In the following, the comments by the reviewer are in green (old comments) and black (new comments) characters and our responses to the comments are made in blue and indented. The comments where the reviewer was satisfied with our initial answers and/or changes were left out of this document for simplicity. Modifications to the text are shown in quotation marks with bold characters indicating newly added text, and normal characters indicating text that was already present in the previous version.

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

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

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".
>
> Authors' responses:
> 1. "We used the model of opportunity and were not able to choose a different simulated period."
>
> *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.*
>
> > *Done*
>
> 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."
>
> *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.*
>
> > *To address this concern, we analysed the sea level data in Alert (data taken from the Canadian Tides and Water Levels Data Archive) and Thule (University of Hawaii Sea Level Center) and found no considerable difference between these two periods (Dec-Jun 2006-2010 and Dec-Jun 2019-2021).*
> > *The corresponding information was added in Section 2.3:*

"Although there is no overlap between the ICESat-2/AMSR data (2019-2021) and the available output of the FESOM2 model (2006-2010), we assume that there is very little interannual variability in the circulation under the ice-bridge given that the two major factors controlling water dynamics in this region (the along-strait sea-level gradient and wind) do not vary much interannually. The mean difference in sea level between Alert (data from the Canadian Tides and Water Levels Data Archive) and Thule (University of Hawaii Sea Level Center) from December to June was 1.56±0.04 m in 2006-2010 and 1.59±0.05 m in 2019-2021. The mean difference of sea level pressure between Alert and Thule, that may be used as a fair proxy of wind speed in Nares Strait (Samelson and Barbour, 2008), also revealed no significant changes from 2006-2010 (3.26±0.22 hPa) to 2019-2021 (3.33±0.23 hPa)."

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.

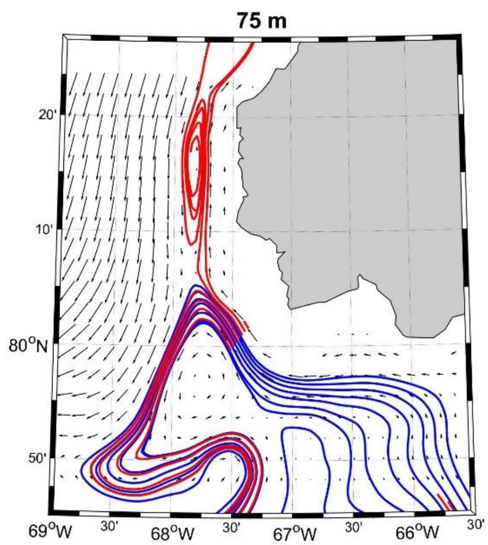
As the reviewer mentioned, we already admitted the role of vertical tidal mixing in the formation of the polynya at Cape Jackson and the corresponding changes were made in the paper. Therefore, one of major disagreements now seem to be about the "established role of tidal currents in maintaining polynya" and "sweeping newly formed frazil and nilas ice" that are suggested by the reviewer to play a significant role in the maintenance of this polynya. Unfortunately, without in-situ measurements, we can only speculate about their roles and about a role of advection and upwelling as suggested in our paper. But the reviewer is definitely right in one thing – we have to at least acknowledge all mechanisms that could result in formation of the Cape Jackson polynya.

In the following, we will try to convince the reviewer (and readers) why we suggest that the mechanical removal of new ice and/or frazil ice and nilas removal play only a secondary role in a formation of this polynya and why a tidally-driven upwelling and mixing is not thought to be the only mechanism that is responsible for vertical heat exchange in this area. Note, that the suggested text changes are presented further in our responses to the particular reviewer's comments.

1) We don't argue with the reviewer that tidal currents are "commonly accelerated around headlands like Cape Jackson". However, this is a general statement and we can't use it as granted without more robust data on the tidal flow structure there. Although the reviewer also mentioned that "a narrow strait is not required", all sensible heat polynyas surrounded by landfast ice are located in the narrows according to Hannah et al. (2009) and other papers. If there are any examples of such polynyas not constrained between landmasses, we are not aware of them. According to WebTide model, the spring tide current velocities in the vicinity of Cape Jackson is about 48 cm/s. We understand, that the model may not distinguish some local effects from prominent coastline features like Cape Jackson, but this is the only number we can operate with in this area. Interestingly, this speed is very close to what Munchow and Melling (2008) reported for KS10 position in Kennedy Channel (the combined effect of M_2, K_1 and S_2 constituents is 44 cm/s, whereas M_2 and K_1 constituents at KS14 were even larger).
   The 48 cm/s is much smaller than the peak 1.2 m/s measured in the polynya at Dundas Island during spring tide and for which Topham et al. (1983) described a mechanism of the mechanical removal of new ice. Within other sensible heat polynyas in the Canadian Arctic, the tidal currents

may be even faster (for example, in Cardigan Strait and Hell Gate) that makes all those areas not very comparable with the not-extremely-prominent Cape Jackson.

2) The horizontal excursion of a tidal flow with the peak velocity of 48 cm/s is 6.8 km. Using sparse bathymetric data, we could estimate the vertical displacement near the bottom corresponding to such excursion as ~30 m - from about 110 m to 80 m. We agree that such displacement is enough to uplift warm upper halocline water to the base of the water column in the polynya area (where it can be further diffused to the surface by the enhanced turbulent mixing). Although this argument may support the idea of tidally-driven vertical upwelling of heat, we don't agree with the reviewer that upwelling is a "mysterious" process. The wind-driven mechanism is not the only possible way to uplift water. A horizontal convergence of mean currents near a wall may also result in a local upwelling. Unfortunately, the dearth of bathymetry data and some limitation of FESOM-2 model don't allow us to estimate its real occurrence in the northern Peabody Bay. However, the regional circulation within the bay generally supports an existence of a convergence zone in its northern part as streamlines congregate along the northern coast:



3) As a continuation of the previous point. The area of thinner ice in the northern Peabody Bay is associated not only with the polynya at Cape Jackson. From Fig.4b and 4c, one can see that the negative anomalies of ice elevations spread over about 50-km from Cape Jackson to Cape Webster and further east. We strongly doubt that this entire area is affected by tidally-driven upwelling. We could admit that local tidal upwellings take place at specific hot spots (like Cape Webster and Cape Jackson) with further advection of the upwelled heat over a relatively long distance along the coast. However, if such combined mechanism works here, why don't we see negative coastal anomalies beyond Cape Jackson – at the entrance to Kennedy Channel?

4) We may partly agree with the idea of mechanical removal of nilas and frazil ice from the polynya. However, in some years the polynya is almost invisible even at the end of winter season (e.g. in 2008, 2011, 2018 in Fig. 2). And the polynya is invisible in early March during most of the years, opposite to the polynya at Dundas Island where an ice-free area is present almost every March since 2000 according to Worldview data. The presence of a relatively thin but solid ice cover near Cape Jackson and also along the entire northern coast of Peabody Bay suggests that mechanical breaking and removal of thin ice or frazil ice can not be considered as factors limiting the ice

growth in these areas and that the sea ice would continue to grow (Topham et al., 1983) if there was no ocean sensible heat flux.

5) To demonstrate that the tidal forcing at Cape Jackson is not strong enough to play an essential role in a mechanical breaking of thin ice, we also analyzed the time series of composed daily images of the polynyas at Dundas Island and Cape Jackson in April-May 2020. We used ALL daily images where clouds allowed to see individual ice floes.

