modification to line 536, namely "This heat may either be upwelled over the mid-basin ridge closer to the surface and/or transported upward to the lower surface of sea ice (or to the ice-free polynya) by vertical mixing" remains inadequate. The authors needs to lay out the requirements: warm water at depth, upwelling and a source of turbulence kinetic energy. Second, a paragraph is needed to properly enlighten the reader about the ocean and ice physics involved and third, the authors need to acknowledge earlier work and third,.

Concerning "The accuracy, precision and possible bias of ice/snow-surface elevation and temperature measurements were not determined."

See my critical responses below to comment 2, 3, and comments on lines 38, 46-47, 132 and 533-534. Work is still required

Concerning "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."

This comment remains difficult to redress, but it should be straightforward to acknowledge and document shortfalls of in situ data. I have continued in the detailed comment below to urge the authors to keep readers aware of all the ifs and buts associated with story that they are telling.

Overall the manuscript remains too speculative in its "explanations". It lacks brief digressions to present simple physic or statistics that could provide some reassurance of validity to speculative interpretations offered. In detailed comment below, I have continued to remind the authors of their responsibility in scientific publication to anticipate the queries of skeptical readers, and to provide enough scientific underpinning to bring this readers along with them set these readers at ease.

Major revision required: This revision is an improvement, but remains in need of further revision.

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".

   I accept the authors' sentence clarifying their use of the terms, and their choice to retain "ice bridge". I am disappointed they have chosen not to clean up the terminology. "Arch" and "bridge" sound like much the same thing to me, but are used for different purposes in the text.

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.

   I am pleased that the authors' have added a sentence discussing an intercomparison of ocean current modeled independently using wind from 55-km and 6-km atmospheric grids. However because the graphics they provided in their response will not appear in the paper, I take issue with their claim that "… the modelled current velocities in Kennedy Channel and in Smith Sound fairly coincide (not shown) with velocities obtained by Shroyer et al. (2015)". I acknowledge that the models produce flow cross-sections that are qualitatively similar. However, quantitatively they are appreciably different; Schroyer's sections show the speed of the undercurrent reaching about 45 and 65 cm/s in the two sections shown, whereas those in the FESOM sections reach only 25 and 40 cm/s, just 55-60% of those with high-resolution forcing.

   Because the models do not honestly "fairly coincide", I recommend that the quoted clause be revised to read "… the modelled cross-sections (not shown) of average November-June ocean current through Kennedy Channel and through Smith Sound driven by winds from FESOM-2 and from MM5 (9x higher resolution: Shroyer et al., 2015) are qualitatively similar but those derived using MM5 forcing yield currents that are 60-80% larger".

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 loses 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.

   I am relieved that the authors acknowledge the serious limitations of the ERA5 meteorological analyses in Nares Strait where variations in substrate (water, sea ice, land, ice cap) and elevation (sea level to 1000m) occur on a scale of a few km. I am pleased that they have added a paragraph that explains their goal in using the ERA5 forcing. They are also correct in stating that local-scale surface air temperature takes second seat to local snow depth in driving variations in ice growth rate. Unfortunately, as mentioned in my earlier review, the true sea surface off Cape Jackson, which would take the second seat, will likely lie hundreds of metres below where ERA5 "thinks" it is, because sea-level features as narrow as Nares Strait (40 km) probably do not exist within the ERA5 grid. Hundreds of metres in the polar atmospheric boundary layer span tens of degrees in temperature, which is non-trivial for ice growth.

Moreover, the authors state in the response (but not in the added paragraph) that “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”. This statement may be true on the 1000-km scale of synoptic weather forecasting, but it is not true at the surface within a few tens of km of a change in substrate, such as the edge of a polynya. The transition from fast ice with a surface temperature of perhaps -25C to open water or thin ice is likely at least 20C. Moreover, the atmospheric boundary layer, warmed and moistened as it moves over the polynya, is quickly covered by ice fog and further downwind stratus cloud (the cause of “water sky” over polynyas). Cloud droplets have high emissivity and their presence can reduce or perhaps reverse the upward heat loss by long-wave radiation that drives ice growth. Here huge differences occur on the mesoscale in this environment.

