Leonard et al. tc-2020-352 author comments

20 June 2021

This document contains the point-by-point reply to the reviewer and editor comments for the Leonard et al. tc-2020-352 manuscript. Reviewer and editor comments are indicated in italics. We provide our responses to Reviewer 1, followed by our responses to Reviewer 2 and finally our responses to the Editor comments.

1 Responses to Major Comments from Reviewer 1

We thank Reviewer 1 for their comments on our submission as a Brief Communication within criteria (c): "to disseminate information and data on topical events of significant scientific and/or social interest". We provide the following responses to the points that they have raised.

The authors present a convincing correlation between storm events and fast ice breakout, indicating that it's probably a direct wind-driven (i.e., dynamical) breakout mechanism (and I agree that this is almost certainly the case) - however no alternative mechanisms are discussed. Other studies have indicated that fast ice may be weakened thermodynamically by basal melt (e.g., Arndt et al., 2020, also TC - however this study implied that summertime mode 3 water incursions were important, which is surely not a factor in the winter). I'm not so familiar with the structure of the water column in the Sound during winter, but is it possible that these wind and polynya events enhance vertical mixing - and if warm water (e.g., mCDW) exists on the shelf here, might its entrainment induce basal fast ice melt? And the lag apparent between some storm events and the breakout might also imply a thermodynamic connection (although I accept your explanation involving the land mask of the sea ice concentration data in probably correct). Looking at Pritchard et al 2012 ("Antarctic ice-sheet loss driven by basal melting of ice shelves"), I can see that there's likely no warm water here so my hypothesis is quite unconvincing - but a brief discussion around alternative mechanisms would be appreciated!

To address this comment (as well as a similar comment from Reviewer 2), we have added a Discussion section to the manuscript where in the first paragraph we briefly investigate potential break-out mechanisms (lines 134 – 146 in the revised manuscript). Regarding the possibility of wind and polynya events enhancing vertical mixing, we have added the following sentence:

"Furthermore, the entire water column in McMurdo Sound during winter is conditioned by supercooled water flowing out from the McMurdo Ice Shelf cavity (Leonard et al., 2006), resulting in it being nearly isothermal and very close to its freezing point (Lewis and Perkin, 1985; Mahoney et al., 2011), suggesting that any water upwelled from the opening of the MSP would not cause melting of the fast-ice cover."

2 Responses to Minor Comments from Reviewer 1

6: add "timing of" between "between" and "break-out"

Done.

14: Brett et al ref needs year.

Done.

18: add "stable" between "the" and "fast"

Done.

32: "activity" here is a little ambiguous. You mean sea ice production, right?

We have removed "enhanced MSP activity" and replaced it with "more frequent opening of the MSP".

35: Probably best to avoid starting a sentence with a number (2019).

We have added "The" at the start of this sentence.

41: The Fraser et al 2020 dataset gives 15 day composite maps, not 14 day.

Changed "14" to "15".

47: The "biased" in here implies that these studies didn't correctly account for the icescape change. Is this what you really mean - if so, for both studies?

Changed "biased" to "influenced".

58: Was this IW mode Sentinel-1 imagery? What resolution?. Also Hall and Riggs refs need years.

We have added a sentence (reference lines 62 – 64) giving more information on the Sentinel-1 imagery used. It reads "The SAR imagery used in this study were a combination of Extra Wide (EW) medium resolution imagery (40 m pixel size) and Interferometric Wide (IW) high resolution mode imagery (10 m pixel size resampled to 40 m pixel size for this study)." We have added the years to Hall and Riggs references.

59: "MSP event" is a little ambiguous. Do you mean a large polynya size event? Also here, I'm curious how an active polynya looks in ice surface temperature - presumably a warm temperature? Or is it masked because largely open water?

Yes, we mean a large polynya-size event. We have added the following definition (lines 59 – 60 in the revised manuscript): "(a "polynya event" being defined here as a polynya opening that impacts on the previously established fast-ice cover)." We have added more description to what we observed in the MODIS IST data during a polynya event (reference lines 65 – 74 of the revised manuscript).

65: "Manually identified events" is a little ambiguous. Events of what?

Changed to: "Manually-identified fast-ice break-out events"

74: "connected to" -> "associated with"

Changed "connected to" to "associated with".

75: "warm temperatures" - what temperature? I presume near-surface air temp?

Changed to: "warm near-surface air temperatures".

79:"are correlated to sea ice concentration" - ambiguous description. High SIC? Low SIC? And isn't "correlated with" better than "correlated to"?

Changed to: "correlated with low sea-ice concentration".