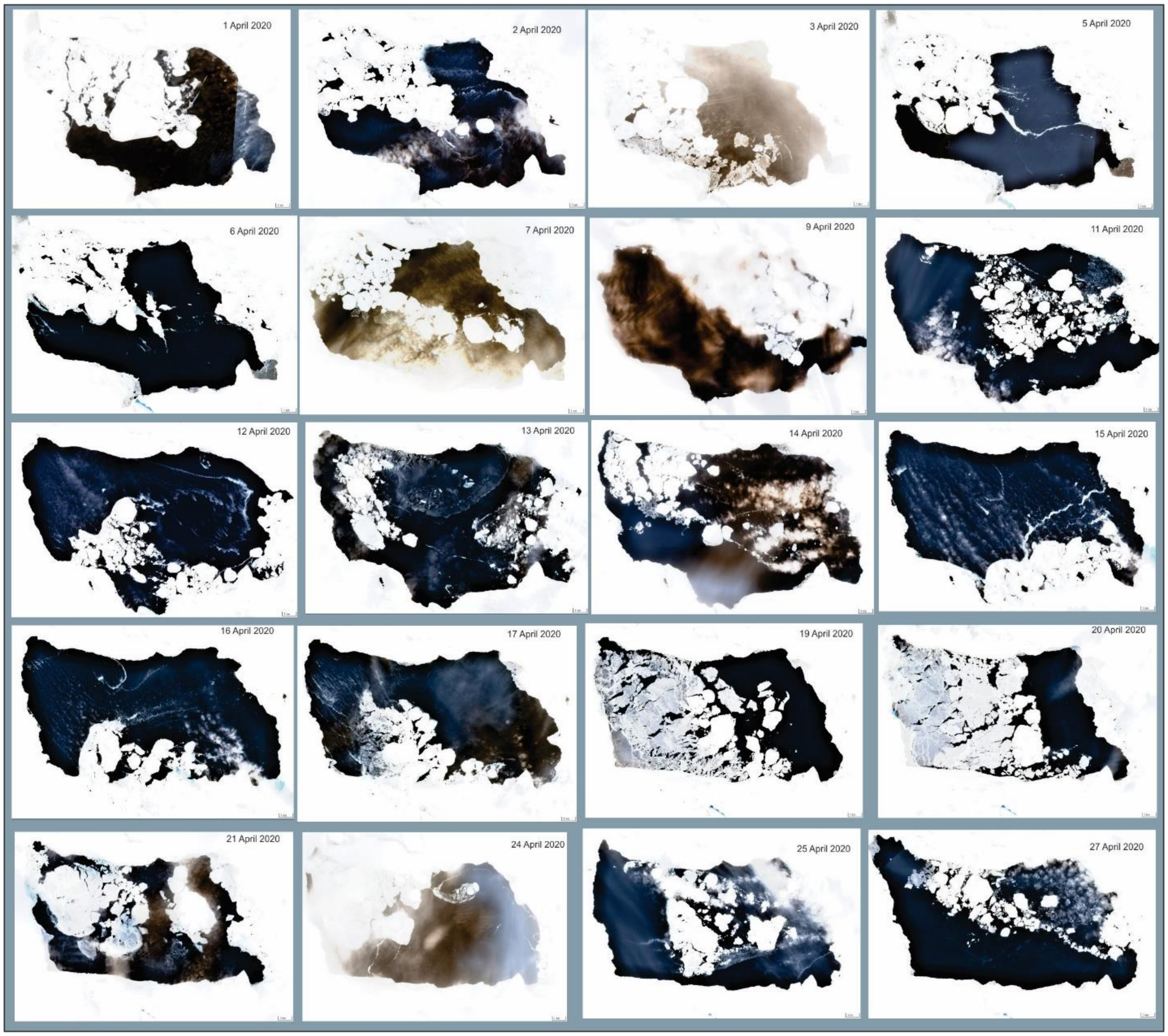


Figure. The polynya at Dundas Island

A combined effect of the strong tidal dynamics and wind forcing in the Dundas polynya result in clustering of the ice floes near polynya edges most of time (see the figure above) that is favorable for preconditioning the mechanical removal of ice. But the situation is different in the polynya at Cape Jackson (see the figure below).
Taking 3 km as a length of opening at Cape Jackson we can find that it would take less than 3 h for sea ice to move from edge to edge by tide with a maximum speed of 48 cm/s. Therefore, one may

expect that satellite images would have captured ice floes clustering near edges in more than 50% of all images. Or even more often, if the tidal flow at Cape Jackson is accelerated around the cape and exceeds 48 cm/s. However, we mainly see a quite random distribution of ice floes with numerous floes idling in the middle of the polynya almost in every satellite image. Note that we didn't select "good" daily images, but took ALL images when cloud conditions allowed to see the individual ice floes in both polynyas.

Based on this visualisation, we may suggest that the effect of tides in the Cape Jackson polynya is not very strong. Or at least considerably weaker than in the Dundas polynya where the process of mechanical removal of new ice was considered by Topham as a key factor responsible for keeping the Dundas polynya ice-free.