The authors have also responded with a couple of plots (to me, but not in the revised manuscript) showing probabilities of wintertime air temperatures and wind speeds measured with the Automatic Weather on Hans Island (red line) and from the nearest node of gridded ERA5 product during the same period “… to demonstrate that ERA5 parameters are realistic”. Unfortunately, for this comparison to have any value, we need to be absolutely sure that the Hans Island data were NOT USED in the ERA5. If they were, then the re-analysis result would be strongly biased to the data there, and quite possibly nowhere near as good elsewhere. Also, the conditions compared are for a rocky site high up (170 m) in the Polar Inversion. Its relevance to ERA5’s capability to predict conditions at marine locations at the bottom of this inversion is questionable.

This is all to say that there is clear potential for atmospheric forcing to vary appreciably on the scale of the polynya because the polynya is capable of creating its own weather.

This is also to say that it is not good science to make a statement such as the one that I quoted two paragraphs up without being very careful about the distinctions between physics of the atmosphere on a synoptic scale above the planetary boundary layers and that on the mesoscale within the planetary boundary layer. Sweeping generalization can lead one astray.

I don’t know what to recommend here to move your paper along, since once again you/we are stymied by the absence of information that you really need, specific to your locations of interest. Rather than torpedoing the paper, I prefer to recommend that you add straightforward discussion to acknowledge and describe the complexity of the analysis that you are undertaking, and the fact that many of your assumptions are ad hoc rather than readily justifiable. Such an approach will enlarge the document, and the result will not be definitive, but a full declaration will be of greater value to future scientific studies than a pretense that everything is just fine.

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 requires 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 authors appear to have misunderstood my response to the authors' original statement "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". However, this statement does clearly say, perhaps unintentionally, that the OCEAN HEAT FLUX must exceed 200 W m-2 to open the polynya as early as in March. They have come back in their response with an image of the polynya, showing thin ice and perhaps open areas and a statement that "If water surface is ice-free, 200 W/m2 does not seem to be extraordinarily high". I agree with this statement, with the reminder that if the surface is ice-free, that heat flux is derived from the latent heat of freezing for seawater and not from sensible heat in the ocean. The authors appear to have confounded these two fluxes.

Continuing their response, the authors write "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". The authors are correct in writing "none of these processes may prevent the polynya to eventually become covered with ice" but a cover of thin and young ice does not terminate the existence of a polynya. Polynyas are not necessarily open-water areas; they are areas with ice cover "appreciably thinner" than adjacent areas of ice on all sides. Many polynyas exist with a thin ice cover through the winter; at times when covered by a new fall of snow, they may be difficult to distinguish from surrounding ice, but they are still there. They only become ice free in the darkness of winter via the action of wind and current in fracturing the thin ice cover and carrying it away – a so called "latent heat polynya". As winter wanes, sometimes in April but more reliably in May and June, insolation will strong enough, at hundreds of W/m^2, to melt the ice.

The authors last point is "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)". The advection of frazil is well known in both oceans and rivers; under level ice, the crystals remain suspended in the turbulent surface waters and travel far, spreading their influence thinly over an area much larger than the polynya. Remember that tidal currents rotate and typically flow in every direction in the course of 12 hours. Under sea ice, frazil may settle against ridge keels, or within their porous structure, with a consequent increase in keel consolidation that is undetectable from space. Flooding, downfolding and submergence of sheets of new and young ice on the downwind, down-wave or down-current sides of polynyas in fast ice is well documented. It the process responsible Joseph-René Bellot's drowning in 1853 and has provided a close call for many others. Sheets of young ice have low buoyancy and can travel large distances in turbulent flows before the water between them and the overlying ice sheets is squeezed out.