82: By "freeze-up" do you mean pack or fast ice?

Changed to: "... the months preceding the formation of a stable fast-ice cover in McMurdo Sound ...".

85: It first struck me as a little unusual to define a KWI without using wind data. What happens if a low pressure system occurs over the central Ross Sea - doesn't this also bring warm air and low pressure? Or is this the effect *you're trying to capture - and these pressure systems enhance the katabatics? A little more clarity here would be appreciated.*

As described more below in our response to Reviewer 2, we have renamed the KWI as MSI (Modified Storm Index) to reinforce that the southerly winds in McMurdo Sound result from an interaction of katabatic winds with synoptic-scale low pressure systems, as the reviewer has correctly inferred in their comment.

91: "break-out events" - do you mean fast or pack?

Changed to: "break-out of the fast ice."

117: Although a brief communication, the "big picture" could do with a little more expansion. E.g., this is one of few case studies on fast ice stability, an area where more research is needed, etc. It occurs to me that this region might be a good one for testing forthcoming fast ice tensile strength parameterisations in prognostic fast ice models (e.g., Lemieux et al., 2016, "Improving the simulation of landfast ice by combining tensile strength and a parameterization for grounded ridges"). Also, are there other regions you know of which have a similar fast ice regime (i.e., deep embayment and lack of grounded icebergs) to which the results of this study might be applicable?

We have moved the content of the Conclusions section in the original manuscript to a new Discussion section, and have inserted a new paragraph in the Conclusions (reference lines 164 – 171 in the revised manuscript) where we place this study into the wider context of fast-ice research.

119: The Fraser et al., 2020 dataset is missing from the availability section.

We have added the following statement in the data availability section (reference lines 174 – 176 in the revised manuscript): "Circum-Antarctic landfast sea ice extent data accessed from the Australian Antarctic Data Centre at https://data.aad.gov.au/metadata/records/AAS 4116Fraserfasticecircumantarctic."

Fig 1: It would be helpful to please annotate the area of active polynya in each SAR image (manually is fine). Similarly for the fast ice edge.

Active polynya areas in Figure 1 are now shown outlined in orange and magenta lines now show fast-ice edges. These have both been manually picked. We have also added the 77.6° S parallel as a green dashed line and added a label for Cape Royds. We have had to remove the symbols and labels for Mt. Erebus and Mt. Terror to accomplish this.

Fig 2: Does the truncation of the upper half of each wind rose remove any/much information? I'd quite like to see the whole thing (if there's detail in the northerly half) but happy to stick with the half roses if no wind from that half.

The winds in the upper half of each wind rose were not particularly informative, as the northeasterly winds are prevailing and of lower wind speeds, meaning they overwhelmed the wind roses but did not contribute much. However, we have revised Figure 2 to include the full wind roses in order to give the reader the entire picture, and be consistent with Figure 3, which now shows all wind speeds and directions for 2019. We have also added the 90$^{\text{th}}$ percentile and above winds for the periods 1997 – 2018 (column three) and 2019 (column four) to further illustrate the extreme nature of the 2019 season. We have replaced the original continuous colour bar with a discrete colour bar in this figure.

3 Responses to Major Comments from Reviewer 2

We thank Reviewer 2 for their comments on our submission as a Brief Communication within criteria (c): "to disseminate information and data on topical events of significant scientific and/or social interest". We provide the following responses to the points that they have raised.

The 2019 anomalous breaking of fast ice appears to be associated with KWI and sea ice concentration, as shown in Figure 3. However, this manuscript did not explain the mechanism of fast ice breaking.

The authors used KWI and southerly winds. I straightforwardly regarded these as due to katabatic wind. What *mechanism do the authors consider for the fast ice break up by the katabatic wind? Figure 3 showed that the KWI increase coincides with the fast ice break up. When the KWI was large, strong winds were blowing from the south (continent). Again, how does the fast ice is broken by this wind? It is widely known that sea swells affect the breaking of fast ice. The swell effect was also discussed in Banwell et al. (2017), cited by the authors. It seems hard to destroy fast ice only by katabatic wind, even if it is a strong wind. Furthermore, since the wind blows from the shore to the offshore, it is expected not to generate swells that destroy the fast ice.*

We have addressed this comment in the following ways:

(1) We have renamed the Katabatic Wind Index (KWI) to the Modified Storm Index (MSI) and provided a more in-depth description of how katabatic winds and synoptic-low pressure systems interact to produce southerly wind events in McMurdo Sound (reference lines 85 – 90 in the revised manuscript).