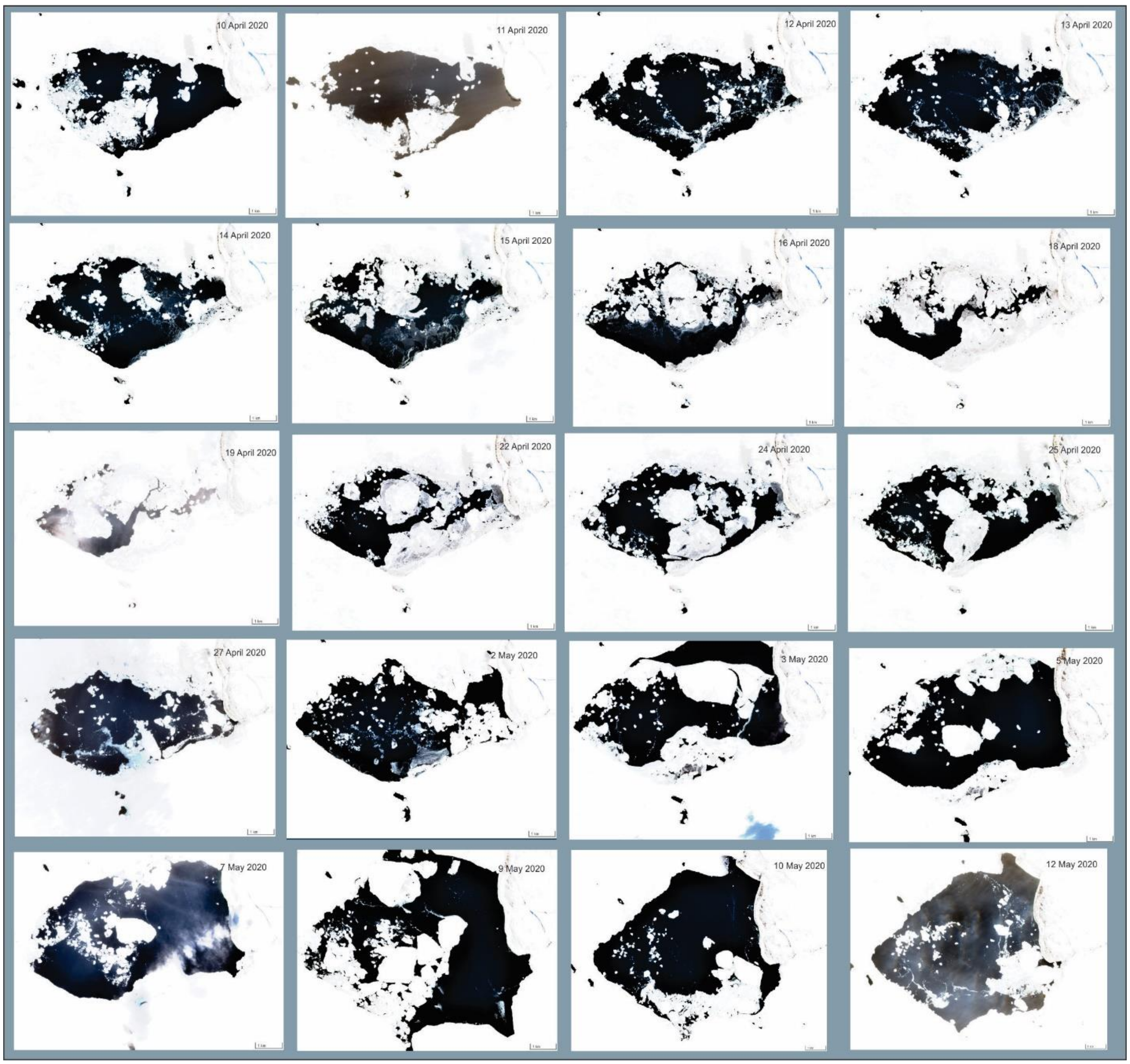


Figure. The polynya at Cape Jackson

Summarizing all these points:

We admit that tidally-driven upwelling and enhanced vertical mixing are likely responsible for maintaining the invisible or ice-free polynya at Cape Jackson. However, few moments allow us to suggest that upwelling associated with a convergence of mean currents in the northern Peabody Bay may also be present in this area. Regardless of this, we don't think that mechanical breaking and removal of ice plays a considerable role in formation of the polynya at Cape Jackson. The removal of frazil ice and nilas is not thought to contribute much either because most of the areas with negative anomalies are invisible polynyas, i.e. covered with thinner but solid ice. Please see the suggested changes addressing these and other reviewer's concerns further in this document.

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)

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 JRA55 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".

The top speed within the undercurrent jet at CATS line is 26 cm/s in the FESOM simulations and from 35 to 40 cm/s in Shroyer's paper (isotaches are separated by 0.05 m/s in their plots, not by 0.1 m/s!). In Smith Sound these numbers are 47 cm/s in FESOM and from 45 to 50 cm/s in Shroyer et al. It means that FESOM gives lower speeds in the Kennedy Channel (65-74%), but almost similar results in Smith Sound (94-104%), which is better than the reviewer initially suggested. We would also like to note that the observed difference in the Kennedy Channel may not be entirely attributed to a different resolution of atmospheric forcing. Using the different period of averaging (2006-2010 in FESOM and 2005 in Shroyer et al.), different parameterizations of ice cover etc. could also play a role.
But the main purpose of the model was to qualitatively show the circulation patterns in Nares Strait and particularly in Peabody Bay. The model speeds themselves were not used in our paper.

To address this concern, we changed the sentence as follows:

"*Despite using a low resolution JRA55 product that is probably unable to reproduce orographic strengthening of wind within the relatively narrow Nares Strait (Moore and Våge, 2018**; Moore, 2021**), the **structure of flow** in Kennedy Channel and in Smith Sound **obtained with FESOM2 qualitatively** coincides (not shown) with **that** obtained by Shroyer et al. (2015) who used the Polar MM5 regional atmospheric model, which has a finer horizontal resolution of 6-km. **Although the maximum speed of the southward jet in Kennedy Channel in FESOM2 simulations is about 30% lower compared to Shroyer's results, the speeds in Smith Sound are almost the same.***"

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.

We have to admit that we did not address this particular concern in our previous response. And first of all we need to apologize for not showing from the beginning the location of ERA5 data used in our paper. For our ice growth model, we used ERA5 data taken at 66.75W, 79.625N (the position was added to Fig.1) – at the center of Peabody Bay. The bay is part of the relatively large Kane Basin which is ~130 x ~200 km and able to accommodate a few zonal and meridional ERA5 nodes. We agree that such resolution is not good for resolving orographic effects for example in Kennedy Channel still, but it is thought to be good enough to suggest that ERA5 surface data represents the surface layer characteristics in Kane Basin reasonably well. For example, Fig.10 and 11 in Moore (2021, www.nature.com/articles/s41598-021-92813-9) support the similarity of 30 km ERA5 and 9 km ECOA surface wind data in Peabody Bay.

The following sentence was added in Section 2.4: "***We used data taken from the central part of Peabody Bay (66.75W, 79.625N, the yellow star in Fig.1) where orographic effects are pronounced than in the main channel of the strait (Moore, 2021).***"

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.

We do not agree with the reviewer here. First of all, our phrase was related to the landfast ice-covered part of Nares Strait only, not to any system with polynya(s). We are aware of all effects associated with open water in winter, but would like to underline that the size of polynya at Cape Jackson is very small and the polynya represents an isolated spot surrounded by hundreds of kilometers of ice. Therefore, its effect is incomparable with the large latent heat flaw polynyas like NOW that may affect a state of the boundary layer over a few hundred kilometers. In addition, the polynya at Cape Jackson is mainly invisible throughout most of winter: an opening barely reaches several hundred meters in early March and it starts to grow in April to reach only about 3-4 km at the end of the month when air temperature is getting higher and air-ocean temperature contrast decreases.

And, with all respect to the reviewer, we suggest that the key point in that phrase was not taken properly. We meant ALL anomalies of ice thicknesses including the coastal anomalies along the western part of the strait and in the northern Peabody Bay. Those anomalies are not associated with open water, just thinner ice that can't change meteorological parameters over these invisible polynyas much. An effect of open water on the boundary layer near Cape Jackson is doubtless. At least at the end of winter when the polynya turns ice-free. However, even here this effect is a consequence of the polynya, not a cause of its formation.

We added the following text in Section 4.1 to discuss a possible effect of an altered balance over the polynya at Cape Jackson on our ice growth calculations there (Fig. 6c):

"***It was found that keeping the polynya open in early March requires a relatively large (>200 W m-2) additional, presumably sensible, ocean heat flux to compensate the heat loss to the atmosphere. This amount was obtained from the model forced by atmospheric conditions in the central part of Peabody Bay. The polynya at Cape Jackson is usually ice-covered (invisible) during most of winter, starts opening in early March and reaches approximately 3-4 km in size by the end of April. Considering that the model covers the period from December to April, we may suggest that a shift in radiation balance caused by the presence of moister air over open water within the polynya mainly occurs at the end of winter and using ERA-5 data from the central Peabody Bay results in a relatively small overestimation of the sensible heat flux needed to keep this polynya nearly ice-free. From our point of view, the more considerable effect may be the mechanical removal of frazil and nilas ice at the end of winter when the presence of open water results in more intensive formation of ice crystals that may be carried away by advection (e.g. Smith et al., 1990). The full loss of heat to the atmosphere in this case is not entirely compensated by the sensible heat flux from below, but partly explained by the latent heat of ice formation. Although it is difficult to estimate an impact of frazil ice removal and shift of atmosphere boundary layer characteristics on the ice balance without specialized in-situ measurements, we suppose that the reported 200 W m-2 as well as 70 W m-2 heat fluxes may not be entirely associated with the sensible heat and, therefore, overestimated. Note also that another possible mechanism of ice loss through breakage and removal of relatively thick new ice floes associated with strong tidal currents and winds (Topham et al., 1983) is not thought to play a considerable role in the polynya at Cape Jackson. The analysis of daily Sentinel-2 images in April-May 2020 (accessible through Sentinel Playground hub operated by Sinergise Laboratory for geographical information systems, www.sinergise.com/en) revealed that the individual ice floes idle in the center of the polynya most of the time, instead of clustering near edges as they usually do in the Dundas polynya studied by Topham et al. (1983). It implies that tidal currents at Cape Jackson are not thought to be strong enough to initiate breakage and removal of new ice from the polynya here."***

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.