The authors' arbitrary dismissal of earlier research on and knowledge of High Arctic polynyas in fast ice is not acceptable. Their continued insistence that the ocean has the capability of providing an unprecedented 200 W/m2 flux of SENSIBLE heat by some mysterious physical mechanism to an open sea surface continuously throughout the winter is highly unlikely. If they wish to stick with this concept, a great deal of careful work and explanation will be required to make a convincing case, if such can actually be done..

The revised text in lines 358 and 497 is therefore unacceptable. That in line 230, concerning the appearance of open water in March off Cape Jackson is okay if the phrase "as the daily downwelling shortwave flux increases with the coming of spring" is appended.

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.

Thank you.

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.

   The authors responses on these points and the proposed revised text are appropriate. Thank you.

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.

   The authors response to my suggestion is gratifying. Thank you.

   As I have mentioned, the baroclinic adjustment associated with the undercurrent core centred near 100 m only moves water above this depth to the surface; baroclinic adjustment below this depth drives water downward. Münchow et al. (2007) might be a better citation for the fractional presence of Pacific Water at different depths on sections along Nares Strait than Jones (2003) [*Münchow, A., Falkner, K. K., & Melling, H. (2007). Spatial continuity of measured seawater and tracer fluxes through Nares Strait, a dynamically wide channel bordering the Canadian Archipelago. Journal of Marine Research, 65(6), 759-788. figs. 6, 88, 11*]. You can see in their figures how the 50% Pacific Water contour corresponds roughly to the core of the undercurrent, with the remainder being predominately what you call mAW below this depth and meteoric water above it. But you also see that the mAW designation is a bit clumsy, since to be consistent you should be calling the PW, mPW – no purity so far from the sources. You might be better off abandoning use of mAW (I'm am not sure if this is an accepted designation anyway) and talking about the water above the core being predominately PW and that below as being predominately AW. Incidentally the upper PW, from the Alaskan Coastal Current in summer, near 80m depth in western Nares Strait, is plenty warm enough to melt ice. It doesn't need help from the Atlantic and is much better positioned in the water column to do so.

Adjustments to the text based on my assessment are recommended.

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.

   The modifications in line 607 have created a paragraph that is a little unclear, since it deals with two features in the ice cover that appear traceable to different physical processes: 1) the continuous band of thinner ice within 10 km of shore, most plausibly attributable to warm water upwelling above the upwelling; 2) The early appearance of water near headlands, most plausibly attributable to upward diffusion of sensible heat driven by turbulent instability of tidal current accelerated around headlines. I suggest that you could be more easily understood by having a separate paragraph for each issue.

   Query by the authors: Not for the paper, but we want to point at one 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.

   Perhaps you are referring to Davis et al. (2018) fig. 4? This actually depicts the amplitude of the semi-diurnally varying flux of tidal energy, not the speed of tidal current. The flux is $\underline{P} = \rho \cdot g \cdot h \cdot \langle \underline{u} \cdot \eta \rangle$, where $\underline{u}$ is the depth-averaged tidal current and η is the tidal elevation; $\underline{P}$ and $\underline{u}$ are vectors. Both these properties vary semi-diurnally and can have different phase. Fig. 4 illustrates that differences in phase result in a northward flux of tidal energy on the Greenland side of Kane Basin and a southward flux on the Ellesmere side. Note also that the tidal energy flux increases with water depth if *u* remains constant. I think that the strength of tidal currents off headlands – a local effect – depends very much on the character of the obstruction that the headland and adjacent submerged terrain pose to tidal flow. It is also possible that at Cape Frazer – a steep high cliff – drainage of snow melt-water from the heights onto the ice may help to expand the polynya in June.

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.

The authors appear to agree with my comment that cohesion of ice at the coast is not important to the longevity of the ice bridge. However, they continue to use the word 'cohesion' in the revised text at line 628, which I think is misleading. I suggest that they substitute something along the lines of "… we suggest that thinner ice and open leads at the shoreline of Kane Basin, linked to locally enhanced oceanic heat flux, provides weakness that allows patches of fast ice further from the coast to fracture and move in response to wind and current".