(2) We have referenced the work of Dale et al. (2017) and Ebner et al. (2013) to present observed linkages between offshore winds with both katabatic and synoptic components and an increase in polynya area due to wind forcing (reference lines 90 – 96 in the revised manuscript).

(3) We have added a Discussion section where in the first paragraph we briefly investigate potential break-out mechanisms (lines 134 – 146 in the revised manuscript). Regarding the comment of whether sea swell could be a potential break-out mechanism, we have added the following sentence: "Finally, we would not anticipate wave action playing a significant role in breaking up the fast-ice cover due to the absence of upstream fetch associated with the southerly winds." (reference lines 145 – 146 in the revised manuscript).

Since the katabatic wind is a strong wind from inside the continent, it is expected that the air temperature will drop during the period when the KWI is large. However, as is clear from Figure 3, the temperature rose when the KWI is large. Please explain the reason for this. Is this because of the breaking of fast ice or a coastal polynya formation? Both of them will increase heat flux from the ocean to the atmosphere.

We have provided an explanation for how interaction between katabatic winds and synoptic-scale low pressure systems can produce strong southerly surface winds that are warmer than the ambient surface air temperature (reference lines 85 – 90 in the revised manuscript).

Regarding the fast ice break up during June-July: The reviewer cannot know the details because the authors only show the southerly wind component, but wondering the influence of low pressure rather than the katabatic wind from the following facts: the wind speed increased, the temperature rose, and the atmospheric pressure decreased (Fig. 3). If so, the reviewer considers that fast ice could be collapsed by sea swell. The authors also described it as a "storm event" in their conclusion (P. 4, L. 108). Is this an atmospheric event due to the katabatic wind only? Otherwise, is it the effect of a low-pressure system? Please clarify this.

We have revised Figure 2 to include all wind directions in the wind roses and have added two new columns that show the 90$^{\text{th}}$ percentile and above winds for 1997 – 2018 and 2019. As mentioned above, we have referenced the work of Dale et al. (2017) and Ebner et al. (2013) to present observed linkages between offshore winds with both katabatic and synoptic components and the increase in polynya area due to wind forcing (reference lines 90 – 96 in the revised manuscript) to support our premise that fast-ice break-out is due to opening of the McMurdo Sound Polynya as a result of wind forcing. We have provided an explanation for how storm events result from interactions between katabatic winds and synoptic-scale low pressure systems (reference lines 85 – 90 in the revised manuscript). We have also interrogated sea-level data from Cape Roberts (5 minute resolution), which showed no indication of anomalous sea level at the times of break-out events.

This study showed a relationship between a coastal polynya and KWI (section 4). By what mechanism does the polynya cause the fast ice break up? Is it just a description of a relationship between KWI (southern wind) and polynya? The air temperature was below -10 degrees Celsius during the period. Under such atmospheric conditions, even if an open water fraction appears by the divergent ice motion due to prevailing wind, the ocean surface will be immediately covered with thin sea ice. In winter, coastal polynyas should be considered as thin ice-covered areas with high ice concentration rather than low ice concentration areas under the passive microwave sensor's coarse spatial resolution. Many sea ice concentration algorithms used for passive microwave satellite data underestimate the concentration in thin ice-covered areas. It may be possible to regard the low ice concentration region as a coastal polynya signal due to this characteristic, but caution will be required. It does not detect coastal polynyas precisely. For the detection of coastal polynyas from passive microwave satellite data, Tamura et al. (2007; 2008) and Nihashi and Ohshima (2015) would be helpful.

We have addressed the comment regarding the break-up mechanism above. We thank Reviewer 2 for highlighting the challenges in using passive microwave satellite data to identify polynya area, and for providing three references that provide information on the development of microwave thin ice algorithms and their application to polynya detection. We have added two sentences in Section 2.1 that acknowledge the challenges in detecting coastal polynyas from passive microwave satellite data and provide reasoning for introducing a sea-ice fraction metric derived from ARTIST sea ice concentrations to characterise regional changes in the sea-ice cover, as opposed to attempting to discern polynya area from passive microwave satellite data (references lines 77 – 80 in the revised manuscript).

4 Responses to Minor Comments from Reviewer 2

Reviewer 2 did not provide any minor comments in their review.

5 Responses to Major Comments from the Editor

We thank the editor for their comments on our submission as a Brief Communication within criteria (c): "to disseminate information and data on topical events of significant scientific and/or social interest". We provide the following responses to the points that they have raised.