That example with Hans Island meteorological data was just an attempt to demonstrate the quality of ERA5 based on the only available measurements in the area of research. With a deficit of other available data, we could offer nothing else. We hope that our aforementioned answers to this comment could convince the reviewer that ERA5 data from the central Peabody Bay is the best data available to represent the state of the atmosphere in the surface layer.

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.

We added the new text to Section 4.1 that contains discussion of a possible effect of altered boundary layer characteristics <u>over the polynya</u> on the estimated ocean heat flux (admitted to be overestimated) – see our response above. In regarding to using ERA5 data in the model, we added a paragraph in Section 3.3 that describes the sensitivity of the ice growth rate to the uncertainties of the meteorological parameters (air temperature, wind speed, snowfall rate) and ocean forcing (sensible heat flux):

"***To investigate a sensitivity of model results to the uncertainties in forcing parameters and/or possible biases of ERA5 data in Peabody Bay, we conducted several model runs with biased air temperature, wind speed, snowfall rate and ocean heat flux for the experiment presented in Fig. 6b. It was found that increasing wind speeds by 1 m s-1 increased ice thickness at the end of the season by 4.02 cm, whereas increasing air temperature, snow accumulation rate and ocean heat flux by 1° C, 1 cm mo-1 and 1 W m-2 reduced ice thickness by 2.45, 7.41 and 3.36 cm, respectively. Using these numbers, we can estimate a relative input of each parameter to the final results. Using 1 m/s, 2 °C, 2 cm/mo and 5 W m-2 as the possible biases or uncertainties of the model forcing parameters and taking the impact of ocean heat flux as 100%, we may estimate the relative input of other factors as 88% (snow), 29% (air temp) and 24% (wind speed) that underlines a larger contributing effect of snow and sensible heat flux on the ice growth.***"

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.

Sorry for misunderstanding this comment first and for not addressing it. We added a paragraph in Section 4.1 that addresses this issue. Please see our response to the previous comment

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 polynya at Cape Jackson is ice-covered (invisible) during most part of the winter and usually starts opening only in March. This made us think that mechanical removal of frazil ice or sheets of thin ice does not play a significant role in this polynya. Please see our response to your Overall Assessment and the suggested changes in the Section 4.1 in the response to the previous comment.

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.

A possible impact of frazil ice was discussed in the new paragraph added to Section 4.1 (see our response to the previous comment)

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.

We don't think it's a correct statement. The consolidation of frazil ice within interstices of keels would increase a buoyancy of a ridge and result in higher surface elevations.

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.

Although we really did not consider such mechanism from the beginning, we have certain doubts that it is workable for the polynya at Cape Jackson. As we explained earlier, the polynya at Cape Jackson is mainly invisible during winter that considerably reduces a possible effect of thin ice removal as a source of extra (latent) heat to compensate the heat loss to the atmosphere. In the new text added to Section 4.1, we discuss a possible impact of both altered boundary layer characteristics over the polynya and frazil/new ice removal. We agreed that the estimated sensible heat fluxes of 200 W/m2 and 70 W/m2 seem to be overestimated. See the changes presented in our response to the comment #3.

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.

The sentence in Line 358 ("*Within the polynya, in order to have open water in May the heat flux would need to reach 70 W m-2, while a heat flux above 200 W m-2 is required to form an ice-free polynya in early March.*") just says how much additional heat is needed to keep the polynya open. Although we really suggested initially that this flux is attributed to the sensible heat, there is no mention of it in this paragraph. However, we changed one of the sentence above to avoid any confusion:

"*Adding in the ~~ocean~~ heat flux **from below** lowers this thickness and also shifts the timing of maximum ice thickness.*"

In the new paragraph, added after Line 497 in Section 4.1 (see our response to the previous comment), we discussed the role of other mechanisms (mechanical ice removal and shift of energy balance over polynya) that could result in our overestimating the ocean heat flux at Cape Jackson attributed to sensible heat. In particular, the followed sentence was included to that paragraph:

"***Although it is difficult to estimate an impact of frazil ice removal and shift of atmosphere boundary layer characteristics on the ice balance without specialized in-situ measurements, we suppose that the reported 200 W m-2 as well as 70 W m-2 heat fluxes may not be entirely associated with the sensible heat and, therefore, overestimated.***"

We agree that it is a change of surface net balance that turns an invisible polynya to an ice-free area. The requested phrase was appended.

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.

Addressing this concern, we substantially changed the text. Particularly, the following changes were made:

Line 18-20 in the Abstract:

"*Using the sea ice-ocean model FESOM2, we then attribute these ice thickness anomalies to* ***the heat from warmer subsurface waters of Pacific and Atlantic origin*** ~~*upwelling of warm modified water of Atlantic origin*~~ *that reduces thermodynamic ice growth throughout winter* ***on the western and eastern sides, respectively****.*"

Section 3.4:

"*This flow occupies the entire water column and consists of 3 distinctive layers: i) cold brackish mixed water within the upper 50-60 m, ii)* ***the relatively warm*** *upper halocline that is observed at 70-110 m and (iii) the warm underlying layer* ***that is mainly associated with*** *Atlantic Water (AW) which originated in the North Atlantic and was transported a long way from Fram Strait into the Arctic Ocean and to Northern Greenland (e.g. Melling et al., 2001). The first two layers* ***are*** *mainly* ***associated with*** *Pacific Water* ***(PW) which comprises more than 50% of the upper 100-150 m in Nares Strait (****Jones and Eert, 2006;* ***Münchow et al., 2007).***"

The end of Section 4.1 was considerably modified:

"***Even though the obtained ocean heat flux exceeding 200 W m-2 or even 70 W m-2 within the polynya at Cape Jackson seems to be overestimated, it is clear that ocean sensible heat plays a principal role in the maintenance of this polynya and in limiting the ice growth along the northern coast of Peabody Bay. In winter, this sensible heat flux is associated with the warm AW penetrating Kane Basin from northern Baffin Bay and spreading northwards along the eastern coast (Fig. 7 and 9). According to FESOM-2 simulations, water temperatures in Peabody Bay start to increase from about 60 m and reach near-bottom maximum of -0.15 °C or ~1.75 °C above freezing (Fig. 9d). Although there is no available data on the fractional composition of water masses in Peabody Bay, we suggest that the upper thermocline may contain a fraction of PW transported southward through Nares Strait. In the southern Kennedy Channel, the fraction of PW exceeds 50% above 110 m (Jones and Eert, 2006; Münchow et al., 2007), while further south, this water may be partly mixed at depth with the northward coastal flow and transported to northern Peabody Bay as part of the cyclonic gyre (Fig. 7a). Regardless of its origin, the presence of water with temperatures above the freezing point below 60 m is thought to be the source of the sensible heat that limits sea ice growth along the northern coast of the bay. This heat is brought to the surface by upwelling that is associated with either tidal flow or the mean circulation. Although the recurrent tidally driven upwelling and vigorous vertical mixing is a*** *well-known process leading to the formation of visible and invisible polynyas in the numerous narrows of the Canadian Arctic Archipelago (Topham et al., 1983; Smith et al., 1990; Hannah et al., 2009), the analysis of daily Sentinel-2 images supports the suggestion that tidal currents at Cape Jackson seem to be considerably lower than in other polynyas. Tidal currents may still contribute to uplifting the upper thermocline water closer to the surface and cause an enhanced vertical heat transfer towards the surface near prominent headlands such as Cape Jackson or Cape Webster, but the presence of the 50-km long coastal zone of thinner ice implies some contribution of upwelling associated with a horizontal convergence of the mean flow. Although the dearth of bathymetry data and limitations of FESOM-2 make a confident evaluation of this mechanism difficult, the regional circulation generally supports the existence of a convergence zone in the northern part of Peabody Bay where northward flow turns westward (Fig. 7a). An additional factor facilitating vertical heat exchange in the northwestern Bay may be associated with the presence of hundreds of icebergs (originating from Humboldt glacier) grounded at the eastern flank of the mid-basin ridge in Peabody Bay. In satellite images, these icebergs are seen to form well-separated parallel chains oriented from NE to SE (Fig. 1c) that may act as a steering grid for a thorough water flow. Although the mean water dynamics in the bay during winter is relatively weak compared to the main channel (Fig. 9b), the strong semidiurnal tidal current may provide a necessary kinetic energy for generating shear instabilities near hundreds of grounded icebergs."*

The new text added to Section 4.2:

"*Similar to Peabody Bay, the oceanic heat flux needed for limiting sea ice growth along the western side of Nares Strait is associated with warm water at depths below 70-80 m (Fig. 9d). The upper thermocline layer in the main channel is mainly comprised of relatively cold PW, whereas the considerably warmer AW prevails at depths below 150 m (Jones et al., 2003; Jones and Eert, 2006; Münchow et al., 2007). The baroclinic adjustment of the ocean to the intensification of the southward current in winter induces upwelling above the core of southward undercurrent that shifts the upper thermocline closer to the surface (Rabe et al., 2012; Shroyer et al., 2017). As a result, water above the freezing point can be found starting from 30-40 m depth near the Ellesmere coast (Fig. 9d) forming favourable conditions for a larger heat transport to the bottom of sea ice here.*"

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.

We changed this part to address all these concerns. And we accepted the reviewer's vision about the major mechanism responsible for the anomalies along the western coast:

"***Similar to Peabody Bay, the oceanic heat flux needed for limiting sea ice growth along the western side of Nares Strait is associated with warm water at depths below 70-80 m (Fig. 9d). The upper thermocline layer in the main channel is mainly comprised of relatively cold PW, whereas the considerably warmer AW prevails at depths below 150 m (Jones et al., 2003; Jones and Eert, 2006; Münchow et al., 2007). The baroclinic adjustment of the ocean to the intensification of the southward current in winter induces upwelling above the core of southward undercurrent that shifts the upper thermocline closer to the surface (Rabe et al., 2012; Shroyer et al., 2017). As a result, water above the freezing point can be found starting from 30-40 m depth near the Ellesmere coast (Fig. 9d) forming favourable conditions for a larger heat transport to the bottom of sea ice here.*** *Unfortunately, the lack of in-situ measurements does not allow us to quantify the vertical oceanic heat fluxes into the western Nares Strait polynyas, but it is likely that the barotropic semidiurnal tide, with magnitudes comparable to the speed of the mean southward flow (Münchow, 2016; Davis et al., 2019), greatly affects their intensity. Transformation of these currents over steep topography generates a baroclinic semidiurnal tidal wave that may considerably enhance vertical mixing* ***in the water column*** *through benthic stresses and shear instabilities (Davis et al., 2019). From this perspective, the fact that most of the western polynyas first appear near prominent headlands (Fig. 11) generally support the idea that the enhanced heat fluxes along the Ellesmere coast are attributed to the topographically controlled instabilities associated with the mean current and reversible tidal flow. Another mechanism that may enhance the heat flux in the area is associated with the sub-ice turbulence generated by interaction of very rough under-ice topography* ***in the channel*** *and tidal flow (Ryan and Münchow, 2017)* ***that is expected to be accelerated around headlands****. In combination with upwelling of the upper thermocline water along the western coast in winter (Shroyer et al., 2017), this mechanism may be* ***considered as a key factor*** *resulting in* ***an enhanced*** *vertical heat flux towards the bottom of sea ice* ***along the Ellesmere coast.***"

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 $P = \rho \cdot g \cdot h \cdot \langle u \cdot \eta \rangle$, where u is the depth-averaged tidal current and η is the tidal elevation; P and 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.

That query had originated from the analysis of high-resolution tidal simulation data in a few positions along the channel shared with us by Laurie Padman while we were working on our previous Nares Strait paper, not from Davis et al. (2018) paper. That hi-res dataset showed the decrease of the tidal velocities towards the (approximate) center of the Kane Basin.

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.

With all respect to the reviewer and understanding all difficulties of testing our hypothesis, we don't want to drop it from the text. The numbers used by the reviewer are somehow arbitrary chosen from our point of view, but even if they are right, the impact of freed ice floes does not replace but adds stress to the arch. Taking into account that the sea ice in the strait is already deteriorated by the middle of summer and therefore weaker, the arch might not be able to bear this top-up stress.

However, we have to admit our other mistake. We forgot to mention another factor associated with the mobile ice areas that seems to play the key role in facilitating the ice bridge collapse – an inward destruction of the bridge. This process is perfectly seen in the MODIS images between 23-26 June, 2020. The partial collapse and closing the polynya at Cape Jackson on June 23 led to a situation when more and more fractures appeared in the surrounding ice and the ice bridge breaks "inward". Such collapse can be observed in our Fig. 12 and 13, but is seen more clearly when switching between the sequential MODIS (for example in the WorldView) or Sentinel-2 images.

One more argument will be referencing Plante et al. (2020) who investigated the behaviour and collapse of the ice bridge in Nares Strait with an idealized ice bridge model. They described a collapse of the bridge initiated by upstream fractures: "The propagation of damage from these locations is composed of two separate fractures. First, a shear fracture progresses downstream along the channel walls, resulting in the decohesion of the landfast ice in the channel from the channel walls. The decohesion of the ice bridge increases the load on the downstream ice arch and on the landfast ice upstream of the channel…".