They subsequently speculate that collisions of these freed patches with remaining parts of the bridge "may" contribute to an earlier collapse of the bridge. This speculation is okay as an hypothesis, but the comment is of little value to the reader if it is just left hanging there. The reader has no idea whether or not this suggestion is worthy. How about adding a little "back of an envelope" physics to see whether the speculation has merit?

I decided to calculate the shock-loading of the fast-ice arch by the impact of a free-drifting floe for comparison with the sustained loading of an equal segment of the arch by the accumulated drag forces of wind and current on upstream fast ice. I assumed a 1-km width of contact by a square floe, 2.5 m thick, moving at 0.3 m/s and brought to a stop after ridging 30 m of ice along the zone of contact; with assumed constant deceleration, the force of impact was 3.45 million newtons, sustained over 200s. For the comparison, I assumed that wind and current stresses were accumulated over a fast-ice strip of the same width stretching 100 km up-channel from the arch, subject to the same 0.3 m/s current and to 15 m/s wind; the calculated loading was 37.5 million newtons on just this 1 km segment of the arch, sustained for days to months. In engineering terms, the arch has a safety factor of 10.9 for resilience against impacts of the specified type. The colliding floe would have to stretch more than 10.9 km up-drift for the safety factor to drop below 1, but this vulnerability would last for only a brief 200-s interval.

In my mind, this calculation suggests that the authors' idea that collisions with freed patches may contribute to an earlier collapse of the bridge is a non-starter … but perhaps the authors can devise a different scenario and simple physical model wherein the odds are not heavily stacked against their idea. If they cannot, I suggest that it be dropped from the text.

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.

    The authors have voiced agreement with my comment. They have changed the sentence to read "This break-up appeared to initiate the further fracturing of ice cover in the main channel". Since break-up and fracturing of the ice cover are essentially the same thing, this sentence seems circular and of little import. I may have missed the point, but either way, the sentence needs to be re-worked for improved clarity.

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.

    The revised manuscript has new text in 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."

Hypotheses are typically the second stage of the scientific method, following collection and examination of data relevant to the phenomenon to be studied. They remain as hypotheses until a mechanism of interaction has been identified and the direction of causation established.

Since both these subsequent steps remain undone, the authors' science is incomplete, meaning in strict terms that this paper is premature. However, if what appears in the conclusions is a limited list of plausible hypotheses to guide future research, with mechanisms tentatively identified) and with the list clearly flagged as such conjecture, with nothing really "proven", this could be sufficient for acceptance of this paper.

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.

The authors response to this comment appears entangled with the definitions of the water masses themselves; see my concluding comments under item (7) above. All water masses are only pure at their sources, so every water mass is a modified something. Moreover, the relationship is reciprocal, if PW mixes with AW, is the result mAW or mPW? The distinction between AW and mAW seems irrelevant. Admittedly, some sensible heat from the AW has reached the overlain Pacific Winter Water by the time the mixture enters Nares Strait, but less dense varieties of PW higher in the water column, namely Bering Sea Summer Water and Alaskan Coastal Current (summer) Water are less likely to have been influenced from below and more likely to have been influenced from above, by summertime Meteoric and ice-melt waters and wintertime freezing-associated brines. As stated under (7), I consider it less confusing to label a water mass PW if it is more than 50% virgin Pacific, and similarly AW if more than 50% virgin Atlantic. Then the hair-splitting about which water mass is most influential can be abandoned.

Definitions aside, I still maintain that if AW is to be lifted somehow to the base of the ice, the overlying PW will by necessity have to have gotten there first, and with a smaller expenditure of energy. So I judge the hypothesis that sensible heat reaches the ice from PW and perhaps also from AW to be more plausible than the converse, namely that sensible heat reaches the ice from AW and perhaps also from PW. Only if the authors could come up with an energetically viable mechanism to explain the latter version would I be more believing of it.