My main concern with your ms and the response to the reviews focusses on differentiating the temperature effects (aka warming) and the forcing from air-pressure and wind-stress changes. I believe that the katabatic you refer to is cool gravity wind, while the Foehn is warm wind. The underlying difference is that the katabatic is a surface wind, while the Foehn wind arises from processes much higher in the atmosphere (Speirs et al., 2010). With this in mind, I invite you to review the definition of the Katabatic Wind Index

We thank the editor for their comment regarding katabatic winds and their invitation to review the definition of the Katabatic Wind Index. As stated above, we have renamed the Katabatic Wind Index (KWI) to the Modified Storm Index (MSI) and provided a more in-depth description of how katabatic winds and synoptic-low pressure systems interact to produce southerly wind events in McMurdo Sound (reference lines 85 – 90 in the revised manuscript). The revised name references directly the origin of the storm index introduced by Brunt et al. (2006), where "modified" refers to adaptations we made to apply the index to discrete events.

The data/images presented do not provide the reader with sufficient information to judge how anomalous the 2019 fast-ice break-out rates in the overall record.

We acknowledge that it was not possible to infer the anomalous wind conditions in 2019 from Figure 2 in the original manuscript. To address this, we have produced a revised Figure 2 that now shows the surface-wind speeds from all directions and the 90th percentile and above surface winds for the years 1997 – 2018 and 2019, respectively. By comparing the 90th percentile wind roses in June and July for the periods 1997 – 2018 and 2019, we believe Figure 2 now clearly shows that the frequency of intense (e.g., 90th percentile and above) southerly winds in June and July 2019 was greater than for the period 1997 – 2018.

We acknowledge that the ARTIST record of sea-ice concentrations is relatively short (2013 – 2019) as compared to the data records for wind speed, surface air temperature and MSLP. We chose not to use more spatially-coarse remotely-sensed sea-ice concentration data due to the fact that the larger footprints do not sufficiently resolve the sea-ice conditions in McMurdo Sound. This has meant that we have a shorter data record to assess winter 2019 break-out events against (Figure 3a) as we do for MSI identified storm events (Figure 3b).

Accepting that some years are influenced by the presence of a large iceberg, the authors should provide a longer record, i.e., to earlier years.

We agree that the number of iceberg-affected years in the data record has affected our ability to construct a longer time record of fast-ice conditions in McMurdo Sound. The two remote-sensing derived sea ice products we have used in this study unfortunately do not extend farther back than 2000 for the Fraser et al. fast ice extent dataset and 2012 for the Spreen et al. (2008) dataset, and the meteorological dataset is only available back to 1996. As our goal with this brief communication is to report on a topical event of scientific interest, we felt that investigating ways to extend these records farther back in time was beyond the scope of the study.

How do the thermodynamic and the dynamic components of the KWI relate to each other? And which one is crucial in driving the ice breakout? Is this consistent over the full data record?

As mentioned above, we have provided an explanation for how interaction between katabatic winds and synoptic-scale low pressure systems can produce strong southerly surface winds that are warmer than the ambient surface air temperature (reference lines 85 – 90 in the revised manuscript). Also as mentioned above, we have added a Discussion section where in the first paragraph we briefly investigate potential break-out mechanisms (lines 134 – 146 in the revised manuscript), and we have referenced the work of Dale et al. (2017) and Ebner et al. (2013) to present observed linkages between offshore winds with both katabatic and synoptic components and the increase in polynya area due to wind forcing (reference lines 90 – 96 in the revised manuscript). On the balance of this, we put forth that it is wind-forcing that is the crucial driver of ice break-out, but we are unable to quantify other effects such as sea-ice internal temperature, sea swell and ocean current / temperature.

What are the elevations for the temp and pressure sensors at Scott Base? And over which surface type are deployed? Glacial ice, rock, ..

We have added that the elevation of the wind, pressure and temperature sensors at Scott Base is 20 m (reference line 106 in the revised manuscript). The sensors are located in the immediate vicinity of Scott Base and the surface type is rock.

One would expect a clearer katabatic signal of the KWI would be a function of the pressure gradient, and even more if also considering a temperature gradient (normalized relative to freezing temperature).

We have renamed the KWI as MSI (Modified Storm Index) to reinforce that the southerly winds in McMurdo Sound result from an interaction of katabatic winds with synoptic-scale low pressure systems.

As KWI is the product of Tair times Pair, how do the authors relate this to a mechanism that leads to the breakup of fast ice in McMurdo Sound?