Addressing this concern, we changed Fig.13 (to make a better focus on fracturing by replacing MODIS images with Sentinel-2 data) and modified this part of the text as follows:

"*We can further speculate that such weakening may facilitate an earlier ice bridge break-up (comparing to a supposed no-polynyas situation) as it leads to formation of patches of mobile ice in the middle of the ice bridge in Kane Basin. While shifting around, this ice may gain some kinetic energy from wind and tide and eventually result in additional dynamical load on the parts of the bridge that still remain in place* ***but are thermally deteriorated by this time of year. More important, however, is that the edge of these patches does not provide a necessary structural support (as the arch in Smith Sound does) and that mechanical stresses cause an inward collapse of the ice bridge. This process can be seen as a propagation of new fractures outwards from the polynya area. Similar fracturing, which results in a decohesion of the landfast ice in the channel from the coast, was reported for simulations of the ice bridge collapse in Plante et al. (2020). For example, in 2020,*** *the polynya was generally well-constrained during March-May* ***(Fig. 12)****. The polynya started to expand in late May, reached its maximum size in mid-June, and* ***the surrounding ice cover*** *broke up around 22 June (Fig. 12). This* ***partial*** *break-up* ***of the landfast ice***

***surrounding the polynya** appeared to initiate further fracturing of the ice cover in the main channel **(Fig. 13)**."*

And the beginning of the next paragraph:

***"Although the satellite imagery presented Fig. 13 and results of the numerical modelling (Plante et al., 2020) generally support** our **suggestion** ~~hypothesis~~ that visible and invisible (i.e., thin ice areas) polynyas **may** facilitate ice bridge break-up in Nares Strait**, this suggestion remains speculative without more detailed research. However,** 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."*

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.

Under a breaking of the polynya we meant a partial collapse of the surrounding ice and filling the polynya area with mobile ice. We see the reviewer's point, but we could not find a better wording than:

"*This **partial** break-up **of the landfast ice surrounding the polynya** appeared to initiate further fracturing of the ice cover in the main channel on 23-26 June **(Fig. 13)***."

Also see our more detailed response to the previous comment.

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.

We hope that the changes suggested in the text and a new version of Fig. 13 were found convincing to address this concern (see our response to the comment #9). Particularly, the problematic sentence was rewritten as follows:

"Although the satellite imagery presented Fig. 13 and results of the numerical modelling (Plante et al., 2020) generally support** our **suggestion** ~~hypothesis~~ that visible and invisible (i.e., thin ice areas) polynyas **may** facilitate ice bridge break-up in Nares Strait**, this suggestion remains speculative without more detailed research. However,** 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.**"

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.

We did the best to solve this problem in the text and focus on consequences of the presence of warm subsurface water in the region rather than on its origin. The suggested changes in the text can be found in our response to the comment #7 above.

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.

We agreed on this and placed PW on the first place wherever we discussed the impact of the uptake of heat from below in the main channel. However, we suggest that the warm subsurface water in Peabody Bay mainly consists of AW originated from Baffin Bay. But a possible recirculation of PW in Kane Basin has also been considered and mentioned in the text.

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.

We got rid of mAW term and changed the text accordingly in terms of water mass terminology.

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.

We did not understand completely what differences the reviewer meant particularly. But we would like to say that we used FESOM-2 data obtained during winter period (Fig. 9b and Fig. 9d) while describing a possible impact of warm subsurface water on ocean heat flux in the main channel and in Peabody Bay. Probably, this comment is not relevant anymore since the text was modified.

Comments (minor)

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.

We understand the difference between random errors and possible biases. However, the changes suggested in our first response were related to the accuracy of the ICESat-2 readings and calculated anomalies, i.e. random errors. The possible biases related to other factors were discussed further in the text and also in Section 3.2. And because we are working with regional anomalies, we limited our discussion to the biases that vary regionally (sea level, steric heights). The biases that seem to not vary much over Kane Basin (like propagation delays from space or the geoid) are not thought to affect our findings and the observed strong local elevation gradients.

Otherwise, we don't understand what kind of additional information on the magnitude and character of bias in ICESat-2 data is requested by the reviewer.

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.

We apologise for not including the results of statistical analysis made by the reviewer, but we don't really think it is relevant to the Introduction. We just share general information and known facts about the bridge in Nares Strait. In the concerned sentence, we presented **the fact** that "the ice bridge failed to form only two times during the first two decades of observational records (in 1993 and 1995; Vincent, 2019) and six times during last two decades (in 2007, 2009, 2010, 2017, 2019 and 2022)". If the main concern is about attributing this information to "changes in environmental conditions", we can partly agree with the reviewer. Neither the fact that 6 out of 8 bridgeless years happened during last two decades nor a general shortening of bridge existence period reported by Vincent (2019) have very high statistical significance.

We slightly changed this sentence as follows:

"*Based on AVHRR satellite data from 1979 to 2019, Vincent (2019) reported on a recent trend**, though not confident,** towards later formation and earlier breakup of the ice bridge. **Additionally, Vincent (2019) found** that the ice bridge failed to form only two times during the first two decades of observational records (in 1993 and 1995; Vincent, 2019) and six times during last two decades (in 2007, 2009, 2010, 2017, 2019 and 2022), underscoring a general shortening **in the duration** of the bridge and pointing to changes in the environmental conditions.*"

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 polynya. Please adjust your text accordingly.

The criticism is reasonable. The sentences were changed as follows:

"*Beyond the NOW and other latent heat polynyas, there are several polynyas that form within the landfast ice cover of the Canadian Arctic* ***and are at least partly attributed to the sensible heat flux from the ocean (Topham et al., 1983; Smith et al., 1990)****. Most of these polynyas are formed in the narrows where strong tidal and mean currents* ***cause vigorous vertical mixing and*** *facilitate upward heat transfer from* ***the warm subsurface water at depth*** *(****Topham et al., 1983****).*"

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.

We don't really like such interpretation of our initial comment. In our response we wrote that "the width of the strait is too small for spatial (cross-channel) SLP variations having a large effect on the CROSS-CHANNEL sea level difference". Regarding the along-channel variations of the SLP we can roughly estimate the mean gradient of the sea level that corresponds to the mentioned difference: 0.45 cm per 10 km. The ICESat-2 along-track elevation changes (within coastal anomaly areas) exceed this gradient more than by an order of magnitude. In addition, our Fig. 4 represents the time-averaged anomalies, whereas 25 mb difference is considerably larger than a typical 4-5 mb along-channel pressure difference (e.g. Samelson and Barber, 2008).

In our initial response we also presented another argument about a long-wave speed in Nares Strait. We found it large enough to adjust the sea level in the strait to moving SLP anomalies reasonably fast.

All aforesaid allows us to reasonably suggest that SLP in general and the accuracy of the inverse-barometer correction in particular have negligible impact on the observed elevation anomalies discussed in our paper. With a respect to the reviewer's opinion, we don't see any necessity of the additional discussion around inverse-barometer correction in the context of our study.

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.

We can give a contra argument by referencing Moore (2021) and his Fig. 10c published in the Scientific Reports. From that figure it's clearly seen that Cape Jackson is located slightly aside of the area where strong orographic or gap-flow winds are present. In addition, we would like to emphasize again that we used no-snow assumption in simulating the ice growth within polynya area at Cape Jackson. The different snow accumulation rates were used for simulating of ice growth in an abstract position located few dozen kilometers south-east of the polynya within landfast ice – much farther from the channel and from the gap-flow region. Also see our response to the comment #3.

However, we don't see a need of referencing Samelson and Barbour (2008) because the gap-flow effect is attributed to the exit from the Kennedy Channel. The mean snow depth of >0.3 m (Landy et al., 2017) characterises snow cover approximately in the centre of Kane Basin. We have specified in the text that Landy's data is related to the **central** Kane Basin.

In terms of a katabatic wind we can't do much. The only relevant publication we could find was a field report of Günther Heinemann who presented the results of a POLAR-5 flight over Humboldt glacier and Peabody Bay in June 2010 ("Investigation of Katabatic winds and Polynyas during Summer" in Reports on Polar and Marine Research, 633). They reported a weak katabatic flow over the glacier and even more weaker wind over landfast ice, but their results were obtained during summer. Without more robust information about this flow, we don't want to speculate about it in the paper.