I recommend that the revised text provided by the authors and quoted under this item be re-worked either in terms of a simpler stack of water masses (i.e., ASW, PW, AW) which perforce are blended across their interfaces, or instead referenced to a partition of source waters in terms of T & S thresholds. Either approach would make this discussion much less enigmatic.

With reference to "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 …" in the revised version of the text, I caution the authors to look carefully at the Rabe papers which show appreciable differences between T & S profiles in winter and those in summer, which are quoted from the Jones papers. Since the present paper concerns winter, the former reference may be a more relevant source.

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.

Good

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.

Good

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.

In their response, the authors do not draw a distinction between random errors, which do shrink as the number of values averaged increases, and bias errors that do not. Since all values in a 1.5x1.5 km square share the same bias, traceable to errors in estimating propagation delays from space, in the geoid and in the (unseen under fast ice) sea level, the average is biased by the same amount, whatever it is. The authors still need to provide information on the magnitude and character of bias in CryoSat as part of the necessary error analysis.

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.

I took pains in my first review to demonstrate that the small number of bridgeless events is insufficient to draw with high confidence a distinction in their occurrence between the first two and the last two decades.

I stress that it remains important to provide limits of confidence on 2 as the mean for the first two decades and on 6 as the mean for the last two, and to calculate the confidence with which the data allows these two numbers to be considered different. Without error analysis, this is just handwaving.

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.

Okay.

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.

Okay, except that Hannah's paper actually did not consider heat sources. It simply correlated existence of polynyas in fast ice with h/u^3. Areas with small values of this parameter typically have strong current, maintaining the likelihood that both latent and sensible heat processes are in play. The Topham reference documented just this situation at the Dundas Island polymya. Please adjust your text accordingly.

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

Good

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".

Okay

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".

Good

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.

Okay

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

Good

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

Okay

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.

Notwithstanding the authors' hopeful "don't worry" comment in their response, this issue remains unresolved in my mind without reassuring observations. End-to-end SLP difference of 25 mb are quite common in Nares Strait (Samelson papers); who knows how much of the 25 cm of sea-level adjustment is actually distributed along the strait?

I therefore urge the authors to at least acknowledge the possibility that bias from inaccurate inverse-barometer correction could be a source of error, unless they can find data do better.

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).

Okay

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.

Okay

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.

The authors must not have looked carefully at the suggested reference before making their comment: "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". In fact, as clear in Samelson's paper, fig. 2, 6 and particularly 9, the areas of strongest gap-flow winds are not in Kennedy Channel but in north-central Kane Basin, off Cape Jackson and south of Smith Sound. Also, I suspect that there is a strong likelihood of katabatic winds off the Humboldt Glacier into Peabody Bay in winter, but the authors seem not to explored this possibility. A renewes look at these issues is highly recommended.

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?

Good

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.

Good

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".

Good

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.

Okay

Lines 218-238 & Fig. 4, "<u>along-track</u> 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.

Okay

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.

Perhaps the authors could include in the manuscript, for readers' benefit, a truncated version of the explanation provided in their response to me?

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?

Good

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?

Good

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?).

Good

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

Good

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.

Good

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.

Good

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.

See my comment under (4)

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?

Please excuse my misunderstanding. On re-reading, I don't know how I was misled, but good that you made an effort to keep others off the wrong path.

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.

Good

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.

Now irrelevant because of changes made in the revision.

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.

Good

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.

Okay

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.

Good

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.

I am okay with the first part of the authors' response, but not with the second, namely "We … 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".

The issue in contention is the difference between scientific publication and news publication. In the latter, the primary goal is to sell newspapers. In science we strive for robust results with carefully estimated bounds of uncertainty, numbers that we can provide with confidence to other scientists to help in their work. We can all speculate that something may or may not happen. However, the key factors are how confident we are in what we know has happened, how confident we are that it will continue to happen and what will be the value some years hence, plus or minus what. Since these key factors have not been addressed, the statement about warming, "if it is right" as the authors say, is of little value to science and can be omitted. If the authors can address these key factors, then the statement certainly belongs in the paper.