The MSI (renamed from KWI in the original manuscript) is constructed from the product of normalised positive temperature and negative pressure anomalies, following what was developed by Brunt et al. (2006) to represent conditions of "storminess" that most consistently led to abrupt break-up, mobilization and clear-out of fast ice in the southwest Ross Sea. Following this definition, and as stated above, we then relate the MSI to the opening of the McMurdo Sound Polynya, which results in the breaking-up of regions of the fast-ice cover.

Also, was any possibly interference with tidal effects considered? If peak tidal elevation would have coincided with the onset of the MSLP gradient, then a plausible mechanism for the fast-ice breakout would have been identified.

We had investigated this, as noted above, and found no evidence of tidal-induced break-out. We have addressed this comment by adding the following text (reference line 141 in the revised manuscript) “..., and we did not observe any correlation between tidal state and the 2019 break-out events in this study.”

6 Responses to Minor Comments from the Editor

Decide on hyphenation rule and apply consistently throughout the manuscript. For example "fast-ice cover" (line 113) versus "fast ice cover" (line 111). Pls apply this rule to all three (or more) noun sequences. Typical recommendation is to hyphenate the first two nouns to establish the context.

We have made these changes throughout the entire revised manuscript.

Why introduce terminology such as "sea ice fractional cover" and "sea-ice fraction" when there is a standard, namely "sea-ice concentration"? Suggest to replace with the latter throughout the manuscript.

We have provided a reason for this approach as (reference lines 123 – 125 in the revised manuscript): “This method was chosen in preference to computing an average sea ice concentration as it is better suited to quantifying changes in sea ice extent, which we equate generally to changes in the fast-ice cover in the sound."

The "° N/E/S/W" is used inconsistently... with regard to the spacings. Please climatological mean"?

We have standardised the spacing for ° N/E/S/W throughout the revised manuscript. The term "climatological mean" is applied to surface air temperatures and MSLP and is the mean value of the respective quantities over the time period 1997 – 2018, smoothed by a 3-day window.

What are the elevations for the temp and pressure sensors at Scott Base? And over rectify.

The elevations are 20 m. This information has been added in line 106 of the revised manuscript.

Correct "e.g." to "e.g.," throughout the manuscript.

Done.

51: Change "Sound" to lower case.

Done.

52: Change "Sound" to lower case.

Done.

58: Add publication year to "Hall and Riggs, a, b)".

Done.

67: Change "Sound" to lower case.

Done.

86: Definition of the KWI: Clarify how the hourly air temperature and mean sea level pressure data/anomalies are used relative to the "climatological mean". I.e., what is the frequency of the "climatological mean"

The climatological mean is calculated from hourly data that has been smoothed by a running 3-day window for the period 1997 – 2018 for temperature, and 2002 – 2018 for MSLP, respectively . The anomalies are the differences between the hourly 2019 data and the hourly 3-day smoothed climatological means.

118: Add publication year to "Brett et al."

Done.

119: Correct "data is" to "data are". TWICE in this line.

Done for the first one. The second one was removed as we no longer include Linda AWS data in the revised manuscript.

123: No need to provide detail on which exact data sets were processed/analyzed by whom. Shorten to "GHL had the initial idea for this study and analysed the satellite imagery/products, KET".

Done.

Fig. 1:

Add "the" to read "inset shows the location".

Replace "from" with "for" to read "for which the sea ice fractions".

b), c) and d): Define acronyms shown: "MSP" and "RSP".

b) - f): Add year to dating provided.

All done.

Fig. 2: Need to add information to place the single winter 2019 into the winter mean for 1998 - 2018. Either add standard deviations are add data/images for all other individual winters. – Lack of this information presents a crucial omission and does not allow the reader to assess the severity of the atmospheric forcing on the fast ice.

We thank the editor for this very important comment, and we have addressed this by adding the 90$^{\text{th}}$ percentile and above winds for the periods 1997 – 2018 (column three) and 2019 (column four) so that the reader can now assess the severity of the 2019 atmospheric forcing on the fast ice.

*Fig. 3: * b) Do the Scott Base data indicate a critical threshold for ice breakout? * c) Data from Linda and Scott Base are clearly shown in this sub figure.*

We do not believe that a critical threshold for ice break-out can be inferred from the Scott Base atmospheric conditions alone as the thickness / strength of the fast-ice cover makes too significant of a contribution.

We have removed Linda AWS data from Figure 3c, and replaced the combined Scott Base and Linda wind speeds (original panel c) with Scott Base wind speeds (new panel c) and directions (new panel d). We have used red dots to identify the wind speeds and directions for 90$^{\text{th}}$ percentile and above wind events. In the new panel c, we have also denoted the monthly 90$^{\text{th}}$ percentile wind speeds for the period 1997–2018 by a red line.