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?

Done as requested. The followed sentence was added to Section 2.2

"*…and then averaged onto a 1.5x1.5 km grid.* ***This resolution was chosen to ensure that neighboring strong beams, which are separated by ~3 km, were not projected into the same grid cell. Although such resolution increases the noise in the spatial distribution of $\tilde{h}$ (especially in the areas with mobile ice), it highlights the areas with strong elevation gradients that are spatially consistent throughout winter***."

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)

A few changes were made in the text to address this reviewer's concern. Please see our responses to the comments #3 and #4

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.

Even if our response to the reviewer sounded scientifically unacceptable for some reason, the speculation about the warming ocean impact on the ice bridge is written in the text as a pure suggestion. We rewrote this part as follows

> *"**In terms of Peabody Bay,** AW in front of **the nearby** Humboldt glacier **has warmed** by 0.9 °C since 1961 (Rignot et al., 2021**), although this trend is based on fairly sparse summer data in the area and is therefore highly uncertain**. However, based on a more consistent, **albeit** much shorter time series of mooring data obtained in Kennedy Channel in 2007-2009, Münchow et al. (2011) reported a statistically significant warming in the southward branch of AW of 0.027 °C/year. **There is also evidence that AW temperatures increased in the Lincoln Sea between 2003-2011 and the 1990s (de Steur et al., 2013). Through the mechanisms we have described here, warming AW in Nares Strait exerts a greater influence on ice growth and therefore the stability of the ice bridge. In fact, coincident to the warming of AW in and around Nares Strait has been a recent tendency for the duration of the ice bridge to become shorter, or for the bridge to not form at all, an event that has now happened during** six of the last 15 years (i.e., 2007, 2009, 2010, 2017, 2019 and 2022). **Continued warming of AW as a result of climate change will continue to affect the formation and stability of the ice bridge, and may lead to even more years when an ice bridge fails to form in Nares Strait.**"*

which sets a task for future studies:

> *"However, a quantitative estimate of the **changing** ocean thermal impact on bridge stability can be only accomplished with coupled ice-ocean models incorporating a comprehensive ice rheology or sea ice dynamics model (e.g. Plante et al., 2020; West et al., 2021), which is beyond the scope of this study."*

that will address factors that were not addressed in the current research.

In the following, the comments by the reviewer are in black characters and our responses to the comments are made in blue and indented. The comments where the reviewer was satisfied with our initial answers and/or changes were left out of this document for simplicity. Modifications to the text are shown in quotation marks with bold characters indicating newly added text, and normal characters indicating text that was already present in the previous version.

General comments:

This manuscript presents a scientifically interesting body of work, with great use of a variety of remote-sensing datasets and a modeling exercise together to make novel inferences about Nares Strait ice bridge structure and breakup. The authors have done a good job addressing reviewers' questions and concerns, and have produced a much clearer and polished revised manuscript. Assumptions and limitations of the results are much improved, and the storyline is a lot tighter. I am very impressed with the creativity in how these datasets were used and am looking forward to referencing it once published. I have a few comments and points of clarification (stated in the attached document) that should be addressed, but otherwise I recommend acceptance with minor revisions.

Specific comments:

121-125– Great addition so far. However, MODIS is thermal infrared not microwave, and brightness temperature is also impacted by cloud and atmospheric constituents/temperature.

> Thank you for finding this. We changed the sentence as followed:
> "*Note neither AMSR2 nor MODIS brightness temperatures are indicative of surface temperature alone, but measure the radiance of microwave* ***and mid-infrared*** *radiation that is expressed in units of temperature (K) of an equivalent blackbody.*"
>
> The fact that MODIS band 31 channel is influenced by cloud was mentioned earlier in Section 2.1.

444-450/Figure 10 – Apologies for the not-quite-complete comment on this during the first round of review. Your edits here improved this part of the discussion a lot despite a lack of complete info on what I was meaning. What I had meant to say is that when I looked at the timeseries of MODIS thermal images in WorldView, the image you show from 2019 seems like it may be a high wind event pushing ice away from the coast (maybe a latent heat polynya of sorts, which would not necessarily be indicative of high ocean temperatures and could cause high sea ice production) and not the best representation of the Tb for that month.
It's an extreme and ephemeral event for the month. A few days earlier and later in Dec 2019 looked very different. A similarly "warm-looking" event occurred in the 2018/2019 season for just a day or two. However, looking at all of the images from Dec 2019, I would agree that Tb was generally higher. A monthly mean Tb or timeseries of clear-sky days for the area would be much more useful in supporting your statements here for this reason. I don't think that a change is required for publication here, although it would make for a stronger statement and it could give you more insight about the relationship between sea ice height and Tb, as well as understanding mechanisms impacting bridge formation and breakup. I also use this as a caution against using extreme synoptic event days (Dec 18, 2019) to represent a monthly/interannual difference. That said, in general this section of text is oversimplifying a lot about Tb and not accounting for the complex sea ice-ocean-atmosphere interactions that are taking place. These

statements are not quite accurate to what you can say from the figures you show without further explanation.

> We generally agree with the reviewer that monthly mean MODIS T_b would be more beneficial for showing a "significant interannual variability". However, in addition to a clear-sky limitation (there are certain difficulties of determining clear sky over the entire Kane Basin during polar night), the presence of mobile ice in the channel make this task impossible. Because what we wanted to demonstrate was a difference of temperatures <u>within individual leads or openings</u> (not the mean temperatures over a large area) in the different parts of the region. If higher T_b are associated with open water at freezing point within leads opened by winds or currents, we would have seen approximately the same temperatures in the leads everywhere.
> To address this concern, we suggested the following changes in this paragraph:
>
> "*The MODIS brightness temperatures, Tb, shown in Fig. 10 generally support the idea that the thermal state of the surface* ***water*** *in Nares Strait* ***may vary considerably****. In December 2019 (Fig. 10b)****, for example****, the high Tb conditioned the ice-free (or covered with thin ice) area in the northwestern part of Peabody Bay and at the eastern side of Kennedy Channel.* ***The pattern of higher temperatures in this area resembles a plume that extends from the landfast ice edge towards the entrance of Kennedy Channel.*** *Although the signatures* ***of surface water with different temperatures*** *in Kennedy Channel can also be traced in the leads within the mobile sea ice in December 2018 and 2020, Tb was observed to generally be lower and may indicate reduced ocean heat transport towards the surface from below* ***during those years****. Of course, the* ***spatial*** *difference in temperatures* ***(within leads)*** *presented in these early winter snapshot images cannot explain the seasonally averaged elevation anomalies shown in Fig. 4. However, these differences demonstrate that there may be significant variability in the* ***sensible*** *heat flux* ***at the ocean surface around the time when sea ice begins to form****.*"

444-446 – "The high Tb conditioned the ice-free area in Peabody Bay", "signatures of warmer water in Kennedy Channel can also be traced through leads within the mobile sea ice" – this ends up being a little misleading for the reader. Since the temperatures you are showing aren't sea surface temperature and sit below saltwater freezing temperatures, it isn't clear what is causing the high Tb in 2019 or in the leads. The high Tb may merely be arising from a lack of sea ice at the surface and cold ocean surface temperatures (ocean is warmer than sea ice). The high Tb and lower sea ice concentrations could arise from low sea ice transport out of the Arctic and/or a windstorm blowing sea ice out of this area (which also could drive upwelling of warm water but doesn't necessarily need to do so to produce the Tb pattern you see). It may or may not also be from warmer ocean water coming to the surface, but I doubt that is happening in isolation from the other phenomena I mentioned. These comments also play into line 488-489.

> As we explained above it is a contrast of temperatures within individual leads in different parts of the research area that made us thinking that surface water temperatures vary spatially and "interannually". The leads formed by wind or currents alone would not have resulted in such differences because the temperatures would have been at freezing point everywhere. In December, when the sea ice has already started to form in the strait, it is the sensible heat flux from below that could explain higher surface water temperatures in some leads compared to others. Please see our suggested changes above.

447 - “may indicate reduced ocean heat transport towards the surface from below” – this is technically true with the use of “may”, but it would be helpful to present the other alternatives so that the argument is balanced for the reader.

Please see our response to the comments to Lines 444-450 and 444-446.

447-449 - “the difference in temperatures presented in these early winter snapshot images cannot explain the seasonally averaged elevation anomalies shown in Fig. 4” - The notion of high winds/low sea ice transport into the strait causing the high Tb can more easily explain why sea ice thicknesses aren’t consistent with the high Tb in 2019 than higher ocean heat flux to the surface can. For instance, more sea ice cover on the surface early in the season could insulate and prevent ocean heat escape that impacts sea ice formation afterwards. This could be discussed a little more in the text.

Please see our response to the comments to Lines 444-450 and 444-446.

605-608 – Since most of the iceberg discussion was removed from the paper, this no longer applies, correct?

No. The removed part was about a possible mechanism that causes the icebergs to form well-separated chains in the Peabody Bay. The idea that grounded icebergs affect the vertical mixing is self-evident, but without real measurements we put it here in a form of suggestion.

Line comments:

29 – “, which” makes the sentence a little clearer here

Changed as suggested

50 – not sure it makes sense to include “the last bridgeless winter” here since it is the most recent winter. I would cut this.

Removed as suggested

51 – points

Corrected

67 – spots based on evidence from nearby…?

Changed as follows:

*“**On the western side of Nares Strait, sensible heat polynyas form near the Bache Peninsula along** the eastern side of Ellesmere Island (Schledermann, 1980; Hannah et al., 2009) and seem to be highly biologically productive spots **given the presence of** prehistoric settlements **in the area** extending back 2500 to 3000 years (Schledermann, 1978).”*

85 – Cape

Corrected

103 – Spell out FESTOM-2 acronym here for first use, rather than on L170

Changed as suggested

125 - "did not"

Changed

130-131 – the segment lengths are in reference to ATL07 specifically and not all ICESat-2 data, so I recommend moving this sentence down one line to come after ATL07 is introduced.

It's a good point. Thank you for finding this out. The suggested changes were made.

134-137 – Have "although" and "however" in same sentence so recommend removing "Although"

Changed as suggested

140 – into "a" 1.5….

Corrected

145 – Compared to

Corrected

150 – Spell out for the first use of acronym (DMSP/SSM/I-ISSMIS), slashes are in the wrong spot at the end also.

We corrected the acronyms accordingly. However, we decided not to spell these acronyms out because they are related to the data used in the referenced studies, not in our paper.

161, 162, 166 – Spell out acronyms for first use?

We decided not to spell all these acronyms for the same reason.

166 – "a" finer

Corrected

178 – the model performs well in reproducing the shift…

Thank you for this suggestion. Changed as suggested.

181 – depth, which is in good…

Changed as suggested

196 – lower than ideal for resolving

Changed accordingly

198 – calculated using

Corrected

199-201 – what is your evidence for this statement?

To address the similar concern from the other reviewer, we performed a few 1D ice growth model experiments to show a sensitivity of ice thickness at the end of winter to the key forcing parameters (wind speed, air temperature, snowfall rate and ocean heat flux). The following text was added to Section 3.3:

"***To investigate a sensitivity of model results to the uncertainties in forcing parameters and/or possible biases of ERA5 data in Peabody Bay, we conducted several model runs with biased air temperature, wind speed, snowfall rate and ocean heat flux for the experiment presented in Fig. 6b. It was found that increasing wind speeds by 1 m s-1 increased ice thickness at the end of the season by 4.02 cm, whereas increasing air temperature, snow accumulation rate and ocean heat flux by 1° C, 1 cm mo-1 and 1 W m-2 reduced ice thickness by 2.45, 7.41 and 3.36 cm, respectively. Using these numbers, we can estimate a relative input of each parameter to the final results. Using 1 m/s, 2 °C, 2 cm/mo and 5 W m-2 as the possible biases or uncertainties of the model forcing parameters and taking the impact of ocean heat flux as 100%, we may estimate the relative input of other factors as 88% (snow), 29% (air temp) and 24% (wind speed) that underlines a larger contributing effect of snow and sensible heat flux on the ice growth.***"

217 – presence of a sensible

Corrected

218 – remove 'sea surface'

Thanks for finding this remain. Removed.

220 – I think you don't want "the latter" here

"…both the later" stands for two last (out of 3) reasons mentioned in the sentence.

238 – central main seems redundant so maybe this should just be central

The "main" was removed

271 – corresponds

Corrected

278 – compared to, also if you have it, it would be good to give a quantitative order of magnitude estimate for each of the gradients

> Unfortunately, there is no data on sea level under the ice bridge that would allow us to do so. This is the reason why we wrote "*are thought to be small*". We could probably use the data on the mean winter sea level difference between Alert and Thule (1.59 m, about 680 km apart) to demonstrate the mean gradient of about 2.3 cm per 10 km. This number is considerably smaller than 10-20 cm cross-shore changes of the elevations over approximately the same distance. However, the real distribution of sea level gradients below the bridge is unknown for such speculations.

292 – spell out SAR

> Done

303-304 – we will roughly estimate … or even "will estimate"

> Changed as suggested

331 – to the 19….

> Corrected

336 – would need to reach 70

> Changed as suggested

338 – at some distance from the polynya

> Corrected

339-40 – is large enough () to keep it ice-free…

> Changed as suggested

341 – to the 0.26 m mode

> Corrected

341-343 – A set of model experiments showed that the maximum…corresponds to … and a snow accumulation rate…

> Changed as suggested

From L343 to the end – stopped noting grammatical errors, but quite a few still exist beyond this point

> We have done a thorough review of the manuscript for grammatical errors.

365 – spell out first use of AO – but since it is the only use could remove the acronym

We don't know why AO appeared there. Changed to the Arctic Ocean

383 – how much shorter? Could give ranges or the average for each

The sentence was changed as follows:

"*Although the average duration of the observed ice bridges in Nares Strait (**about 5 months,** between 20 January and 28 June; Vincent, 2019) is shorter than that predicted with the model **(>6 months**),…*"

Figure 10 – What is the vertical-ish line in (b) and (c)?

These lines correspond to the approximate positions of the landfast ice at the moment of each imagery. The figure caption was modified accordingly.

459 – what does "contrast of the latest" mean?

"The latest" was replaced by " $\tilde{h}$ "

462 – away from Cape Jackson

Changed, thank you.

491 – the "only" available source – this isn't accurate based on your second sentence

Following recommendations of the other reviewer, this part was considerably changed.

527 – "ice elevation anomalies"

Changed as suggested

530-532 – "by the fact of forming the chain of polynyas" to "by the fact that chains of polynyas for…"

Changed as suggested

532 – Fig 11 should probably be referenced the sentence before.

Changed as suggested